NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS

VOLUME XVIII

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 27, 1905.

Sir William Crookes, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Edward A. Wilson, Esq., M.B. F.Z.S., Naturalist on board the "Discovery" in the British Antarctic Expedition, 1901–4.

Life History of the Emperor Penguin.

[Abstract.]

The Emperor is the largest of all the Penguins, and is limited strictly to the ice-covered regions of the Antarctic. The interest of its life-history lies chiefly in the fact that its breeding ground was first discovered during the recent expedition made by the "Discovery" into the Antarctic. Its young and its eggs were brought home for the first time when the "Discovery" returned to England in September 1904.

In reviewing the life of this bird, the difficulties of investigating its breeding habits were explained as the result of certain peculiarities: for example, that of laying the eggs in the middle of the winter darkness; each hen laying a single large egg, which it incubates as it stands in an upright position on sea-ice, keeping the egg from contact with the actual ice by holding it on the *dorsum* of the foot, and allowing a heavily-feathered fold of skin to fall over it from the abdomen, thus completely obscuring it from view, and keeping it closely appressed to the abdomen, warm enough to hatch out, probably in some seven weeks. In the coldest month of the whole year, viz. August, the chicken is hatched out, and becomes the unwilling recipient of so much attention from its parents, and from such other adults as have no young of their own to attend to, that upwards of 77 per cent. die, and may be picked up frozen on the sea-ice, within the first month or two of their existence. This high death-rate is in a large measure the result of the quarrels of adult birds for possession of a chicken, all having an overpowering desire to brood over something. In many cases the desire leads to brooding over dead chicks till they are actually rotten.

Much was said of the trials that must be endured by the naturalist who wishes to see this bird in its breeding haunts. He must be ready to encounter the lowest temperatures hitherto recorded, under canvas, sleeping three in a bag for what warmth can be procured at 40°, 50° and 60° below zero Fahrenheit, and for a fortnight or three weeks at a stretch. Much, also, was said of the various sledge expeditions undertaken, after its first discovery by Engineer-Lieutenant Skelton, R.N., for the purpose of fully investigating the Emperor Penguin rookery at Cape Crozier; of the discovery of the first egg on the sea-ice by Lance-Corporal Blissett, R.M.L.I., and of the exceptional circumstances which, in the following year, enabled the lecturer to bring back to the ship a series of some fourteen eggs and several dozen of the young.

Examples were shown at the close of the lecture, which was further illustrated by a series of lantern slides, made from photographs taken mainly by Mr. Skelton and from drawings by the lecturer of the various stages in growth of the Emperor Penguin, from infancy to old age.

[E. A. W.]

WEEKLY EVENING MEETING,

Friday, February 3, 1905.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

PROFESSOR T. CLIFFORD ALLBUTT, M.A. M.D. LL.D. D.Sc. F.R.S.

Blood Pressure in Man.

THE lecturer began by contrasting Galen's conception of the oscillation of the blood, about the liver as a centre, with the cardiac circulation of Harvey. The pulmonary circulation—for the purposes of this lecture—was omitted, and attention directed exclusively to that in the systemic arteries.

The physical characters of the flow of fluids were briefly described by the example of water in an open stream. A stream might well up from a spring in a flat country, and swim with very low pressure to its mouth ; or, falling from a mountain, might have pressure enough to carry men and horses off their legs. If the volume were also great, as in the sea, it might exercise a pressure of many tons to the square yard, and smash great bulwarks to pieces. But in the higher animals the blood flows in closed channels, so that in such a scheme as theirs the dimensions of the channels assume a very important value. Moreover, in mammalia the circulating fluid is not water, but a thicker fluid—the blood—which (in man) has at least four times the viscosity of water. The enormous value of friction in the circulation was then considered, and it was shown that in this factor the kind of vessel wall does not signify much, as the wall is lined by a practically stationary layer of the fluid ; friction, therefore, which uses up $\frac{99}{100}$ of the heart's power, depends on the factor of viscosity together with that of the dimension of the channels, or closed bed. It may be said that the blood pressures—that is, the arterial pressures—in man depend on viscosity and dimension of stream bed.

Now so far the closed tubes had been regarded as rigid. But if in animals the tubes were rigid, the circulation would be carried on under great difficulties. For instance, there would be no accommodation ; only so much blood could be driven into the system as issued at the periphery ; the stream, too, would be quite intermittent, with very high maximum and very low minimum pressures, which would not serve for continuous nutrition, and by its extremes of pressures would soon wear down the arteries. For instance, in the bagpipes, were it not for the air reservoir the sound would issue in spasmodic screams ;

whereas the air-bag turns the intermittent blowing into a continuous feed of air. In the arterial system of man the same provision is made; its tubing is highly elastic, and a chief part of it—namely, the aorta—being relatively wider than other branches of the tree, contains, like the bagpipe reservoir, accommodation for very variable supplies of output from the heart pump. Thus a very large part of the heart power is used in dilatation of the vessels, and by these is given back to the blood. The valves of the heart serve a like purpose of regulating the pressure of the supply to the vascular system.

The lecturer in the next place dealt with the pulse, contrasting the travel of the wave with the travel of the blood itself. The wave due to the shock of the heart beat travels, ordinarily, about twenty times as fast as a given particle of the blood itself. The tenser the walls of the arteries the faster the wave travels along the taut vessels, but the slower the passage of the blood itself. Herein lies one of the chief evils of a morbid rise of arterial pressure; more stress on the vessels, less distribution of their contents. Many of these processes were illustrated by lantern slides and demonstrations by Dr. Dixon, demonstrator of pharmacology in Cambridge.

After these principles Dr. Dixon exhibited the various instruments in use for measuring blood pressures in man, and the means by which their curves may be recorded on a revolving drum (kymograph).

The lecturer then entered upon the vital properties of the arteries—that they are not only elastic, and so accommodate themselves to the varying pressures, but are endowed also with nervous governance, whereby they effect a large economy in work and material. Several functions of the human body cannot, save within small limits, work together. If we are digesting, we are not apt for thought; the Alpine climber is mercifully unable to worry over affairs—his mind is put into abeyance, and so on. Thus the arterial system by the means of its nervous connections, contracting in some areas and dilating in others, automatically diverts its fertilising streams hither or thither as needs arise. Moreover, it can enlarge or diminish its bed according to the total quantities of blood temporarily in active circulation—a quantity which is very variable. By contracting the arteries in considerable areas and correspondingly dilating them in others, the fields of the various functions of the body can be used alternately, as we see in the irrigation of Alpine meadows. By the same means the very various pressures of the blood can be counteracted. When under muscular effort, for instance, the pressure is raised, a corresponding area outside the muscles is dilated, and pressure more or less equalised; thus the heart is enabled to do the most work with the least disturbance of stresses. So in a bath, cold or very hot, the crimping up of the large cutaneous areas is compensated by large dilatations in internal areas, and pressures return to the normal in two or three minutes. The chief area in which blood can be accommodated, and thus for a time put out of circulation, is a large abdominal area.

By these considerations the lecturer was led to explain why the blood in the body does not drop down into our feet and legs, and leave the brain and other vital parts. Indeed, the blood has a strong disposition thus to obey the action of gravitation, and one of the events of approaching death is the falling of the blood into lower parts of the body, deserting the heart and brain. Obviously this is especially the case in upright animals, as in man chiefly, and in apes in some measure. It is by the vigilance of the nervous governance that the blood is held up, by the contraction of the abdominal vascular fields; and it is the failure of these mechanisms which appears as shock, syncope, or collapse. The lecturer, assisted by demonstrations by Dr. Dixon, illustrated these dispositions, citing especially the researches of Prof. Leonard Hill on the distribution of the blood in various positions of the body. He also referred to the bearing of these principles on the researches of Prof. Waller and others on the dangers of anæsthetics. By some most interesting experiments by Dr. Cushing he showed how enormously the arterial pressures may be raised in case of danger of failure of supply of blood against gravity when, as in apoplexy or a depressed fracture of the skull, the blood-vessels, in the parts of the brain where all these mechanisms find their centres, are compressed and thus more or less liable to be emptied.

In the last part of the lecture the lecturer apologised for occupying time with so much physiology, in which subject he is not an investigator. But it was necessary to make manifest to his audience how great is the importance of the integrity of the arteries themselves, and of their nervous governance in function, an integrity which is a matter of life and death; for if the circulation fails in the nervous centres or heart, life must cease. Now the arteries are subject to many injurious conditions, as of certain poisons and infections, or of hard muscular labour; there are also the unexplained deteriorations of age. His personal investigations had been into the effects on the arteries of gradual increases of blood pressure. Normally, arterial pressures, as taken in the arm, rise somewhat from childhood to age —say from 80–90 mm. Hg. to 140° or perhaps 150°. These upper limits are not inconsistent with health at the age of three score, though no doubt they signify some loss of mechanical efficiency. A demonstration was given by Dr. Dixon of the difference in vascular efficiency under muscular effort between a young and an elderly man. Into the notable effect of certain poisons and infections on the arteries he could not enter. Senile degenerations of the arteries are not essentially allied to rise of blood pressure, though in such subjects, as in others, high pressures may arise, and must be, of course, the more dangerous. Still, senile arterial degeneration is compatible with very long life, even if with diminution of function, as the vessels close or silt up rather than burst.

The lecturer's own observations, now extended over many years,

had been upon rise of pressure in middle life beyond, often very far beyond, that which he had mentioned as normal for elderly persons. The reasons of this morbid tendency cannot yet be given, but fortunately, by medicinal and dietetic means, it can be abated, and in early stages abolished. If permitted to persist, and it is not rarely consistent with fair general health or but vague indisposition, it slowly ruins the vascular system by overstretching it. It is in such persons that the arteries may break, as in apoplexy, a catastrophe which, by timely precautions, can be prevented. The lecturer strongly urged upon all persons of middle and advancing years to have their arterial pressures tested by their physicians every four or five years, so that any disposition to excessive pressures may be averted and the integrity of the arterial tree preserved.

[T. C. A.]

GENERAL MONTHLY MEETING.

Monday, February 6, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Lieut.-Col. Henry Edward Gaulter,
Edwin Percy Harvey, Esq.
Mrs. Ludwig Mond,
Dr. Tcherniac,
Evelyn C. B. Wilbraham, Esq., Ph.D.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Linguistic Survey of India: Vol. II.; Vol. III. Part 3; Vol. VI. 4to. 1904.

Archæological Survey—

Annual Report, Panjab Circle, 1904. 4to.

Progress Report of the Archæological Survey of Western India for 1903-4. 4to. 1904.

Annual Progress Report of the Archæological Survey Circle, United Provinces. 4to. 1904.

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Rendiconti, 1904, Vol. II. 4to.

Amalgamated Press, Limited—Daily Mail Year Book, 1905. 8vo. 1904.

American Academy of Arts and Sciences—Proceedings, Vol. XL. Nos. 8–10. 8vo. 1904.

Memoirs, Vol. XIII. No. 2. 4to. 1904.

American Geographical Society—Bulletin, Vol. XXXVI. Nos. 11–12. 8vo. 1904.

American Philosophical Society—Proceedings, Vol. XLIII. No. 177. 8vo. 1904.

Amsterdam, Royal Society of Zoology—Aflevering 17–18. 4to. 1893–1904.

Amsterdam University, Vereenigen Secties voor Wetenschappelijken Arbeid—Réactions Phagocytaires. By E. Metchnikoff. 8vo. 1904.

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Automobile Club—Journal for Dec. 1904 and Jan. 1905. 4to.

Bankers, Institute of—Journal, Vol. XXV. Part 9; Vol. XXVI. Part 1. 8vo. 1904–5.

Basel, Naturforschenden Gesellschaft—Verhandlungen, Band XVII. 8vo. 1904.

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British Astronomical Association—Journal, Vol. XV. Nos. 2–3. 8vo. 1904.

Buenos Ayres—Monthly Bulletin of Municipal Statistics for Sept.–Nov. 1904. 4to.

California, University of—Bulletin, Vol. V. No. 3; Vol. VI. No. 1. 8vo. 1904.

Canada, Geological Survey—Catalogue of Canadian Birds, Part III. 8vo. 1904.

Chemical Industry, Society of—Journal, Vol. XXIII. Nos. 23–24; Vol. XXIV. Nos. 1–2. 8vo. 1904.

Chemical Society—Journal for Dec. 1904 and Jan. 1905. 8vo.

Proceedings, Vol. XX. Nos. 286–288. 8vo. 1904.

Chicago, University of—Publications of the Yerkes Observatory, Vol. II. 1903. 4to. 1904.

City and Guilds of London Institute—Programmes of the City and Guilds Technical Colleges. 8vo. 1904.

Civil Engineers, Institution of—Proceedings, Vol. CLVIII. 8vo. 1904.

Subject Index, Vols. CLV.–CLVIII. 8vo. 1904.

Cornu, Madame A.—Œuvres Diverses de Monsieur A. Cornu. 5 vols. 8vo and 4to. 1864–1901.

Dax, Societé de Borda—Bulletin, 1904, Fasc. 2. 8vo. 1904.

Devonshire Association—Report and Transactions, Vol. XXXVI. 8vo. 1904.

Calendar of Devonshire Wills, Part VI. 8vo. 1904.

Dewar, Sir James, M.R.I. M.A. LL.D. F.R.S.—First Report of the Wellcome Research Laboratories, Khartoum. By A. Balfour. 4to. 1904.

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Athenæum for Dec. 1904 and Jan. 1905. 4to.

Author for Jan.–Feb. 1905. 8vo.

Board of Trade Journal for Dec. 1904 and Jan. 1905. 8vo.

Brewers' Journal for Dec. 1904 and Jan. 1905. 8vo.

Chemical News for Dec. 1904 and Jan. 1905. 8vo.

Chemist and Druggist for Dec. 1904 and Jan. 1905. 8vo.

Editors—continued.
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Electrical Engineer for Dec. 1904 and Jan. 1905. fol.
Electrical Review for Dec. 1904 and Jan. 1905. fol.
Electrical Times for Dec. 1904 and Jan. 1905. 4to.
Electricity for Dec. 1904 and Jan. 1905. 8vo.
Engineer for Dec. 1904 and Jan. 1905. fol.
Engineering for Dec. 1904 and Jan. 1905. fol.
Engineering Review for Dec. 1904 and Jan. 1905. 8vo.
Homœopathic Review for Jan.–Feb. 1905. 8vo.
Horological Journal for Jan.–Feb. 1905. 8vo.
Journal of the British Dental Association for Dec. 1904 and Jan. 1905. 8vo.
Journal of Physical Chemistry for Dec. 1904 and Jan. 1905. 8vo.
Journal of State Medicine for Dec. 1904 and Jan. 1905. 8vo.
Law Journal for Dec. 1904 and Jan. 1905. 8vo.
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Machinery Market for Dec. 1904 and Jan. 1905. 8vo.
Model Engineer for Dec. 1904 and Jan. 1905. 8vo.
Mois Scientifique for Nov. 1904. 8vo.
Motor Car Journal for Dec. 1904 and Jan. 1905. 8vo.
Motor Car World for Dec. 1904 and Jan. 1905. 8vo.
Musical Times for Dec. 1904 and Jan. 1905. 8vo.
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Nuovo Cimento for Oct.–Dec. 1904. 8vo.
Page's Weekly for Dec. 1904 and Jan. 1905. 8vo.
Photographic News for Dec. 1904 and Jan. 1905. 8vo.
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Public Health Engineer for Dec. 1904 and Jan. 1905. 8vo.
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Odontological Society—Transactions, Vol. XXXVII. No. 2. 8vo. 1904.
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WEEKLY EVENING MEETING.

Friday, February 10, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D., President, in the Chair.

CECIL SMITH, Esq., LL.D., Keeper of Greek and Roman Antiquities, British Museum.

The Art of the Ionian Greeks.

[No Abstract.]

WEEKLY EVENING MEETING.

Friday, February 17, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

John W. Gordon, Esq., *M.R.I.*

High Power Microscopy.

In the exhibition of a microscopic object under high magnifying power, there are three stages at which difficulties have to be encountered and surmounted.

1. In the preparation of the object for exhibition under suitable conditions of illumination.
2. In the representation of the object by means of an image.
3. In the transmission of the image so formed in the instrument to the eye of the observer.

Dealing first with the preparation of the object. Professor Wright has suggested a classification from this point of view according to which microscopic pictures fall into two classes, which, adopting a nomenclature employed by Professor Koch, he calls colour pictures and outline pictures. A colour picture, as its name suggests, is usually the result of a stain, but its specific character does not depend upon its tint. The distinctive property of a colour picture is that the structure is shown by masses or washes of colour without delineation, whereas in the outline picture the contours are delineated and the masses have the same tone as the background. Fig. 1—a photograph of a piece of lung tissue—is an example of a colour picture. Fig. 2—a photograph of four strands of gossamer—affords an illustration of an outline picture. The colour picture possesses many advantages, especially where measurements are in question, for the single boundary which is the common outline both of the object and of the background, is more easily identified than the more or less nebulous line by means of which an outline picture is delineated. But with a large number of objects the staining method fails altogether, and it is necessary therefore to have recourse to the alternative type of picture. This depends for its formation upon difference of refractive index between the object and the background. The theory is perfectly well known, but may be usefully brought to mind by a simple illustration such as is afforded by Fig. 3. A very close and easily intelligible analogy to the theory of refraction may be found if we assume first that Fig. 3 illustrates

the march of a company of soldiers across a field, part of which—the darkened portion—is covered with grass and affords good foothold, while the light part represents ice across which it is possible to march only with a shortened step. Now assume that the men receive directions to march shoulder to shoulder and straight ahead. The first step will carry the line forward in unbroken formation parallel with itself into the position shown by the second line of men in the diagram, but upon the second step the man of the first file on the left will step short since he steps upon ice. In order to observe the shoulder to shoulder rule he must advance his right shoulder, and for the same reason his right hand man must retire his left shoulder. Upon the third step both the first and the second file men will step short, keeping step with one another, and the second man will have to advance his right shoulder to keep touch with his right hand man, thus completing the half-turn which he commenced by retiring his left shoulder when his left hand man fell behind. At the next step the third man will execute the same evolution, and so, gradually, a new line will form itself upon the ice, breaking off at a definite angle from the line of the original formation. When the farther edge of the ice is reached all these evolutions will be repeated in inverse order, with the result, shown in the diagram, that the column which has passed over the ice marches thereafter, when the farther grass is gained, in a new direction branching away from the unchanged direction of the column which has never left the grass. It is obvious, without detailed discussion, that the extent of the deviation—the angle of refraction—depends upon the change of step at the two boundaries where grass and ice meet, and if the matter were investigated it would be found that the mathematical rule which determines this angle is the rule commonly known as the law of sines, by which the refraction of light is calculated.

To pass from this imaginary march to the analogous case of the progress of a beam of light is quite easy. The successive ranks in the formation, which represent equal distances measured in steps from an original position, or zero line, correspond to wave fronts in a beam of light. The step, which lengthens or shortens according to the nature of the medium traversed, corresponds to the wave length of light; and the grass and ice, which offer more or less impediment to the column, correspond to transparent media of varying optical density. If we assume that the step of the marching man is shortened when he passes from grass to ice in the proportion of $1\frac{1}{2} : 1$ we shall have a precise equivalent, so far as the mathematical theory is concerned, of a wedge of glass in an atmosphere of air. The diagram then illustrates what would happen to a beam of light transmitted through the field of an optical instrument if that field were occupied by a fragment of glass having the sectional formation shown by the diagram.

Herein lies the principle of which the microscopist takes advan-

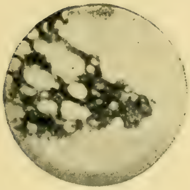

Fig. 1.

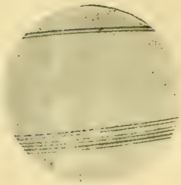

Fig. 2.

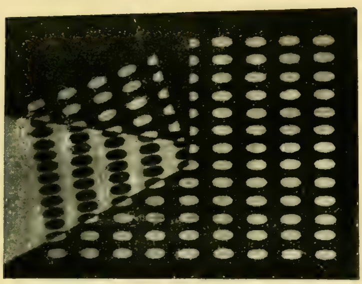

Fig. 3.

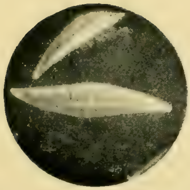

Fig. 4

tage to produce dark outlines on a bright field or bright outlines on a dark field. Since neighbouring beams of light can thus be made to take different paths it is quite possible to arrange the eye-piece so that it may catch the one and miss the other beam; and, according as the one rejected comes from the margin of the object or from the background, so the image will be outlined dark or outlined bright, in either case in contrast with the field. The effects are known as bright field or dark field illumination, the case of dark field illumination being exactly analogous to the effect obtained by artists by means of cross lighting. The most universally familiar example of cross lighting is the gibbous or crescent moon, upon the surface of which the mountain tops and salient points stand out from a sea of shadow. Fig. 4 is a diatom shown under dark field illumination, the instrument being so arranged that the light from the field escapes the eye-piece, while the light refracted by the silex of the valve is transmitted to the eye-piece and furnishes the picture seen in the photograph. The reciprocal case of bright field illumination is illustrated by Fig. 2.

The application of this mode of producing a picture is evidently subject to this limitation, that light can be thrown upon the object under an angle so wide that some of it shall reach the eye-piece and some of it shall travel out of the instrument altogether. Hence it is difficult of application when objectives of very wide angle are used, and when the angle of the condenser which supplies the light is less than that of the objective which receives it the method of dark field illumination cannot be applied at all. This limitation has long been felt to be a considerable drawback in connection with the use of high power lenses and especially of immersion lenses, which always have great angular grasp. The problem thus presented, of devising a system of dark field illumination which shall be applicable to object glasses of the widest possible aperture, has recently been attacked, and with notable success, by Dr. Siedentopf, who has succeeded by its means in rendering visible objects so minute and clustered so close to one another that by no other known contrivance can they be rendered separately visible at all. Thus he has demonstrated—to take one example only—the minute particles of gold which, disseminated through a piece of glass, impart to it by their combined effect a ruby colour. Viewed under the highest magnifying power and under any ordinary illumination these massed particles of gold would appear at best as nothing more definite than a luminous haze. But by means of the Siedentopf apparatus the illuminating beam is thrown in at the side of the specimen and crosses the field of view at right angles to the axis of the instrument, so that if it were not for the reflection and refraction of the light at very wide angles to its original direction no single ray would find its way to the eye of the observer looking down the instrument. But even this would not suffice to bring individual particles of disseminated gold into view if any considerable thickness of the specimen were so lighted up, for

in that case the diffused light from illuminated particles lying above and below the focal plane would swamp the light emanating from the focal plane itself, and would substitute a luminous haze for a definite image, obscuring the focused picture precisely as a fog wraps and obscures the objects of a landscape. To provide against this difficulty Dr. Siedentopf employs a beam of light which, while broad enough to fill the entire field of the instrument, is very shallow in the direction of the line of sight. He takes as his source of light a narrow slit like the slit of a spectroscope, arranges it in a horizontal position, and illuminating this slit by an electric arc, he forms by means of a suitable and suitably mounted lens an image of the slit in the very middle of the field of the microscope. The beam so transmitted is, of course, extremely shallow at the focal point where it is a reduced image of the external slit. Thus the passage of the beam makes an optical section of the specimen so thin in the middle of the field that the gold particles included in it stand out as individual objects against the dark background of the unilluminated glass before and behind it in the line of sight. Under these conditions it is possible to exhibit particles, however small, provided only that they are sufficiently bright to be visible, and with a source of light so powerful as that which Dr. Siedentopf's apparatus enables us to employ that limit includes bodies which are extremely minute. The disseminated particles of gold in ruby glass, to take the example already cited, can thus be seen with such precision that their distances apart can be directly measured.*

The particles of gold themselves, which appear as mere shining points, are so excessively small as to be beyond the imaging power of any microscope as yet constructed, but in aggregation they form a picture which is perfectly defined, so that it can be photographed or drawn and the space relations of its constituent parts accurately determined. In this, the latest development of the art of lighting the stage, we seem to have pressed the artifice of dark field illumination to the full limit of its theoretical capacity.

Passing now to the second of the topics with which we have to deal this evening, we are confronted by a question which may be formulated thus: Assuming that we have an object suitably mounted and suitably lighted on the stage of the microscope, how is the best representation of that object in the form of an image to be secured?

This question suggests at once the highly technical subject of lens correction and the structure of object glasses. That, however, is much too technical to be attempted on this occasion, but it is not possible to allude to it at the present time, even for the purpose of dismissing the subject, without recalling in this connection the name of the late Professor Abbe, whose untimely death a few

* A specimen of Dr. Siedentopf's apparatus exhibiting the particles of gold in ruby glass was shown by the firm of Carl Zeiss, of Jena, in the Library of the Royal Institution at the close of the lecture.

weeks ago has deprived the world of the labours of one of the most successful makers of these appliances, and one from whose inventive powers applied to this subject matter much might still have been expected had his life been prolonged. But, while putting aside the more technical and intricate questions related to lens construction, which are and always will be connected with the name of Abbe, attention may be drawn, and perhaps usefully, to a proposition concerning the limit of useful magnifying power in the microscope objective, which was formulated and proved by Helmholtz so long ago as 1874, but has been strangely overlooked by writers on the subject since that date. According to that theorem, the object glass reaches the limit of its useful development in the direction of increased magnifying power so soon as, by reason of the shortening of its focal length, the diameter of the object glass in its principal plane is reduced to something not much less than the diameter of the pupil of the observer's eye. The reason of this may be shortly stated in this way: the human eye is not a homogeneous and perfectly transparent body like a well made lens of glass or crystal; on the contrary, its mass is intersected by connective tissue, and very commonly the aqueous humour is infested by minute opacities, while through the whole structure blood corpuscles circulate, carrying oxygen and minute shadows into every part. When the retinal picture is formed of broad beams of light, these small obstructions are unnoticed, the eye can look round them by means of the unobstructed rays which enter into the beam. But if the diameter of the beams which furnish the picture is cut down to something commensurable with the diameter of the obstruction, then the loss of light occasioned by it is serious, and may, when sufficiently pronounced, cause a visible shadow of the obstruction to be projected upon the picture. The same effect of intrusive shadows is produced by specks of dust and imperfections of polish in the ocular, and by the combined effect of all these causes a practical limit is put to the magnifying power which can be usefully employed. Object glasses can be made, and have been made, with as short focal length as $\frac{1}{50}$ of an inch. But they are mere curiosities, possessing no practical advantage over the $\frac{1}{8}$, $\frac{1}{10}$ and $\frac{1}{12}$ in common use. Added magnifying power to any required extent can be obtained by means of high-power oculars, and Helmholtz has shown that the image formed in that way may be just as perfect as the image formed by an object glass of higher magnifying power backed by a lower eye-piece. In fact, the one system is the exact optical equivalent of the other, and the object glass of greater focal length has the advantage of a greater working distance from its object. Thus, in practice, the high-power objective has come to have a diameter about equal to that of the pupil of the human eye, but this—although a theoretical reason for the rule was published thirty years ago, and by no less a writer than Helmholtz—has in fact been reached as the result of

practical experience, the empirical process of trial and error, rather than of scientific deduction, for the practical opticians have treated Helmholtz's investigation of the laws of the microscope with most singular neglect.

Before passing away from this branch of the subject, it will be interesting in this room to refer to certain experiments which at the present time are in a very different position, for they are as yet quite immature, and the special interest attaching to them arises from the circumstance that they have been suggested, and very recently, by certain theoretical conclusions deduced by Lord Rayleigh from the wave theory of light. It has been commonly assumed by earlier writers, and was assumed by Helmholtz in his discussion of the theory of the microscope, that the instrument would develop its utmost resolving power when the stage was so illuminated that each part of it should shine as nearly as possible with independent light. To make the meaning of this proposition clear, suppose that you fix your gaze upon a candle flame; it all appears to be of one colour and one brightness, but you know that the uniformity is rather apparent than real, and results from the averaging by the eye of an immense number of impulses received from every point upon the luminous surface, which impulses are individually by no means all alike, but range between wide limits of variation in colour and brightness. In a word, every point is, in respect of the light which it emits, independent of every other point, and this independence of its various parts, down to the minutest into which it can be subdivided, is characteristic of a self-luminous body. Next, in contrast with this, suppose that the light of the candle is received not directly from the flame itself, but indirectly by transmission through a sheet of ground glass, or by reflection from the surface of a sheet of white paper. In that case it is plain that adjacent points upon the luminous surface—the ground glass or the white paper, as the case might be—would not be independently illuminated. They would all shine at any moment with the same borrowed light derived equally from all parts of the candle flame. Therefore, as the same components at all times enter into the light transmitted from every part of the secondary source, it will follow that this secondary source will shine with a really uniform—i.e. very approximately uniform—illumination in all its parts.

The question now proposed may be formulated thus: It being open to the microscopist to choose such a form of illumination as may best suit the object with which he is dealing, let us suppose that the object is extremely minute, and that it is of first importance, therefore, for him to develop the full resolving power of his instrument: will this be best accomplished by lighting the stage of the instrument so that every part of it shall shine, as in a candle flame, with independent light, or, as in the case of the white paper, with light of more or less uniform phase?

Now it has hitherto been taken for granted that the highest resolving power would be developed by means of the self luminous surface; but quite recently Lord Rayleigh has made this assumption the subject of mathematical investigation,* with the result that it now appears that certain forms of regular illumination—light structure, as it may, perhaps, be termed—afford a background on which very minute objects can be better displayed and more vigorously delineated than upon the structureless field of a self-luminous area. Lord Rayleigh's discussion of this topic has been purely mathematical, and he must not be held responsible for the attempts which have been made to carry into practice the suggestion contained in his paper. But such attempts have been made, and with a sufficient promise of success to warrant a brief reference to the matter in this place, although they are at present purely tentative, and indeed, in the initial stages of a tentative effort.

The exact practical problem, as it results from Lord Rayleigh's theoretical conclusions, is to illuminate the stage of the microscope with light, the phase of which shall change according to a rule of variation from point to point, so that where a strong defining line occurs in the object, it may be reinforced by the interference of the light given off by adjacent parts of the luminous field. Such a condition would not necessarily be satisfied by the borrowed light with which a sheet of white paper, or say, the surface of the moon, shines. But the method of mixing by reflection the emanations from a primary light source is by no means the only mode in which a regular structure can be given to a beam of light. Many other plans can be followed, and one which is particularly susceptible of nice adjustment and exact control is the employment for the illumination of the field of the diffraction fringe formed upon the edge of a shadow. It can be shown that in such a diffraction fringe the phase of the light varies from point to point according to a law which results in isophasal zones drawn parallel to the edge of the shadow; and experiment shows that when such zones are formed in the luminous field of a microscope, and are arranged parallel to the outlines of an object lying in that field, the outlines receive a notable accession of density. Thus, if a test plate, say, for example, a Nobert or Grayson ruling, be taken and viewed by means of an objective which is barely powerful enough to resolve it under full illumination, it may be seen strongly resolved if for the full light a suitably graduated diffraction fringe be substituted.

This experiment may be repeated upon a large scale by means of the very simple appliances which are here upon the table. The lantern is arranged so as to yield a slightly divergent beam of light. The central pencil of this divergent beam may be regarded as consisting of substantially parallel rays, like a beam of sunlight, and

* See the Journal of the Royal Microscopical Society, 1903, p. 474.

this central pencil is used to throw upon the screen the magnified shadow—which can be seen from every part of this theatre—of a toilet comb. The coarser teeth throw separate and well-defined shadows, so that in this part of the picture the image is well resolved. But the fine teeth are too fine to yield a resolved picture, and in this part of the image, therefore, the structure is entirely lost. But now, if between the comb and the source of light I introduce the blade of a table knife, with its straight edge held parallel to the teeth of the comb, you will observe that the edge of the shadow thrown by the knife is blurred by a diffraction fringe, and that where this diffraction fringe serves as a background to the picture, the images of the coarse teeth become much stronger than before, and the fine teeth are represented by a fully resolved image. Exactly the same effect is produced in the microscope by the artifice of introducing such a diffraction fringe into the background of the picture which you want to exhibit with improved definition. Figs. 5 and 6 are photographs of the shadows exhibited in this experiment.*

Assuming the best possible image to be formed of the object exhibited upon the stage, there still remains the problem of seeing the image so provided. To see an image is not quite the same thing as to see a material object, for the object can be seen from many points of view; the image, if it be an aërial image, will be visible only through a limited angle, and when the image is highly magnified as well as aërial, this limited angle is very limited indeed. Mere limitation of this angle of view is of no great consequence to the user of an optical instrument, so long as the beam emitted by the instrument is large enough to fill the pupil of the observer's eye. But Lagrange proved that, in the case of a telescope, the diameter of the emergent pencil is proportioned inversely to the magnifying power of the instrument and directly to the diameter of the object-glass. In 1874 Helmholtz extended Lagrange's theorem to the microscope, showing that in that instrument also the emergent pencil of light has a diameter inversely proportional to the magnifying power. But as the microscope cannot be fitted, like the telescope, with an object-glass many inches in diameter, it is not possible to expand the transmitted beam of light by the expedient of using a large object-glass. Helmholtz showed that in the case of a microscope it is the angle under which light from the object is received at the front face of the objective which determines the breadth of the transmitted beam, and he modified Lagrange's formula accordingly, so as to express the law that the emergent pencil of light is proportional directly to the "numerical aperture," or, as he called it, the normal magnifying power, and inversely to the actual magnifying power of the instrument.

* In the photographs which have been prepared for the illustration of this paper, a thick wire has been substituted for the knife-blade, by which means two diffraction fringes (one at the upper, the other at the lower edge of the wire) are obtained in place of one.

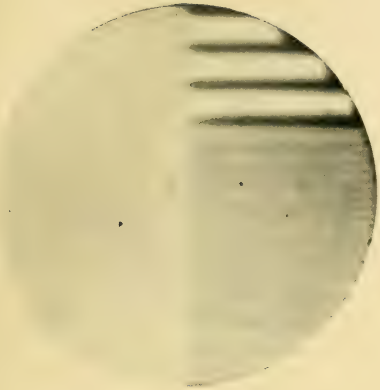

Fig. 5.

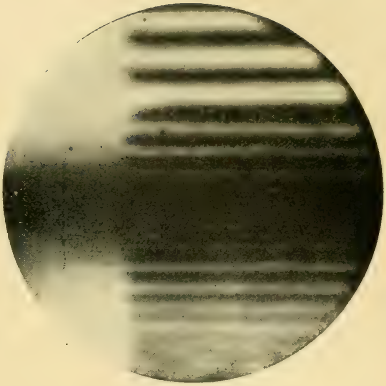

Fig. 6.

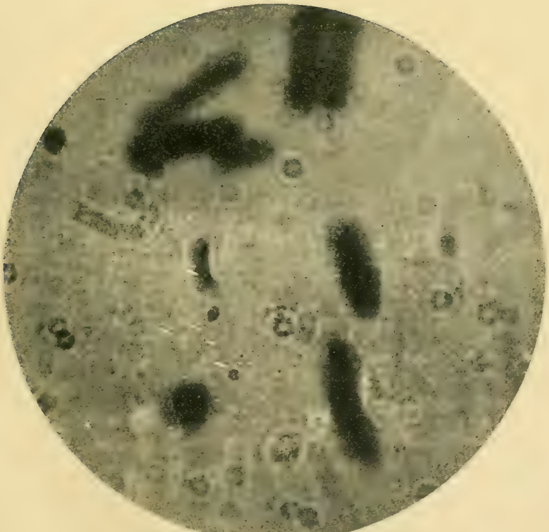

Fig. 7.

From this principle it follows that with very high magnifying powers the emergent pencil of light will become extremely small. Thus, in a microscope exhibiting to the eye a magnification of what is conventionally accounted 1000 diameters, the pencil of light which enters the eye has a diameter of about $\frac{1}{100}$ inch. The actual magnifying power of an instrument reckoned at 1000 according to the opticians' convention is about 80. It will, therefore, be understood that if photographs having any considerable magnification are to be produced it must be by the use of extremely narrow beams of light.

Reference has been already made to the inconveniences which beset the employment of such very narrow beams of light. They are more serious even in visual microscopy than in photomicrography, for, although it is possible by careful polishing and scrupulous cleaning to purge a lens of dust and obstructions, it is not possible by any expedients so to clear the eye. Eyelashes, tears, and *muscæ volitantes* at least will be introduced into the picture in addition to any specks which may be lodged on the ocular or on the back of the objective. Hence a limit is soon put to the enterprise of the instrument maker who essays to exhibit a really large scale image to the eye of his customer. Fig. 7 is a photograph of typhoid bacillus blemished in this way. The photograph does not, of course, exhibit eyelashes or specks seated in the eye. Blemishes of that sort would in practice be added to what is here shown, with the result of still further deteriorating the image. This particular specimen has been produced under extreme magnifying power, such indeed as is never used for direct vision, and is used only in connection with photography when special appliances are employed to avoid the use of an ocular. This, however, has been taken with an ordinary ocular and by means of a camera specially constructed to fill the exact position taken in the optical system of the microscope by an observer's eye. As it stands, the magnification is about 7000 diameters, the picture having been enlarged to a convenient scale for reproduction by photographic process. The original negative had a magnification of 1900 diameters, equivalent to about 6000 according to the opticians' convention, for the camera used has about one third of the magnifying power of the conventional human eye. It is not surprising that microscope makers have given up the attempt to produce pictures upon this scale of magnification, seeing that the enlargement of scale involves so much corruption of the image.

Down to the present time this defect of a highly magnified image has been thought to be manifestly insuperable. Even if the users of microscopes could be relied upon to take the extraordinary pains necessary to keep their lenses absolutely clean, they could not keep their eyes clean. Even to preserve the ocular against dust is no small matter. It might be supposed that the ocular which supplied the dust and minute hairs that so seriously impair Fig. 7 was selected

as an awful example of what an ocular may come to under exemplary neglect, but in fact there is nothing at all exceptional in its condition. At moderate magnifying powers these intrusive particles are quite invisible, and the ocular in fact was in the condition in which an ordinarily careful user would be accustomed to employ it. Nothing but the scale is out of the common, and with the high magnifying power employed the narrow beams which can be quenched by these diminutive obstructions are associated by a mathematical law. It was perfectly reasonable, therefore, for the instrument makers to regard the presentation to the eye of a satisfactory "super-amplified" image as an insoluble problem of physics.

But the insoluble problem has quite recently been solved, and by the simplest imaginable expedient. Everybody knows that when a magic lantern picture is thrown upon a screen it becomes just as visible as a real object, that is to say, the angle from which it can be viewed becomes so widely extended that the picture is visible to an entire audience when transmitted by a screen, although it would be visible only by the one or two beholders who could stand approximately in the axis of collimation, if the picture were aërial. The same expedient gets over all the difficulties of high power magnification in a microscope, and is employed in an instrument which you will be invited to examine in the library this evening. Fig. 8 is a diagram of this instrument. The arm which is extended from the tripod at its side into the tube of the microscope carries at its free extremity a small screen of finely ground glass. This glass screen is by it held in the image plane of the microscope, and receives the image formed by the object. Its grain scatters the light of this image over a wide angle, exactly as the magic lantern screen scatters the light of a lantern picture, so that the Lagrange relation between the angle of the beam and its magnifying power is broken down, and the screened image can now be seen, as if it were a real object, under any desired angle. Such an image may be subjected without impairment to high magnifying power, and so the eye-lens of the ocular is replaced by a compound microscope fitted with a half-inch objective and an ocular magnifying eight times. This system of "eye-piecing" yields, of course, an enormously super-amplified image, and it was with this apparatus, but minus the ground glass screen, that the photograph reproduced in Fig. 7 was taken. We shall now be able to see, by means of a strictly comparable photograph of the same object taken with the screen, what is the optical advantage accruing from its use.

One very obvious optical *disadvantage* there will clearly be unless steps are taken to avoid it, that is to say, the grain of the screen will itself be visible in the picture since it is displayed in the focal plane. Fig. 9 shows this result, and although the dust has disappeared, the grain of the roughened glass screen is itself a much worse blemish than any reasonable accumulation of dust and flymarks on the lenses

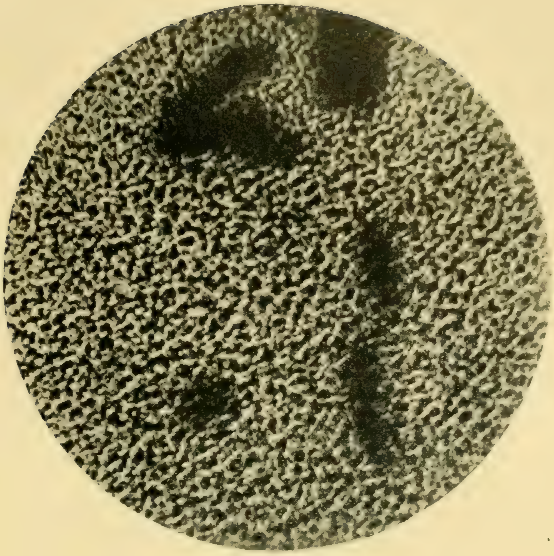

Fig. 9.

Fig. 11.

of the microscope. Microscopists call Fig. 7 a "rotten image." Fortunately they have not been called upon to find a description for

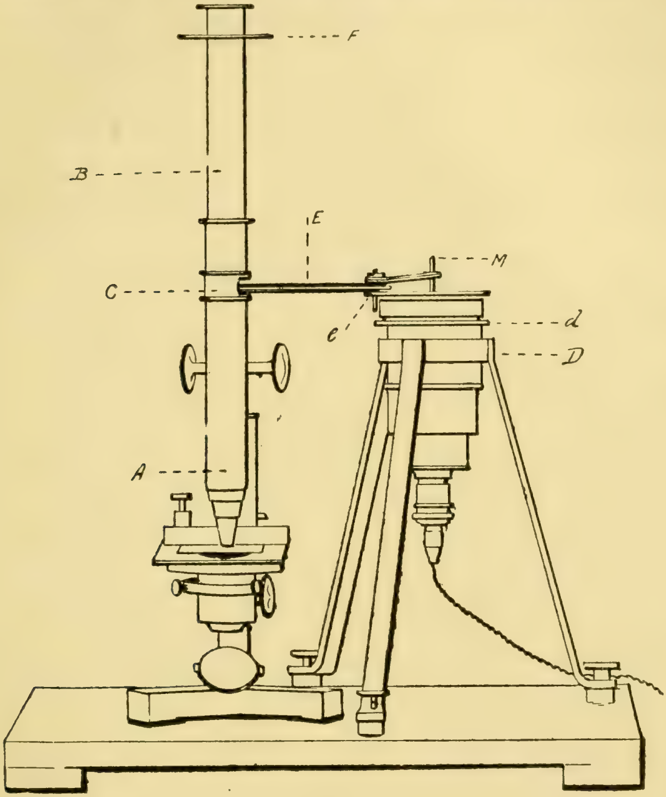

FIG. 8.

A is the principal microscope.
B is the auxiliary microscope which replaces the eye-piece of the principal instrument.
C is the image plane of the principal microscope, and indicates the position in which the oscillating screen is mounted.
D is the tripod supporting the screen and its motor.
d is a focusing ring by which the screen supporting arm is adjusted to the correct level.
E is the screen supporting arm, and
e is its elbow joint.
F is a ring for coarse focusing of the auxiliary microscope.
M is an electric motor driving the oscillating screen.

Fig. 9, for the vice of that image, formidable as it looks, is very easily corrected. The grain of the glass is conspicuous only so long

as it is at rest. By keeping it in motion it can be rendered invisible, and the motion need not be rapid for, like the grain itself, the movement is magnified by the upper microscope through which it is viewed. But two precautions must be observed. The screen must be kept accurately in the focal plane and it must not move in a closed path. The first is obvious and it will appear upon a moment's reflection that if the bright points upon the screen described closed paths in the field of view those paths would be delineated in the picture by bright lines.

The contrivance is illustrated in Fig. 10 by which in this piece of apparatus the necessary oscillation in a path too complicated to be followed by the eye or traceable on a photographic plate is imparted to the screen. Here a pulley revolves upon a stud, the stud being perforated to allow the image-forming beam to pass. This foramen in the stud occupies in use the exact centre of the tube of the instrument. The top of the stud is flattened and forms a platform upon which the ground-glass screen rests. Upon the upper face of the pulley a ring is mounted eccentrically, and in the hollow of this circular ring the screen lies; fitting it very loosely. Now when the pulley rotates it will of course rotate the eccentric ring, and the ring, wobbling round, will drag the screen round with it in its eccentric revolution. But the screen, being loose within the ring and resting on the stationary stud, will tend to lag behind the ring in its movement and will roll upon its edge within the ring with a relatively backward motion. The whole result is that the screen oscillates and revolves about a constantly varying centre of motion, and the paths described by its various parts do not return into themselves.

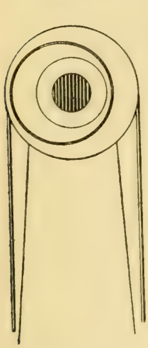

Fig. 10.

By this simple expedient the whole difficulty is overcome. The screen abolishes the intrusive images of the dust and foreign matter; the motion renders the screen itself invisible. Fig. 11 is a photograph of the image which with these precautions can be obtained even under such extreme conditions of super-amplification as those above described. And the image so formed is just as perfect when viewed directly as when recorded by the aid of photography.

[J. W. G.]

WEEKLY EVENING MEETING,

Friday, February 24, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Professor Marshall Ward, D.Sc. F.R.S.

Fungi.

Having pointed out that the attempts to derive the word fungus from *funere*, or *funus* and *ago*, *fungor*, etc., have been shown to be failures—that it comes from the Greek σπογγος, and is the same word as sponge, the lecturer proceeded to give illustrations of the fungi known to the ancients. These were, of course, all of the larger kinds, since no knowledge of micro-fungi was possible. Nevertheless, references in the Old Testament show that certain diseases—mildew, smuts, etc.—were known to the Hebrews, but of course their connection with fungi was not suspected.

The Greeks and Romans not only knew several forms of Amanita, Agaricus, Boletus, Polyporus, and of Truffles, Morels, etc., but they discriminated clearly between certain poisonous and wholesome species.

Their ideas as to the nature and origin of such fungi may seem childish to us, but they were consistent with the naïf attitude of the Greeks towards natural objects. Theophrastus, about 320 B.C., Dioscorides, about 60 B.C., and Pliny, for example, argued that since truffles and other fungi had no roots, leaves, stems, etc., they are objects apart. They arise spontaneously from earth, or by fermentation from the sap of trees, or from water.

It is interesting to note that *Polyporus officinalis* was imported and used as an article of medicine not only during classical times, but also for centuries afterwards.

In mediæval times the herbalists chiefly copied from Galen, Theophrastus, etc., and as they had no figures the early herbals afford us little information. In 1576, however, Clusius gave a series of woodcuts which are well worth looking at, and in 1601 he made a series of water-colour sketches of eighty-two of the fungi of Austria—the first drawings of the kind known. Figures in Dalechamps, 1536, Dodoens, 1593, and Parkinson, 1640, may also be compared.

The next step forward was only possible after the microscope had come into use as a scientific instrument.

It is a curious point that, abundant and conspicuous as the powdery

spores of the fungi are, no one seems to have observed their importance until Micheli, in 1729, collected and sowed a series of them, and with results, for he obtained mycelia, and in a few cases even sporophores; but it was not until a century later, 1820, that Ehrenberg, in his classical "De Mycetogenesi," traced the larger fungi to their mycelial filaments, collected and sowed spores, and grew several species of Moulds, and especially discovered the sexual act in Zyzygites. For although Micheli's ideas had been confirmed by Gleditsch in 1753 and by Schaeffer in 1762, Rudolphi and Persoon had more or less denied the germination of spores, and insisted on the spontaneous generation of the moulds.

However, before 1840 Nees von Esenbeck had cultivated a Mucor from spore to spore, and Dutrochet, 1834, and Trog, 1837, had seen the "puffing" of asci, and practically established the doctrine of wind-distribution of spores.

By these and similar successes the era of the Mould-fungi was initiated, and the labours of Corda, Tulasne, Prigsheim, Cohn, and De Bary soon introduced system into their study, and especially the exact study of life-histories showed what important results for morphology lay in the biological investigations of these micro-fungi.

The lecturer here gave illustrations of the commoner types of mould fungi, with notes on their botanical importance, and some remarks on the points he wished to emphasise later.

An early outcome of the investigations of the moulds and their allies was the discovery of what curious substrata some of them grow upon. A rapid survey of all saprophytic fungi shows that while the majority grow on the soil, on plant remains, or on dung of various kinds, peculiar forms or species occur on such bodies as resin, cork, bees' and wasps' nests, bones, limestone, insect-remains, horn, hair, feathers and hoofs, fats, and in chemical solutions such as picric acid, copper sulphate, arsenic, and poisons such as atropin, muscarin, and so forth.

Here, also, the lecturer gave some notes on details, of which the most striking was, perhaps, his own proof that the horn-destroying fungus *Onygena* will not act until its spores have been passed through the alimentary tract of an animal, or subjected to the influence of gastric juice.

In 1866, the year of publication of De Bary's book on mycology, a revolution in the study of fungi was brought about by the first morphological proof of parasitism and infection, and the clear distinction drawn between the saprophytic micro-fungi or "moulds" and the parasitic fungi which induce "diseases." The matter was of especial importance as explaining away prevalent erroneous ideas according to which these disease-fungi were outgrowths (*exanthemata*) from the moribund tissues of the host-plant itself.

De Bary's great service was to prove that a spore of a fungus arrived from outside, and, after germinating on the leaf or other

organ of a plant, bored its way in, or through a stoma, and entered the tissues. Here it lives, as does a plant in any other medium, at the expense of the substances in the tissues, which it eventually kills. It then emerges and develops its spore on the outside.

Thus was founded the "germ theory" of disease.

The lecturer here gave illustrations of the kinds of parasites referred to, and showed how the spotting of leaves is brought about by various epiphytic and endophytic forms, such as Oidium and Erysiphe, Phytophthora, Ustilaginæ and Uredinæ, etc., and directed attention to certain special genera, such as Botrytis, Aspergillus, etc.

That the ancients were acquainted with the phenomena of rot in timber is attested by remarks of Theophrastus on hollow trees and the decay of oak; but it was not until about 1830 that any idea of connecting the phenomena with fungi can be traced, and even then Theod. Hartig who discovered hyphæ in the rotten wood, thought they originated from the wood-fibres themselves. Schacht, in 1850 and 1863, figured many instances of hyphæ in wood, and showed that the fungus fed on the starch, pierced the cell-walls, and in some way induced their putrefaction; and to these and Willkomm's researches, in 1864, we may trace the origin of our knowledge of fungi as the causes of decay in timber.

Meanwhile the palæontologists also were bringing forward examples of fungus-hyphæ in fossil woods.

But the real founder of this important subject was R. Hartig, who in his works, 1874 and 1878, proved that not only are there several kinds of wood-rots in different species of trees, each induced by different forms of fungi, but that the different woods show special markings, and break up in a peculiar manner for each case, so that particular kinds of rot can be recognised by particular symptoms. Hartig, moreover, showed how the fungi got into the tree, and that these wound-fungi have special peculiarities of their own. He traced their hyphæ into the vessels and wood-elements, showed how they pierce the cell-walls, and, most important of all, proved that they dissolved out from the wood-elements the lignified constituents to which their fundamental physical properties—as wood—are due, and either leave the delignified walls soft and cellulose in character or dissolve them to a jelly.

Here the lecturer showed illustrations of the mode of action of dry rot, of *Polyporus igniarius*, and of other wood-destroying fungi, and referred to Czapeck's recent discovery of Hadromal, the probable uniform constituent of wood hitherto vaguely known as Lignin.

In another direction attention was turned to the fungi which attack insects, and which are now known often to become epidemic, to the great advantage of areas devastated by locusts, cockchafers and other grubs, caterpillars, etc.

It is a remarkable fact that, whereas the diseases of plants due to fungi are numbered by their thousands, only some two hundred or so

of animal maladies due to fungi proper are known. Whether this is due to the more acid nature of vegetable sap, to the high temperature of animal tissues, or to the greater abundance of the anti-bodies in animals, cannot be decided.

The lecturer gave illustrations of caterpillars with their destroyers, Cordyceps, Isaria, etc., growing from their mummified bodies, and referred to Torrubia's "Vegetable Wasp" legend of 1749. He also showed photographs of the "plant-worms" used in Chinese medicine, and rapidly surveyed the work of Cesati, Pasteur, De Bary, Cohn, etc., on Muscardine, Entomophthora, Empusa, Saprolegnia, and other insect-killing fungi.

But these entomophagous fungi are merely particular cases of mycoses. Every group of animals from the Protozoa and Infusoria upwards have their fungus parasites; hyphæ penetrate the ceratin of sponges and the calcareous walls of corals, and fishes and amphibia are by no means immune.

Birds and mammals suffer particularly from certain mycoses due to fungi which we have been in the habit of regarding as harmless moulds, *e.g.* Aspergillus, and even man is sometimes in danger from such fungi.

When, in 1869–70, Grohe and Block showed that small doses of the spores of Penicillium and Aspergillus are fatal to kittens, their statements were emphatically disbelieved; but Grawitz confirmed them, and the body of evidence showing that Aspergillus contains poisons toxic to birds and higher animals can no longer be overlooked. Some of these forms of aspergillosis are very serious diseases indeed.

While the new era of mycology was stimulating observers to new investigations into the life-histories of moulds, and of the parasites of animals and plants, and into the ætiology of the timber-destroying fungi, and so forth, on the one hand, it was, on the other, gradually attracting to its domain areas of investigation which had grown up independently out of the past, and which the older thinkers could never have dreamed of associating with fungi.

A conspicuous example was the study of fermentation, which, since Janssen in 1590 had brought forward a microscope of several lenses, and Leeuwenhoek had applied an improved form of it to the animalculæ in putrefying liquids, had undergone the initial stage of passage into the hands of the naturalists.

The lecturer then sketched in rapid outline the history of the theory of fermentation, from the early days when the lees or sediment (yeast) were known as the "*Faeces Vini*"—apparently owing to the shrewd suggestion of a Venetian doctor, who, in 1762, said putrefactive and fermentation processes are due to the vital activity of minute worms, the excreta (*faeces*) of which induce the turbidity and mal-odour of the liquid—to the days when the living plant-nature of these "*faeces*" was gradually established by the work of Astier, 1813, Desmazières, 1826, Quivenne, 1838, and Persoon, and especially

by Erxleben, 1818, Kützing, 1834, Cagnard Latour and Schwann, 1837.

At the same time, the sketch included an outline of the first great controversies regarding abiogenesis or spontaneous generation, brought forward from its ancient strongholds in the ignorance of the classical and mediæval writers—*e.g.* Pliny, Bock, Van Helmont—by Needham in 1745, and confuted by Spallanzani, 1765–76, Schultze, 1836, Schröder and Dusch, 1854; and to which the *coup de grâce* was given by the work of Pasteur, 1862, Cohn, 1870–75, and Tyndall.

Information derived from the brewing of quass, saki, pulque, kava, toddy, koumiss, mead, metheglin, spruce, and other beers and wines by peoples all over the world has only confirmed the ideas, of Pasteur especially, that all such fermentations are due to the presence of fungi; and although the discussions as to the process itself being due to catalytic actions and the communication of internal movements to the molecules of sugar broken up, initiated by Stahl in 1697, and revived in various forms by Liebig, 1839, and Naegeli, 1879, culminating in Buchner's views on the discovery of zymase in 1896–97, have modified the older forms of the vitalistic theory of Cagnard Latour and Pasteur, they have not dissociated fermentation from the life of the cell.

The lecturer then passed to a survey of the enzymes, those remarkable bodies which, though not themselves living, are capable of breaking up organic substances apart from the protoplasm of the cells which secrete them, and showed that since the discovery of diastase in malt by Payen and Persoz in 1833, of pepsin in gastric juice by Schwann in 1836, and of invertase in yeast by Berthelot in 1860, numerous other special enzymes have been isolated, and all the principal forms of sugar-inverting, starch-saccharifying, cellulose-dissolving, fat-splitting, proteid-converting, and oxidising enzymes occur in the fungi. Bourquelot has shown the presence of nine such enzymes in *Polyporus sulphureus*, and of seven in Aspergillus alone.

The presence of certain deadly poisons in putrefying fish, flesh, etc., and the researches consequent on the increasing knowledge of septic poisoning of wounds—with which Lister dealt so practically at the time—led to researches which, in the hands of Brieger, Sonnenschein, Armand Gautier, Selmi, and others, resulted in the isolation of more or less specific bodies, such as sepsin, cadaverine, ptomaines, leucomaines, etc. In 1876 Neucki obtained an unusually pure form, and the doctrine of ptomaine poisons may be regarded as thereby established.

For us, the point of interest here is that these poisons proved to be analogous, if not identical as a class, with a number of vegetable poisons, such as atropine, brucine, nicotine, strychnine, or at any rate presented striking resemblances to them in their physiological actions.

As close, or even closer, resemblances were found in the poisons extracted from the fungi; amanitin, bulbosin, cornutin, sphacelotoxin,

etc., all came under the same general category. In 1880 Pasteur showed that fowl cholera could be produced by means of the poison excreted by the bacilli into the liquid, from which the bacilli themselves had been removed; and Brieger, in 1885, then showed the same to be true for tetanus and typhoid. Löffler, 1887, and Hankin, 1890, then showed the same to be true for diphtheria and for anthrax, and the toxins of tetanus, cholera, etc., were obtained shortly afterwards.

Thus was founded the doctrine of toxins. The bacilli of disease do not merely induce the formation of ptomaine poisons in the decomposing tissues; they form the toxins in their own cells, and then excrete them.

The lecturer then referred to the similarities of the venenenes of snakes, scorpions, and spiders; of the toxins in eels' blood; and of the vegetable toxins ricin, robin, etc., emphasising the fact that all these bacterial, animal, vegetable, and fungal poisons belong to one and the same great family of toxic bodies.

The horribly intoxicating and poisonous drink made by certain Siberian and Kamschatkan peoples from the fly Agaric, the dry gangrene and paralysis due to ergotism, now a rare disease in western Europe, and the effects of the toxins of tetanus, diphtheria, and other bacilli, all have points in common with the poisons of snakes, of certain seeds, and so on—certain Australian species of Swainsonia impel horses which have eaten it to behave as if trying to climb trees, or to refuse to cross a twig as if it were a large log, reminding one of the effects of *Amanita muscaria* on man.

In great part, if not entirely, owing to an experiment of Nuttall's in 1888, in which he found that normal blood has bactericidal properties, researches were undertaken which resulted in the discovery that the sera of animals, either normally or if rendered immune by minimal doses of toxins, contain antidotal substances to the toxins. Behring and Kitasato, in 1890, who demonstrated the antitoxic power of blood immunised with diphtheria or tetanus to the toxins of these bacilli, was followed in rapid succession by Brieger, Ehrlich, Pick, and others, and the doctrine of the antienzymes and antitoxins was established.

The lecturer then gave two illustrative cases. Dunbar, in 1903, showed that hay-fever as already maintained by others, was not only due to the pollen of grasses, but he isolated from the pollen-grains a toxin which itself induces all the symptoms of the malady.

Not only so. He showed that the serum of horses, etc., to which the hay-fever is communicated, becomes antitoxic to the malady. This antitoxin has been distributed, and the statistics uphold the accuracy of Dunbar's views.

That pollen-grains contain enzymes has long been known, and the experiments of Darwin and others have shown that some pollens are poisonous to the stigmas of the wrong plant. Another suggestive

illustration is that given by Woronin, in which, bees having conveyed pollen, together with the spores of a Sclerotinia, to the stigmas of certain species of Vaccinium, the pollen-tubes and the fungus-hyphæ race each other down the style, and the latter usually win, and destroy the ovules. Moreover, everyone knows how corrosive and destructive the pollen-tubes of pines, etc., are in the tissues, and we must not forget that pollen-grains are spores.

The second case dwelt on by the lecturer is that of pellagra, a disease to which the ill-nourished peasantry of maize-growing countries are liable in bad seasons, when the crops are poor and mouldy.

Cene and Beste, in 1902, referred the malady to the presence of an Aspergillus in the bad grain. They also extracted from this mould a highly toxic body. Mariani, in 1903, then showed that the blood of patients cured of pellagra is antitoxic to the poison of the disease.

The lecturer pointed out that, without committing ourselves to any premature opinion as to the absolute accuracy of these views, there are two increasing classes of evidence which support his suspicion that numerous as yet insufficiently examined cases of this kind will turn out to be due to what he calls "lurking parasites" in bad grain and fodders.

The first is the large class of mycoses now referred to the poisonous action of such a "mould" as Aspergillus, a fungus shown to abound in enzymes and toxic bodies. The second is the increasing number of cases of poisoning by fodder and grain-plants, normally wholesome, but found to be deleterious in certain circumstances or years.

Cases of poisonous wheat, rye, oats, etc.—the "Taumel-Getreide," "Taumel-Roggen" of the Germans—have long been known, and the lecturer quoted cases where similar noxious effects are traced to the presence of Ustilagineæ, Helminthosporium, Cladosporium, and other fungi.

A notable case is that of the Darnel, a tiresome weed in some countries. The ancients—*e.g.* Galen—knew that darnel in bread causes dizziness, headache, and sickness, and thought that neglected wheat, etc., was transformed into darnel. Hofmeister, in 1892, examined and extracted the toxic bodies and confirmed the repeated statements as to their deleterious and even fatal action on animals.

Yet it was not until 1898 that Vogl discovered the existence of a mycelium in the seed-coats of the poisonous darnel, and in the same year this was confirmed by Hanausek and Nestler, though they did little beyond recording the presence of a fungus.

In 1903, Freeman, in the lecturer's laboratory at Cambridge, worked out the details, and left no doubt that the poisonous property is due to the fungus.

The lecturer then pointed out that a whole series of questions concerning these and similar diseases now being investigated in his

laboratory lie under suspicion of connection with grain-poisoning, or at any rate with poisoning of fungi used as food.

To say the least, we want further and extensive researches from this point of view into the ætiology of Acrodymia in Mexico, Algeria, etc., and of the Columbian Pelade, of the "trembles" of cattle and sheep, and of the "milk-sickness" of the North American prairies, and even diseases like beri-beri, etc.

The conclusions, the lecturer pointed out, to which we are driven may be thus summarised :—

(1) Fungi, like animals and other plants, including bacteria, excrete enzymes, and utilise them in the same way and for the same purposes.

(2) The poisons of the fungi are toxins, not only similar in character to the poisonous alkaloids, toxalbumens, etc., of the bacteria, and of the higher plants, the venenenes of the snakes, etc., but their poisonous actions in the paralysis of nerve-ends, etc., are essentially the same.

(3) These poisons, etc., introduced into the blood of animals, call forth the activities of antitoxins and antienzymes, as do the toxins of animals, bacteria, etc., in similar circumstances.

(4) The presumption is, therefore, justified that the action of the enzymes and toxins of parasitic fungi on the proteid cell-contents of their plant-hosts is similar in principle to that on animal proteids, and that the host reacts by means of antienzymes and antitoxins.

The lecturer then adverted to the difficulties of obtaining the toxins and antitoxins from sap, and concluded by showing in specific cases—the rusts of wheat and grasses—how probable it is that, since no anatomical features explain the facts of predisposition and immunity, and the latter cannot be referred to climatic conditions or to peculiarities of soil, etc., the above considerations will be found to apply, a matter dealt with elsewhere by the lecturer.

[M. W.]

WEEKLY EVENING MEETING.

Friday, March 3, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

CHEVALIER G. MARCONI, LL.D. D.Sc. *M.R.I.*

Recent Advances in Wireless Telegraphy.

[ABSTRACT.]

THE phenomena of electro-magnetic induction, revealed chiefly by the memorable researches and discoveries of Faraday carried out in the Royal Institution, have long since shown how it is possible for the transmission of electrical energy to take place across a small air-space between a conductor traversed by a variable current and another conductor placed near it; and how such transmission may

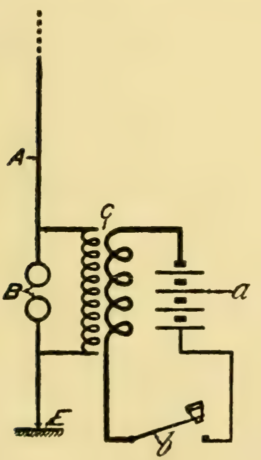

FIG. 1.

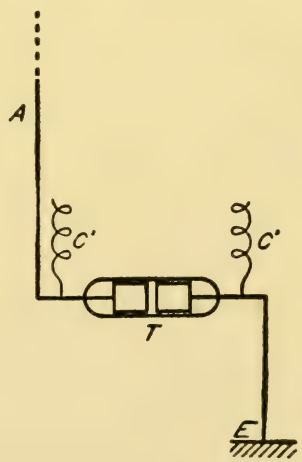

FIG. 2.

be detected and observed at distances greater or less, according to the more or less rapid variation of the current in one of the wires, and also according to the greater or less quantity of electricity brought into play.

Maxwell, inspired by Faraday's work, gave to the world in 1873 his wonderful mathematical theory of electricity and magnetism,

demonstrating on theoretical grounds the existence of electro-magnetic waves, fundamentally similar to but enormously longer than waves of light. Following up Maxwell, Hertz in 1887 furnished his great practical proof of the existence of these true electro-magnetic waves.

Building on the foundations prepared by these great men, the author carried out in 1895 and 1896 his first tests, with apparatus

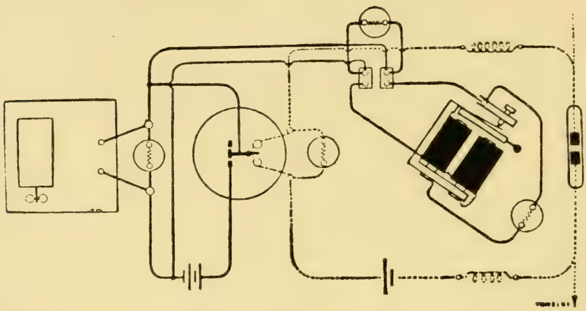

Fig. 3.

which embodied the principle on which long-distance wireless telegraphy is successfully worked at the present day. This early arrangement is shown in Figs. 1, 2, and 3.

In Figs. 1 and 2 are shown diagrammatically the complete transmitting and receiving plants, and in Fig. 3 are shown the circuits of the receiving instruments.

The main feature of the system is the utilisation of the earth-effect by connecting both the transmitting and receiving instruments between earth and a raised capacity.

The later improvements introduced in the author's system of wireless telegraphy have been directed towards the following ends:—

1. To obtain independence of communication, or the prevention of interference between several neighbouring stations.

2. To increase the distance of communication.

3. To increase the efficiency of the apparatus, its accuracy and working speed.

One of the chief objections which is raised against wireless telegraphy is that it is possible to work only two or a very limited number of stations in the immediate vicinity of each other without causing mutual interference, or producing a jumble by the confusion of the different messages. This objection appears to be much more serious to that section of the public which knows little or nothing of telegraphy in general than to telegraph engineers, who know that without organisation and discipline the same interference would occur in the great majority of ordinary land telegraphs. For example, there is an "omnibus" line between Cork and Crookhaven. On this

line there are a dozen or more telegraph offices, all with their instruments joined up to the same wire running from the terminal stations. Now, if any of these offices should proceed to send a message, say, to Cork, whilst this office is receiving another message from Crookhaven, it would cause an interference which would result in the confusion of the two messages, thus rendering them unintelligible. Any message sent on the line will affect all the instruments, and can be read by all the other telegraph offices on the line; but certain rules and regulations are laid down, and adhered to by the operators in the employ of the General Post Office, which make it impossible for one station to interfere with the rest. It is obvious that these same rules are applicable to every case in which a group of equally tuned wireless telegraph stations happen to be in proximity to each other.

Although in many instances untuned wireless telegraphy may prove of great utility, it is, however, clear that, so long as some method of rendering stations completely independent of one another was not devised, a very important and effectual limit to the practical utilisation of wireless telegraphy would be imposed.

The new method adopted by the author, in 1898, of connecting a proper form of oscillation transformer in conjunction with a condenser (Fig. 4), so as to form a resonator tuned to respond best to

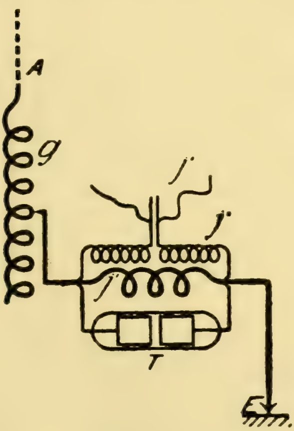

Fig. 4.

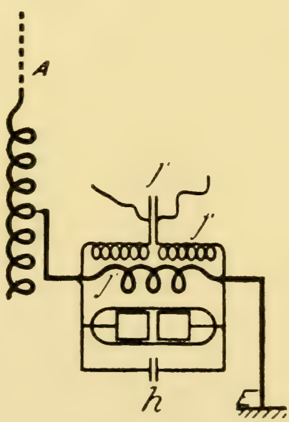

Fig. 5.

waves emitted by a given length of vertical wire, was a step in the right direction. This improvement was described by the author in a discourse which he had the honour to deliver in the Royal Institution in February 1900.

Apart, however, from these improvements introduced into the receiving circuits, it had been for some time apparent that one

difficulty in the way of obtaining syntonic effects was caused by the action of the transmitting wire. This straight rod or wire in which electrical oscillations are set up, forms, as is well known, a very good radiator or emitter of electric waves; but, at the same time, in all such good radiators, electrical oscillations set up by the ordinary spark-discharge method cease, or are damped out very quickly by the electrical radiation, which removes very rapidly the small amount of their stored up energy.

It is well known that if two tuning forks are taken, having the same periods of vibration or note, and one of them is set in motion by striking it sharply, waves or sounds will form in the air; and the other tuning-fork, if in suitable proximity, will immediately commence to vibrate, or sound in unison with the first.

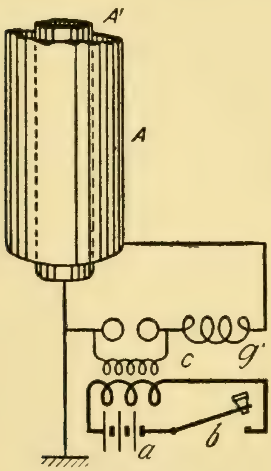

Fig. 6.

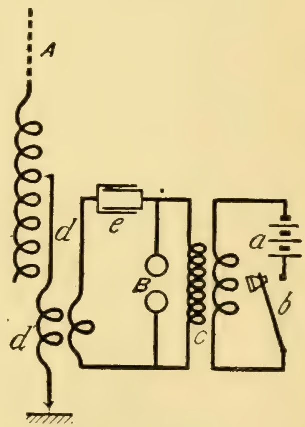

Fig. 7.

Of course, tuning-forks have to do with air waves, and wireless telegraphy with ether waves, but the action in both cases is analogous.

There is one essential condition which must be fulfilled in order that a well-marked tuning or electrical resonance may take place, and it is based on the fact that what we call electrical resonance, like mechanical resonance, depends essentially upon the accumulated effect of a large number of feeble impulses properly timed. Tuning can only be achieved if a sufficient number of these timed electrical impulses reach the receiver.

Over four years ago, the author obtained satisfactory results by increasing the electrical capacity of the radiating and resonating conductors by arranging them at each station in the form of two

concentric cylinders, or in other forms of closely adjacent conductors. The electrical capacity of such conductors, as shown in Fig. 6, is very large compared with that of a single vertical wire, with the result that the amount of electrical energy stored up in the system referred to in the first case is much larger, and does not radiate or get away in one or two waves, but forms a train of timed impulses which subsist for a certain time, which is what is required.

An arrangement consisting of a circuit containing a condenser and a spark gap, Fig. 8, constitutes a very persistent oscillator. Sir Oliver Lodge has shown that by placing it near to another similar circuit, it is possible to demonstrate effects of tuning. The experiment is usually referred to as "Lodge's syntonic jars," and is extremely interesting, but as Lodge himself points out in his book, the "Work of Hertz," a closed circuit such as this is "a feeble radiator and a feeble absorber, so that it is not adapted for action at a distance."

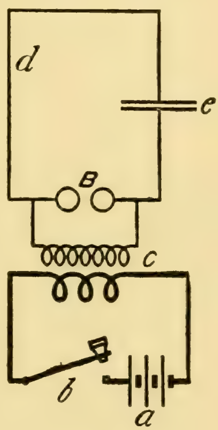

Fig. 8.

If, however, such an oscillating circuit is inductively associated with one of the author's elevated radiators, it is possible to cause the energy contained in the closed circuit to radiate to great distances, the essential condition being that the natural period of electrical oscillation of the radiator should be equal to that of the nearly closed circuit.

All the latest syntonic transmitting arrangements are based on modifications of this combination.

The general arrangement is indicated in Fig. 7.

The arrangements for syntonising or tuning the receiving stations are shown in Fig. 5. Here is shown the usual vertical conductor connected to earth through the primary of a transformer, the secondary circuit of which contains a condenser, which is connected across the coherer or detector. In this case also it is necessary that the period of electrical oscillations of the vertical wire, which includes the primary of the transformer and earth connection, should be equal to that of, or in tune with, the secondary circuit of the said transformer, which circuit includes a condenser. Therefore, in order that a transmitter (Fig. 7) should be in tune with the receiver (Fig. 5), it is necessary that the periods of oscillation of the several oscillating circuits at both stations should be equal, or very approximately so.

It is easy to understand that if we have several stations, each tuned to a different period of electrical oscillation, the periods of resonance of which are known, it will not be difficult to transmit

messages to any one of them without the signals being picked up by the other stations for which they are not intended. It is obvious that the greater the difference in periods of the oscillation or tune between two stations, the smaller will be the possibility of tapping and mutual interference.

It is also possible to connect to one sending wire, through the

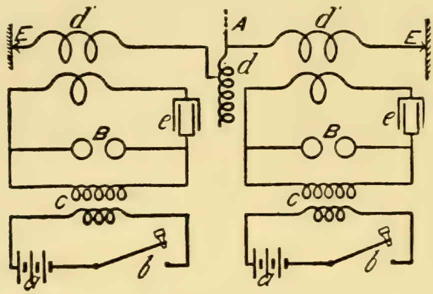

Fig. 9.

connections of different inductances, several differently tuned transmitters, and to a receiving wire a number of corresponding receivers, as is shown in Figs. 9 and 10.

It was possible, nearly five years ago, to send different messages simultaneously without interference, the messages being received on differently tuned receivers connected to the same vertical conductor.

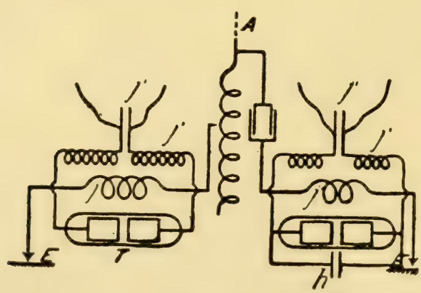

Fig. 10.

This result was described in the *Times* of October 4, 1900, by Professor Fleming, who, in company with others, witnessed the test.

A recent improvement introduced in the method of tuning the receiver is that shown in Fig. 11.

There exists at present among a large section of the public considerable misconception as to the feasibility of tuning or syntonising

wireless telegraphic installations, and also as to what is generally termed "the interception of messages." According to the accepted understanding, "intercepting" a message means or implies securing by force, or by other means, a communication which is intended for somebody else, thereby preventing the intended recipient from receiving it. Now this is just what has never happened in the case of wireless telegraphy. It is quite true that messages are, and have been, tapped or overheard at stations for which they are not intended, but this does not by any means prevent the messages from reaching their proper destination. Of course, if a powerful transmitter giving off strong waves of different frequencies is actuated near one of the receiving stations, it may prevent the reception of messages, but the party working the so-called interfering station is at the same time unable to read the message he is trying to destroy, and therefore,

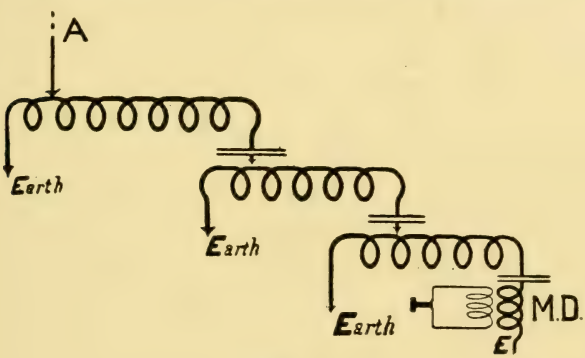

FIG. 11.

the message is not, in the popular sense of the word, "intercepted." It should be remembered that any telegraph or telephone wire can be tapped, or the conversation going on through it overheard, or its operation interfered with. Sir William Preece has published results which go to show that it is possible to pick up at a distance on another circuit, the conversation which may be passing through a telephone or telegraph wire.

Up to the commencement of 1902, the only receivers that could be practically employed for the purposes of wireless telegraphy were based on what may be called the coherer principle—that is, the detector, the principle of which is based on the discoveries and observations made by S. A. Varley, Professor Hughes, Calsecchi Onesti, and Professor Branly.

Early in that year the author was fortunate enough to succeed in constructing a practical receiver of electric waves, based on a principle different from that of the coherer. Speaking from the experience

of its application for over two years to commercial purposes, the author is able to say that, in so far as concerns speed of working, facility of adjustment, reliability and efficiency when used on tuned circuits, this receiver has left all coherers or anti-coherers far behind.

The action of this receiver is in the author's opinion based upon the decrease of magnetic hysteresis which takes place in iron, when under certain conditions this metal is exposed to high frequency oscillations or Hertzian waves.

It is constructed in the following manner and is shown in Fig. 12.

On an insulating sleeve surrounding a portion of a core, consisting of an endless rope of thin iron wires, are wound one or two layers of thin insulated copper wires. Over this winding insulating material is placed, and over this again another longer winding of thin copper wire contained in a narrow bobbin. The ends of the windings nearer the iron core are connected one to earth and the other to the elevated

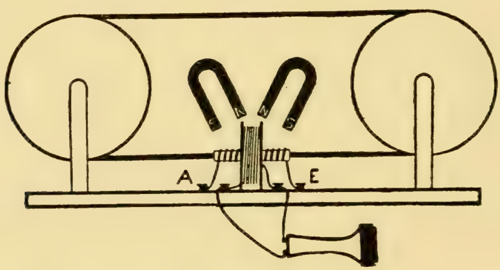

Fig. 12.

conductor, or they may be joined to any suitable syntonising circuit, such as is now employed for syntonic wireless telegraphy. The ends of the longer winding are connected to the terminals of a suitable telephone. A pair of horse-shoe magnets are conveniently disposed for magnetising the portion of the core surrounded by the windings, and the endless iron core is caused to move continuously through the windings and the field of the horse-shoe magnets.

This detector is and has been successfully employed for both long and short distance work. It is used on the ships of the Royal Navy and on all transatlantic liners which are carrying on a long-distance news service. It has also been used to a large extent in the tests across the Atlantic Ocean.

As already stated, the adoption of this magnetic receiver was the means of bringing about a great improvement in the practical working conditions of wireless telegraphy, by making it possible to do away with the troublesome adjustments necessary when using coherers, and also by considerably increasing the speed at which it is possible to

receive, the speed depending solely on the ability of the individual operators. Thus a speed of over thirty words a minute has been easily attained with the apparatus as shown in Fig. 12.

This form of magnetic receiver, however, presented a disadvantage which some people considered very important—of being able to bring about only an audible reproduction of the signals in a telephone, and consequently ineffective for actuating a recording instrument, such as would leave a documentary proof in the form of Morse signals received and inscribed on tape.

When the author had the honour to deliver his last lecture at the Royal Institution, he expressed a hope that by means of this magnetic receiver it might be possible to work a recording instrument, and he is glad to be able to announce that he has recently been able to construct a magnetic receiver that will work a relay and a recorder.

The causes which prevented the author's earlier type of magnetic receiver from working a relay were the rapidity and alternating character of the current induced by the effect of the oscillations on the iron. This current or impulse is so sudden that, although it proves to be suitable in producing a sound or click in a telephone diaphragm, it is far too quick to impart any appreciable movement to the comparatively heavy tongue of a relay, and in that way to allow a current to work a recording or other instrument. By modifying the circuits, especially by increasing their length and by the use of a particular quality of iron, the author has been able to obtain an impulse from the magnetic receiver, which is capable of working a recording instrument.

The instrument is eminently adapted for receiving messages from stations such as Poldhu, where the length of wave radiated is considerable.

The advantages of this receiver over the coherer system of receiver are very great.

In the first place, it is far more simple, requires far less attention, is absolutely reliable and constant in its action, and possesses a low and unvarying resistance. But the chief advantage lies in the fact that with this receiver it is possible to attain a very high speed of working.

The speed of the author's earlier form of magnetic receivers was limited to the rate at which the operator could read by sound. So far as speed is concerned, however, this new detector is not dependent upon the ability of the operator. It is possible to use an automatic transmitter to send messages at the rate of 100 words a minute, and the messages will be picked up and recorded quite clearly and distinctly by means of this new form of receiver.

The author here gave a demonstration of wireless transmission and reception by means of high speed "Wheatstone" instruments lent by the G.P.O., used in conjunction with his magnetic receiver.

This form of recording receiver has been satisfactorily worked

over a distance of 152 miles over land, and will shortly be employed in connection with the new transatlantic stations.

In conjunction with Professor Fleming, the author has recently introduced further improvements which greatly increase the efficiency of the apparatus, but which he is not at present free to describe. The author here demonstrated the effect of the improvement by means of a galvanometer, showing the deflection without and with the new device. The author also exhibited and explained Dr. Fleming's cymometer for measuring the length of waves used in wireless telegraphy.*

A very considerable amount of public interest has been centred during the last few years on the tests and experiments in which the author has been engaged in investigating the possibilities of wireless telegraphy over very great distances, and especially on the tests which are being carried out across the Atlantic Ocean.

The facility with which distances of over 200 miles could be covered with the author's apparatus as long ago as 1900, and the knowledge that by means of syntonic devices mutual interferences could be prevented, led the author to advise the construction of two large power stations, one in Cornwall and the other in North America, in order to test whether, by the employment of much greater power, it might not be possible to transmit messages across the Atlantic Ocean.

On the erection of these stations very extensive tests and experiments were carried out during the latter part of 1902. These tests were greatly facilitated by the courtesy of the Italian Government, which placed a 7000-ton cruiser, the *Carlo Alberto*, at the author's disposal. During these trials the interesting fact was observed that, unlike what occurs with moderate power-transmitting stations, the effect of intervening land or mountains between the sending and receiving apparatus does not bring about any considerable reduction in the distances over which it is possible to communicate; this result being due, no doubt, to the much greater length of wave radiated by the big elevated conductor of the long-distance stations, compared with the shorter wave-length radiated by the smaller and less powerful installations. Thus messages were received from Poldhu at the positions marked on the map (Fig. 13), which is a copy of the map accompanying the official report of the experiments. These positions, at which signals were received direct from Poldhu, are in the Baltic near Sweden, at Kiel, the North Sea, the Bay of Biscay, also Ferrol, Cadiz, Gibraltar, Sardinia, and Spezia. Messages were received distinctly in these places from Cornwall, although, in the Baltic, the whole of England, the Netherlands, and part of Germany and Scandinavia lay between Poldhu and the *Carlo Alberto*. Also, at Cadiz and Gibraltar, the whole of Spain

* Dr. J. A. Fleming, "On an Instrument for the Measurement of the Length of Long Electric Waves and also Small Inductances and Capacities." *Proc. Roy. Soc. Lond.*, vol. lxxiv.

intervened ; and at Spezia and Cagliari, in the Mediterranean, the whole of France, including the Alps, lay in a direct line between the two stations.

After these experiments the *Carlo Alberto* was sent back from the Mediterranean to Plymouth, and thence conveyed the author to Canada ; and in October 1902 signals from Poldhu were received on board ship throughout the voyage up to a distance of 2300 miles.

In December 1902 messages were exchanged between the stations at Poldhu and Cape Breton, but it was found that communication was better from Canada to England than in the opposite direction.

The reason for this is to be attributed to the fact that, owing to the support and encouragement of the Canadian Government, the station at Cape Breton had been more efficiently and more expensively equipped ; whilst as regards Poldhu, owing to the uncertainty as to what would be the attitude of the British Government at that

Fig. 13.

time towards the working of the station, the author's company was unwilling to expend large sums of money for the purpose of increasing its range of transmission.

As, however, messages were sent with ease and accuracy from Canada to England, the author considered it his duty to send the first messages to their Majesties the Kings of England and Italy, both of whom had previously given him much encouragement and assistance in his work. The author was thus enabled to announce that the transmission of telegraphic messages across the Atlantic Ocean without the use of cable or wire was an accomplished fact. Messages were also sent to His Majesty from Lord Minto, the Governor-General of Canada, who had taken a considerable interest in the author's early experiments in Canada. Officers delegated by the Italian Government, and a representative of the London *Times*, were present at the transmission of the messages, and over

2000 words were sent and correctly received in the presence of these Government delegates.

Further tests were then carried out at the long-distance station erected at Cape Cod, in the United States of America, and a message from President Roosevelt was successfully transmitted from this station to His Majesty the King.

In the spring of 1903 the transmission of news messages from America to the London *Times* was attempted, and the first messages were correctly received and published in that newspaper. A breakdown in the insulation of the apparatus at Cape Breton made it necessary, however, to suspend the service, and, unfortunately, further accidents made the transmission of messages unreliable, especially during the spring and summer. In consequence of this, the author's company decided not to attempt the transmission of any more public messages until such time as a reliable and continuous service could be maintained and guaranteed under all ordinary conditions.

It is curious to note that the transmission of messages across the Atlantic appeared to be much easier during the winter months of December, January and February, than during the spring and summer, but no serious difficulties were encountered before April. These were partly caused by the insulation of the aerial not being so good during the damp spring weather, when the snow and ice are melting and thawing, as at this period the insulation is much more difficult to maintain in an efficient condition than during the dry and crisp Canadian winter.

A new station, supplied with more powerful and more perfect apparatus, is in course of erection, and the author has not the slightest doubt but that in a very short time the practicability and reliability of transatlantic wireless telegraphy will be fully demonstrated.

In connection with these very powerful stations, it is interesting to observe that the fact which the author had noticed in 1895, and which he expressed in his patent of June 2, 1896, that "the larger the plates (or capacities) of the receiver and transmitter, and the higher from the earth the plates are suspended, the greater the distance that it is possible to communicate at parity of other conditions," still holds good, and therefore, the elevated conductors at these stations are much larger and higher than those used at the smaller power stations. The potential to which they are charged is also very much in excess of that used at the short-distance stations.

Pending the reconstruction of these long-distance stations, valuable tests have been carried out, and daily commercial work is carried on over distances of about 2000 miles. In October 1903, it was found possible to supply the Cunard ss. *Lucania* during her entire crossing from New York to Liverpool with news transmitted direct to that ship from Poldhu and Cape Breton.

Since June a regular long-distance commercial service has been in operation on certain ships of the Cunard Steamship Company,

which ships, throughout their voyage across the Atlantic, receive daily news messages collected for transmission by Messrs. Reuter in England, and by the Associated Press in America. At present five transatlantic steamships are thus publishing a daily newspaper containing telegraphic messages of the latest news.

The practical and experimental work carried out in connection with the long- and short-distance stations has afforded valuable opportunities for noting and studying various unknown and unexpected effects of the condition of space on the propagation of electro-magnetic waves.

The author being able to avail himself of the daily reports of over 70 ships and 50 land stations, the chances of error from what might be termed accidental results are reduced to a minimum. Thus it is interesting to observe that the difference between the propagation of the wave by day and by night is only noticeable in the case of long-distance stations; or, in other words, where a considerable amount of energy is forced into the transmitting aerial wires. For instance, all the short-distance ship-to-shore stations having a range of about 150 miles, averaged the same distance of communication by day as by night; but the long-distance stations, such as Poldhu, Cape Breton and Cape Cod, as originally constructed, averaged by day two-fifths of the distance covered by night.

The opinion has been expressed that the reason for shorter distances being covered by day is due to the electrons propagated into space by the sun, and that if these are continually falling like a shower upon the earth, in accordance with the hypothesis of Professor Arrhenius, then that portion of the earth's atmosphere which is facing the sun will have in it more electrons than the part which is not facing the sun, and therefore it may be less transparent to long Hertzian waves.

The full scientific explanation of this fact has not yet been given, but Professor J. J. Thomson has shown in an interesting paper in the *Philosophical Magazine** that if electrons are distributed in a space traversed by long electric waves, these will tend to move the electrons in the direction of the wave, and will therefore absorb some of the energy of the wave. Hence, as Professor Fleming has pointed out in his Cantor Lectures delivered at the Society of Arts, a medium through which electrons or ions are distributed acts as a slightly turbid medium to long electric waves.

In fact, clear sunlight or blue skies, though very transparent to light-waves, may act as a fog to Hertzian waves. Apparently the amplitude of the electrical oscillations radiated has much to do with the interesting phenomenon, for the author has found that if a considerable amount of power is applied to the radiating apparatus of the so-called short distance stations, the difference between the range

* Vol. iv. Series 6, August 1902.

of transmission by night and by day becomes at once apparent, although no difference is made in the wave length radiated.

A curious feature of what may be called the daylight effect is the suddenness with which it may cut off the signals at great distances. These do not, as might be supposed, die off gradually as daylight increases, but seem to fade away rapidly, and disappear entirely within the space of about two minutes.

The author does not for a moment think that this daylight effect will prove to be a serious drawback to the practical application of long-distance wireless telegraphy, as its result amounts to this, that rather more power is required by day than by night to send signals by means of electric waves over long distances.

It has been stated that one of the serious objections to wireless telegraphy lay in the fact that no means existed for directing the energy emitted by the stations. If we assume this fact to be correct, we certainly find that, if it presents certain disadvantages, it also presents many perhaps counterbalancing advantages. For example, if a cable is laid between England and Canada it can only serve for communication between these two countries; but if a wireless connection is established between two such countries the stations may be instantly used in time of war, or in any other emergency, to communicate with other stations, situated say, at Gibraltar, the West Indies, or some inland point in North America, and also, if necessary, with warships carrying apparatus tuned to the waves such stations radiate. By means of syntony, although the energy cannot be directed in one direction, it can however be picked up at certain distances only by certain tuned receivers, as occurs now with the ships crossing the ocean. Fifty of these ships carry wireless apparatus, but only five of them have the instrument tuned to receive the long-distance news messages sent from Poldhu; and, as a matter of fact, these messages are received only by those five specially tuned ships.

Before concluding, it may not be out of place to give a few details as to the practical uses to which the author's system of wireless telegraphy has already been put.

There are now over 80 British and 30 Italian warships equipped. A number of these warships are fitted with long-distance apparatus, and are therefore able to keep in touch with England when far out on the Atlantic, at Gibraltar, and in the Mediterranean. Admiral Lord Charles Beresford has authorised the author to say that during the last cruise of the Channel Fleet from Gibraltar to England they had no difficulty whatever in receiving messages from Cornwall during the entire voyage by means of special long-distance receivers.

Seventy liners, belonging respectively to England, Italy, France, Germany, Holland, Belgium, and the United States, are fitted with the author's apparatus, and are engaged in carrying on commercial work for the benefit of passengers between ship and ship and between ship and shore; and for this latter purpose there are over 50 land

stations with which to communicate. During 1904, 67,625 commercial messages were sent and received at the ship and shore stations controlled by the author's company.

It is also used as a branch of the Italian telegraphic system for ordinary commercial purposes across the Adriatic Sea, namely, between Bari (in Italy) and Antivari (in Montenegro), and in the Straits of Messina at Messina, Reggio and Giovanni. Also, in connection with the British Post Office, from Cornwall to the Scilly Islands, on the not infrequent occasions of the breaking down of the cables.

As to the future of wireless telegraphy, the author expresses his confidence in its ability to furnish a more economical means for the transmission of telegrams from England to America, and from England to the Colonies, than the present service carried on by the cables.

It is true that many scientific men are dubious of the practicability of sending electric waves to great distances. Others are not. On a recent memorable occasion at Glasgow University, Lord Kelvin publicly stated that he not merely believed that messages could be transmitted across the Atlantic, but that some day it would be possible to send messages to the other side of the globe. Apart from the practical and economical possibilities of this step, when realised, the transmission of messages to the Antipodes would open up the possibility of carrying out tests of very great scientific interest. For example, if transmission to the Antipodes were possible, the energy ought to go over or travel round all parts of the globe from one station to the other, and perhaps concentrate at the Antipodes, and in this way it might perhaps be possible for messages to be sent to such distant lands by means of a very small amount of electrical energy, and, therefore, at a correspondingly small expense.

[G. M.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

William Knibb Appleton, Esq.
George Henry Burford, Esq., M.B.
Walter Spencer Burns, Esq.
Mrs. Elinor C. L. Close,
Miss Alyce Donaldson,
Paul von Fleischl, Esq.
George Ernest Haslip, M.D.
The Lady Honora Janet Hodgson,
Mrs. Laye,
Alfred J. B. Tapling, Esq.
Lieut.-Col. Frederick Drummond Vincent-Wing, C.B.
John Edward Wolfe, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Brewers' Journal for Feb. 1905. 8vo.
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Electrical Review for Feb. 1905. 4to.
Electrical Times for Feb. 1905. 4to.
Electricity for Feb. 1905. 8vo.
Engineer for Feb. 1905. fol.
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Journal of the British Dental Association for Feb.–March, 1905. 8vo.
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Law Journal for Feb. 1905. 8vo.
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London University Gazette for Feb. 1905. 4to.
Machinery Market for Feb. 1905. 8vo.
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Mois Scientifique for Jan. 1905. 8vo.
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Musical Times for Feb. 1905. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 10, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

PROFESSOR J. J. THOMSON, LL.D. D.Sc. F.R.S., Cavendish Professor of Experimental Physics, University of Cambridge.

The Structure of the Atom.

IN 1897 I had the pleasure of bringing before the Royal Institution experiments showing the existence of *corpuscles*, i. e. negatively electrified bodies having a mass exceedingly small compared with that of an atom of hydrogen, until then the smallest mass recognised in physics. A suggestive and striking property of these corpuscles is that they are always the same from whatever source they may be derived. The corpuscles were first detected in the rays which are projected from the cathode when an electric discharge passes through a vacuum tube, and it was found that whatever the nature of the residual gas in the tube, or whatever the metal used for the electrodes, the corpuscles were always the same. Other sources of corpuscles soon came to light; they were found to be projected from incandescent metals, from metals illuminated by ultra-violet light, and from radio-active substances; but whatever their source the corpuscles were always the same. This fact, in conjunction with their small mass, suggests that these corpuscles form a part of the atom, and my object this evening is to discuss the properties of an atom built up of corpuscles. As these corpuscles are all negatively electrified, they will repel each other, and so if an atom is a collection of corpuscles, there must in addition to the corpuscles be something to hold them together; if the corpuscles form the bricks of the structure, we require mortar to keep them together. I shall suppose that positive electricity acts as the mortar, and that the corpuscles are kept together by the attraction of the positive electricity. We do not know nearly so much about positive as we do about negative electricity; we have never obtained positive electricity associated with masses less than the mass of an atom; in fact, appearances all point to the conclusion that positive electrification is produced by the withdrawal of corpuscles from a previously neutral body. These conditions are satisfied, if we suppose with Lord Kelvin that in the atom we have a sphere uniformly filled with positive electricity, and that the corpuscles are immersed in this sphere. The attraction of the

positive electricity will tend to draw the corpuscles to the centre; the mutual repulsion between the corpuscles will tend to drive them away, and they will arrange themselves so that these tendencies neutralise each other.

Let us now consider the kind of atom we could build up out of corpuscles and positive electricity. The mathematical investigation of this problem leads to the following results. The simplest atom containing 1 corpuscle would have 1 corpuscle at the centre of the sphere of positive electrification; the 2 corpuscle atom would have the 2 corpuscles separated by a distance equal to the radius of this sphere; the 3 corpuscle atom would have the 3 corpuscles at the points of an equilateral triangle, whose side is equal to the radius of the sphere; 4 corpuscles would be at the corners of a regular tetra-

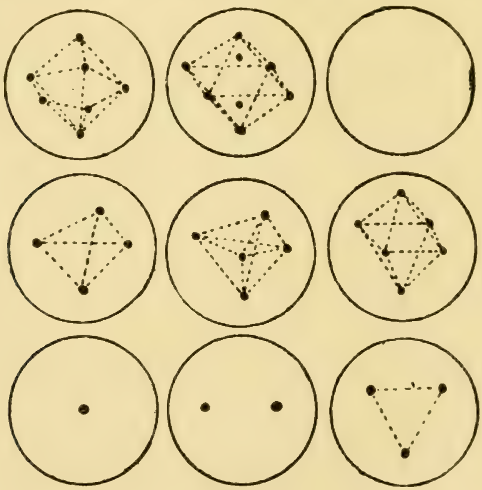


FIG. 1.

hedron, whose side is equal to the radius of the sphere; 5 corpuscles are situated, 4 at the corners and 1 at the centre of a tetrahedron; 6 at the corners of an octahedron; 7 and 8 are more complicated, as the simplest arrangements for 7 and 8, an octahedron with 1 at the centre and a cube, are both unstable; and for 7 we have a ring of 5 in one plane with 2 on a line through the centre at right angles to the plane; and 8 we have the octahedron with 2 inside. These arrangements are shown in Fig. 1.

When the number of corpuscles is large, the calculation of the positions of equilibrium becomes very laborious, especially the determination of the stability of the various arrangements. I will therefore treat the subject from an experimental point of view, and apply to this purpose some experiments made with a different object many

years ago by an American physicist, Professor Mayer. The problem of the structure of the atom is to find how a number of bodies, which repel each other with forces inversely proportional to the square of the distance between them, will arrange themselves when under the attraction of a force which tends to drag them to a fixed point. In these experiments the corpuscles are replaced by magnetized needles pushed through cork discs and floating on water. These needles having their poles all pointing in the same way repel each other like the corpuscles; the attractive force is due to another magnet placed above the surface of the water, the lower pole of this magnet being of the opposite sign to the upper pole of the floating magnets. This magnet attracts the needles with a force directed to the point on the water surface vertically below the pole of the magnet. The forces acting on the needles are thus analogous to those acting on the corpuscles in our model atom, with the limitation that the needles are constrained to move in one plane.

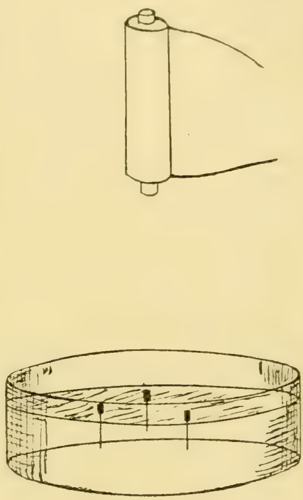


Fig. 2.

As I throw needle after needle into the water you see that they arrange themselves in definite patterns, 3 magnets at the corners of a triangle, 4 at the corners of a square, 5 at the corners of a pentagon; when, however, I throw in the sixth needle this sequence is broken. The 6 needles do not arrange themselves at the corners of a hexagon, but 5 go to the corners of a pentagon, and 1 goes to the middle; a ring of six with none in the inside is unstable. When, however, I throw in a seventh, you see I get the ring of 6 with 1 in the middle; thus a ring of 6, though unstable when hollow, becomes stable as soon as 1 is put in the inside. This is an illustration of the fundamental principle in the architecture of the atom: the structure must be substantial. If you have a certain display of corpuscles on the outside, you must have a corresponding supply in the interior; these atoms cannot have more than a certain proportion of their wares in their windows. If you have a good foundation, however, you can get a large number on the outside. Thus we saw that when the ring was hollow, 5 was the largest number of needles that could be stable. I place in the centre a large bunch of needles and you see that we get an outer ring containing 22 needles in stable equilibrium.

The proportion between the number which is in the outer ring and the number inside required to make the equilibrium stable is shown in the following table:

Number in outer ring	5	6	7	8	9	10	12	13	15	20	30	40
Number inside. . .	0	1	1	1	2	3	8	10	15	39	101	232

We see from these illustrations how the corpuscles would arrange themselves in the atom, confining ourselves for the present to the case where the corpuscles are constrained to move in one plane. The corpuscles will arrange themselves in a series of rings, the number of corpuscles in the rings getting greater and greater as the radius of the ring gets greater. By the aid of the above table we can readily calculate the way any number of corpuscles will arrange themselves. Let us suppose for example we have 20 corpuscles and try to arrange them so as to have as few rings as possible ; we see from the table that we cannot have more than 12 in the outside ring, for 13 would require 10 inside, and would be impossible with less than 23 corpuscles : thus 12 will be the number in the outside ring and there are eight left to dispose of ; these cannot form a single ring with no corpuscles inside, as 5 is the greatest number that can do this ; the 8 corpuscles will therefore break up into two systems, a ring of 7 with 1 inside. You see that when I try the experiment with 20 magnets they arrange themselves in this way.

If we follow the kind of atoms produced as we gradually increase the number of corpuscles, we find that certain arrangements will recur again and again ; thus take the case of 20 corpuscles ; this consists of the arrangement 1–7–12, the arrangement for 8 is 1–7 ; the atom of 20 corpuscles may be regarded as formed by putting another storey to the atom of 8 corpuscles ; if we go to 37 corpuscles, we find the arrangement is 1–7–12–17, i.e. another storey added to the atom of 20, while for 56 we have 1–7–12–17–19, the atom of 37 with another storey added. Thus the possible atoms formed by numbers of corpuscles from one to infinity could be arranged in classes, in which each member of the class is formed by adding another storey to the preceding member ; the structures of all the atoms in this class have much in common, and we might therefore expect the physical as well as the chemical properties of the atoms to have a general resemblance to each other. This property is, I think, analogous to that indicated by the periodic law in chemistry. We know that if we arrange the elements in the order of their atomic weights, then, as we proceed in the direction of increasing atomic weight, we come across an element, say lithium, with a certain property ; we go on and after passing many elements which do not resemble lithium, we come across another, sodium, having many qualities in common with lithium. Then as we go on, we lose these properties and come across them again when we arrive at potassium ; exactly the kind of recurrence we should get with our model atoms, if we suppose the number of corpuscles in the atom to be determined by its atomic weight.

Let me give another instance of the way the properties of these

atoms resemble the properties of the chemical atom. I will take the electro-chemical property of the atom. Some atoms, such as those of lithium, sodium, potassium, have a strong tendency to be positively electrified, while others like chlorine, bromine, iodine, tend to be negatively electrified. Now the way our model atom gets positively electrified, is by losing a negatively electrified corpuscle; thus, those atoms in which the corpuscles are loosely held would tend to get positively electrified, while those whose corpuscles are very firmly held would not get positively electrified, and might be able to bear the disturbance due to another corpuscle placed outside without disintegration, and with this additional corpuscle they would be negatively charged. Now let us see how this property would vary from atom to atom. I will take a numerical case. Suppose we begin with 59 corpuscles; we should have by the table 20 on the outside, and 39 in the inside; but as 39 is the least number of corpuscles that can hold a ring of 20 in stable equilibrium, the equilibrium of this atom would have nothing to spare; it would be in rather a tottery condition, and a corpuscle would be easily detached, leaving the atom positively charged. Let us now go to the atom with 60 corpuscles; it would still have 20 on the outside, but it would have 40 on the inside, and be more stable than 59; it would not so easily lose a corpuscle; and would not thus be so electro-positive as 59; as we go on up to 67 we have still 20 on the outside but get more and more in the inside, the difficulty of getting a corpuscle out therefore increasing, and the atom getting more and more electro-negative. Let us see what happens when we get to 68; here we have 21 on the outside and 47 inside, but as 47 is the smallest number which can keep 21 in equilibrium, this equilibrium is shaky, and as in the case of 59 corpuscles the atom would be very electro-positive. Thus, as we increase the atomic weight, we get for a certain range, a continual diminution in the electro-positive character; this goes on until we get to 67, then there is a sudden jump from the electro-negative 67 to the electro-positive 68, followed again for a time by a continual decrease in electro-positive characteristics with increasing atomic weight. Compare this with the behaviour of the atoms of the chemical elements

Li	Bi	Bo	C	N	O	Fl
Na	Mg	Al	Si	P	S	Cl
K	—	—	—	—	—	—

The electro-positive character diminishes as we proceed from Li to Fl, then there is a sudden change from the electro-negative Fl to the electro-positive Na, then another diminution in the electro-positive character to Cl, and then another sudden change from Cl to K.

The model atoms we are considering are all built up of the same materials—positive electricity and corpuscles—hence the atoms of any one element would furnish the raw materials for the atoms of any other element, and a rearrangement of the positive electricity and corpuscles

would produce transmutation of the elements. Whether the atoms of our elements will tend to break up into the atoms of other elements will depend upon the relative stability of the atoms, and the stability of an atom will depend mainly upon its potential energy; if this is large, the atom will be liable to break up or change. I have calculated for atoms containing from 1 to 8 corpuscles the potential energy of the atom per corpuscle: i.e. the potential energy of the atom divided by the number of corpuscles in the atom, making the assumption that positive electricity behaves like an incompressible fluid, i.e. that its density is invariable. The result is represented graphically in Fig. 3; the vertical ordinates represent the potential energy per corpuscle, the horizontal abscissæ the number of corpuscles in the atom. You will notice that the curve is a wavy line with peaks and valleys; the atoms corresponding to the peaks would have greater potential energy than their neighbours, and would therefore tend to be unstable, while those in the valleys, having relatively little potential energy, would be stable.

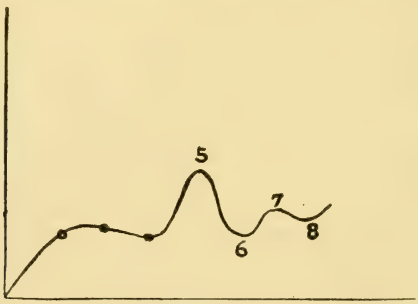


Fig. 3.

The case is in many respects very analogous to the case of a number of stones scattered over a hilly country whose section is represented by Fig. 3; the stones, if subject to disturbances, would run from the hills into the valleys, and though the stones might be uniformly distributed to begin with, yet in course of time they would accumulate in the valleys. So also in the chemical problem, though the number of atoms of the different elements might initially not be very unequal, yet, in course of time, those in the valleys would increase, and those on the peaks diminish, so that some elements would increase, while others would tend to become extinct. The smallest potential energy is that of an atom consisting of a single corpuscle; this is the goal which all the atoms would ultimately reach, if subject to disturbances sufficiently intense to lift them over the intervening peaks. Thus, on this view, the general trend of the universe would be towards simplification of the atom—though there might be local eddies. The final stage would be that in which all the atoms contained only one corpuscle. This result depends upon the assumption that the positive

electricity is incompressible, i.e. that its density is constant; if we had assumed that the volume of the positive electrification is the same whatever may be the quantity of electricity, we should have found that, although there would still have been changes from one element to another, the general trend would have been in the opposite direction, i.e. the simple atoms containing only one corpuscle would gradually condense into more and more complex atoms.

Chemical Combination. Action of the Atoms on each other.—We have hitherto confined our attention to the consideration of the stability of the arrangements of the corpuscles in the atom. We shall now proceed to discuss the question of the action of one atom on another, and the possibility of the existence of stable configurations of several atoms, in fact the problem of chemical combination.

As far as I know, the only cases in which the conditions for equilibrium or stable steady motion of several bodies acting upon each other have been investigated, is that suggested by the solar system; the case in which a number of bodies—suns, planets, satellites—attract each other with forces inversely proportional to the square of the distance between them. The complete solution of this problem, or anything approaching a complete solution, has proved to be beyond the powers of our mathematical analysis; but enough has been done to show that with this law of force, stable arrangements of the mutually attracting bodies only occur under stringent conditions. Thus, to take a very simple case, that of three bodies, it has been shown that, when the bodies are equal, there is no arrangement in which the steady motion is stable; if, however, the masses are very unequal, then it is possible for such an arrangement to exist. Another very interesting case is one investigated by Maxwell in connection with the theory of Saturn's rings. It is that of a large planet surrounded by a ring of satellites, each satellite following its neighbour at equal intervals round one circular orbit. Maxwell showed that this system was only stable under certain conditions, the most important being that the mass of the planet must be much greater than that of the satellite. The proportion between the mass of the smallest planet able to retain the ring in steady motion and the mass of one of the satellites increases very rapidly as the number of the satellites increases: if P is the mass of the planet, S that of a satellite, n the number of satellites, Maxwell showed P must be greater than $\cdot 43\, n^3$ S. The consequences of this are interesting from the analogy shown in the case of chemical combination. Thus, suppose the mass of a satellite were $\frac{1}{100}$ part of that of the planet, then the result shows that the planet could retain 1, 2, 3, 4, 5, 6 satellites, but not more than 6. With 6 satellites the planet is, to use a chemical term, saturated with satellites, and the behaviour of the system is equivalent to that of the atom of a sexavalent element, which can unite with 6 but with not more than 6 atoms of hydrogen.

The existence of a limit to the number of systems in a ring, which a central system can hold in stable equilibrium, is not peculiar to any

special law of force. We have already seen examples of it inside the atom, where the central force on the satellites is supposed to be proportional to the distance. We have just seen that it holds in the planetary system, where the central force varies inversely as the square of the distance. I have found that this limit exists for all the laws of force I have tried, although of course the number of satellites which can be held in equilibrium depends on, among other things, the law of force.

The law of the inverse square is not favourable to the formation of stable systems, even when, as in the astronomical problem, the forces between the various bodies are all attractive ; it is quite inconsistent with stability when, as in the case of the chemical atoms, some of these bodies carry charges of the same sign, and so repel each other. Thus, suppose we have the central body charged with positive electricity, while the satellites are all negatively electrified, so that the central body attracts the satellites, while the satellites repel each other. With forces varying inversely as the square of the distances between them, it is easy to show that with more than one satellite stability is impossible.

The mathematical investigation of the case where the satellites repel each other shows that, in order to ensure stability, the central attraction must, in the neighbourhood of the satellite, increase when the distance of the satellite from the planet increases. Inside the atom we have supposed that the central attraction was proportional to the distance from the centre, so that in this region the central force increases rapidly with the distance at all points. It is not necessary for equilibrium that the increase should be as rapid as this, nor indeed that the force should everywhere increase with the distance ; all that is necessary is that in the neighbourhood of the satellite the force should increase and not decrease as the distance increases.

It might appear at the outset as though atoms of the kind we have been considering, made up of positive electricity and corpuscles, could never form stable arrangements, for there is a theorem known as Earnshaw's theorem, to the effect that a system of bodies attracting or repelling each other with forces varying inversely as the square of the distance between them, cannot be in stable equilibrium. This result does not prevent the existence of stable arrangement of atoms in the molecule, for Earnshaw's theorem only applies to the case when the bodies are at rest ; it does not preclude the existence of a state of steady motion, in which there is no relative motion of the atoms. Again, in the case of our atoms there are other forces besides the electrostatic attractions and repulsions, for if the corpuscles are in rotation inside the atom, they will produce magnetic forces, so that outside the atom there will be a magnetic, as well as an electric field. The magnetic field will greatly promote the stability of the atoms if these are charged, for it will, if strong, practically prevent motion at right angles to the direction of the magnetic force, so that the arrangement of atoms will be stable provided the electrostatic

forces give stability for displacements *along* the lines of magnetic force. For example, if at any point near an atom the magnetic force were radial, then a second charged atom at this point would be in stable equilibrium, provided the radial attraction between the atoms at that point increased as the distance between the atoms increased.

Let us now consider the forces produced by an atom of the kind we have described. Take the case of an uncharged atom, i.e. one where the sum of the charges on the negatively electrified corpuscles is just equal to the positive charge in the sphere in which the corpuscles are supposed to be placed. Let us consider the radial force to the centre due to such an atom. Since there is as much positive as negative electricity in the atom, the average radial force taken over the surface of a sphere with its centre at the atom is zero ; this does not mean that the radial force is everywhere zero, but that at some places it is directed towards the centre, and at others away from it. There

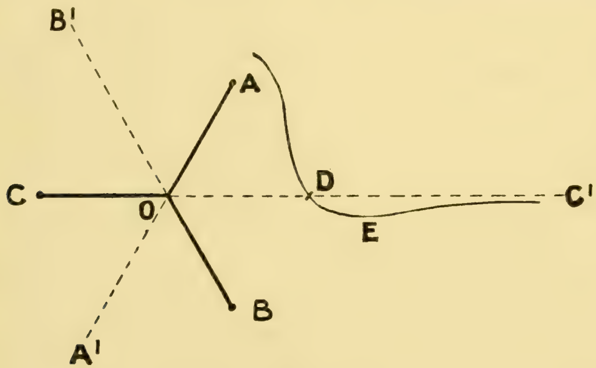


Fig. 4.

may be, as we shall see, certain directions in which the force changes from attraction to repulsion, or *vice-versâ*, as we travel outwards from the sphere.

Thus take the case of three corpuscles placed in a sphere. The corpuscles, when in equilibrium, are at the corners of an equilateral triangle A B C ; let O be the centre of the atoms of which these corpuscles form a part. Consider the force on a positively charged particle. As we travel from A radially outwards, we find that the force is always towards O, and gets smaller and smaller as we get further and further away. As the attraction diminishes as the distance increases, there is no place at which the particle would be in equilibrium, stable or unstable. Suppose, however, we travel outwards along O C^1, the prolongation of C O, then when the particle is just outside A B, the force on the particle is repulsive. This repulsive force diminishes as we recede from the atom and vanishes at a certain distance D ; at

greater distances from the atom than D, the force is attractive and remains attractive at all greater distances; thus a positively charged particle would be in equilibrium at D, and it is easy to see that the equilibrium would be stable, for if the particle were made to approach O, the repulsive force would drive it back to D, while, if the particle were to recede from D, the attractive force would drag it back. If we represent the relation between the radial force and the distance by a graph, a point above the horizontal axis corresponding to repulsion, and one below it to attraction, we obtain a curve of the following character. The curve crosses the axis at the point D, the place where the force vanishes; after passing D, the force which is now attractive increases as the distance from the atom increases, until a point E is reached when the force is a maximum; beyond E the attraction diminishes as the distance increases. Thus, since in the region D E, the force is attractive and increases as the distance increases, a positive particle, placed in this region, might be in stable equilibrium, while outside this region the equilibrium would be unstable.

There would, of course, by symmetry be similar regions on O A^1, O B^1, the prolongations of O A and O B respectively. It will be seen

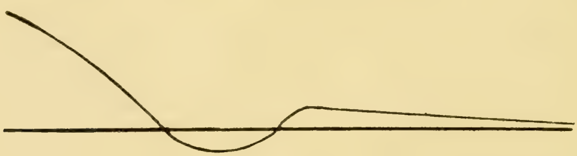


Fig. 5.

that the nature of the force between the atom and the charged particle, is of the type postulated by Boscovich, i.e. a repulsion at short distances succeeded by an attraction at greater ones. With the very simple type of atom we have been discussing, there is only one change from repulsion to attraction; with atoms containing more corpuscles, the graph representing the relation between force and distance becomes more complicated, and we may have several alternations between repulsion and attraction instead of only one as in Fig. 5.

However complicated the atom, a distribution of forces of this kind will only occur in a limited number of directions, or rather only along directions making small angles with a limited number of axes drawn in definite directions.

I have here an arrangement to show the change in direction of the force due to an atom. The atom is supposed to be one with three corpuscles; these are represented by the negative ends of three electromagnets arranged radially on a board, the positive ends of the magnets which represent the positive electrification in the sphere being at the centre. We see that along the lines O A^1, O B^1, O C^1, the magnetic force on a positive pole changes from repulsion to attraction at a

certain distance, and that the system can hold three floating magnets in stable equilibrium at a finite distance from its centre.

An atom analogous to the one we have just been considering would have the power of keeping three positively electrified particles

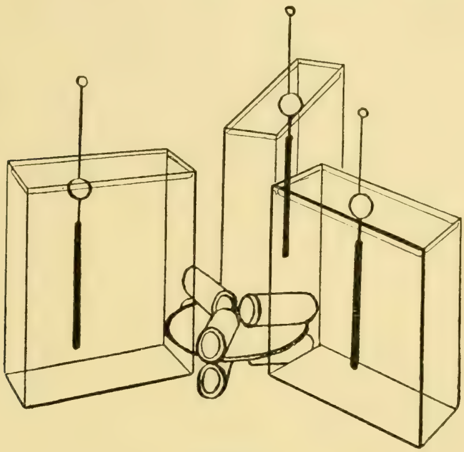


FIG. 6.

in stable equilibrium, provided these are placed at suitable distances along the lines O A[1], O B[1], O C[1]. With other arrangements of corpuscles, we should get atoms able to keep negatively electrified particles in equilibrium. Thus, for example, if we have 5 corpuscles placed at the corners of a double pyramid as in Fig. 7, then along the lines OA, O B, O C, at suitable distances from O negatively electrified particles could be in equilibrium, even if the atom were uncharged. If, however, the central atom were uncharged while the satellites were charged, the molecule, as a whole, would be charged, whereas we know the molecule is electrically neutral; we must consider, therefore, what would be the effect of giving a charge of electricity to the central atom.

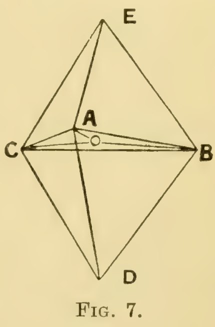


FIG. 7.

In the case of the three corpuscles, if we gave a negative charge to the central atom, the axes O A[1], O B[1], O C[1], might or might not cease to be axes of stable equilibrium for positively electrified particles. The effect of the charge would be to bring the point D of equilibrium closer to the atom—how much closer would depend upon the charge given to the atom; but as long as D kept outside the atom, stable equilibrium for positively electrified particles would

be possible ; if, however, D came inside the atom, the axes O A[1], O B[1], O C[1], would cease to be axes of possible equilibrium.

In some cases, the communication of a charge to the atom might, in addition to affecting the position of equilibrium along the axes for the uncharged atom, introduce axes of stability which did not exist when the atom was uncharged ; thus, in the case of a double pyramid Fig. 7, if we gave a positive charge to the atom, the axes O E, O D, which were not axes of equilibrium for the uncharged atom, would become so for the charged one ; for if the atom had a positive charge, the force on the negatively electrified particle would at a point a great distance from the centre along O E be an attraction, while close to E it would be a repulsion ; there must be some point then when the force changes from repulsion to attraction, so that this axis will be one of equilibrium.

In the case of a more complicated atom giving a distribution of force changing from repulsion to attraction more than once, as in the case represented in Fig. 5, there would be places along this axis where a negatively electrified particle would be in stable equilibrium and other places where a positively electrified particle would be in stable equilibrium. The effect of giving a positive charge to this alone would be to make the positions of equilibrium for the negative particles approach the atom, those for positive particles recede from it ; the effect of a negative charge would displace those positions in the opposite directions.

The forces we have been considering are those exerted by an atom on a charged particle ; they would be a part (and in many cases, I think, the most important part) of the forces acting on a second atom, if that atom had an excess of one kind of electricity over the other. Remembering, however, that there is an electric field round an atom, even when it is uncharged, and that an uncharged atom is not an atom in which there is no electricity, but one where the negative charge is equal to the positive, we easily see that two uncharged atoms may exert forces on each other ; the calculation of these forces is, on account of the complex nature of the atom, very intricate, and I shall not go into it this evening. I shall treat the subject from the experimental side. I have here two systems, each built up of magnets, each containing as many positive as negative poles, and thus analogous to an uncharged atom ; one of them is suspended from the arm of a balance, Fig. 8. You see that I can place these systems so that they repel each other when close together and attract each other when further apart, so that these atoms would be in stable equilibrium under each other's influence when separated by the distance at which repulsion changes to attraction.

The force which an atom A exerts on another atom B may be conveniently divided into two parts : the first part, which we shall call the force of the E type, depends upon the charge on B ; it is proportional to this charge and independent of the structure of B, and we might, without altering this force, replace B by any atom we pleased,

provided it carried the same charge. The other part of the force, which we shall call the M part, is independent of the charge on B, but depends essentially on its structure; this part of the force would be entirely altered, if we replaced B by an atom of a different kind.

The question now arises, What part do these two types of force play in determining the nature of the molecule? Is the stability determined by forces of the E or of the M type?

The E forces depend on the charges carried by the atom, so that in those compounds in which stability is due to the E forces, the

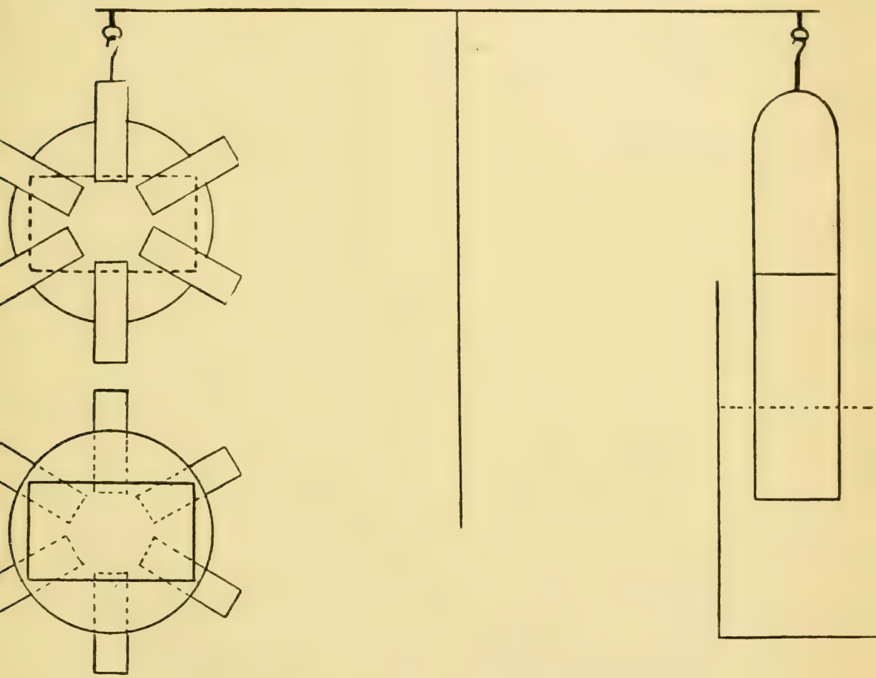


Fig. 8.

atoms must be charged. We are thus confronted with the question, Are the atoms in a molecule charged with electricity, or are they electrically neutral? Thus, to take a definite case, in the molecule of marsh gas, which we picture as a carbon atom at the centre of a tetrahedron with the four hydrogen atoms at the corners, are the hydrogen atoms charged with equal quantities of negative electricity, the carbon atom having a four-fold charge of positive, or are both carbon and hydrogen atoms uncharged? It is difficult to get direct evidence on this point, since the molecule as a whole is neutral on either supposi-

tion. There is, however, considerable indirect evidence to support the view that the atoms in many compounds are electrified. I may mention, as examples of such evidence, the power possessed by certain molecules, such as those of sugar, of rotating the plane of polarisation of light passing through them,. This power, which is associated with the presence of the asymmetric carbon atom with four dissimilar atoms attached to it, is readily explained by the electromagnetic theory of light; if the atoms in the molecule are charged, it is difficult to see how uncharged atoms could produce sufficient rotation.

Let us consider the difference in the chemical properties of a substance according as the atoms in the molecules are held together by forces of the E or M type and one held together by the M type. Let us take the molecule of marsh gas as an example, and suppose that the molecule is in equilibrium under the E forces exerted by the carbon atom on the negatively electrified hydrogen atoms and the mutual repulsions between these atoms. The forces exerted by these hydrogen atoms depend entirely on the charge carried by the hydrogen atom; none of these forces would be affected if we replaced any or all of the hydrogen atoms by any atom which carried the same charge. Hence, without altering the architecture of the molecule, we might replace any or all of the hydrogen atoms by atoms of any univalent substance. In this case, the replacement of an atom by another of the same valency would be a very simple thing.

Suppose, however, that the atoms in the molecule were held together by forces of the M type, then the forces between two atoms would depend on the structure of both the atoms. If now we were to replace one of the H atoms by an atom of another kind, not only would the force exerted by the carbon atom on this atom be altered, but the forces exerted by the atoms on the remaining three hydrogen atoms would be radically changed; this change in the forces would involve a complete change in the structure of the molecule. Thus the effects of replacement are much more serious when the forces are of the M type than when they are of the E type. The forces of the E type are, I think, those which are most effective in binding atoms of different kinds together, while the M type of forces finds its chief scope in binding similar atoms together as in the molecule of an element, or as in the connecting the carbon atoms in the carbon compounds.

Let us sum up the results we have arrived at. We have seen that an atom built up of corpuscles in the way we have described possesses, whether charged or uncharged, the following properties. There are certain directions fixed in the atoms, along which or in directions not too remote from which, electrified particles, positively electrified for some kinds of atoms, negatively electrified for others, and either positively or negatively electrified for still other kinds of atoms, will be in stable equilibrium, if placed at suitable distances from the centre of the atom. We may call those directions the valency directions, and the regions within which the equilibrium is stable the valency regions. Those who are familiar with the beautiful theory of Van't Hoff and

Le Bel on the asymmetric carbon atom, which supposes that the attractions exerted by a carbon atom are exerted in certain definite directions, these directions being such that, if the carbon atom is at the centre of a regular tetrahedron, the attractions are along the lines drawn from the centre to the corners, will perceive the resemblance between that theory and the results we have been discussing. There is, however, an important difference between the two, for on our theory the forces exerted by the atom are not confined to any special direction; the atom exerts forces all round. It is only, however, in certain directions that these forces can keep a second atom in stable equilibrium. We picture, then, the atom A as being connected with a limited number of closed regions of finite size, and any body attached to the atom must be situated in one of these regions; when each of these regions is occupied by another atom, the atom A can hold no more bound to it, and is said to be saturated.

I have not time this evening to discuss in any detail further developments of these ideas. I may however, in conclusion, call attention to a point which is illustrated by the behaviour of the carbon compounds. Suppose that C_1 C_2 are two carbon atoms near together. Then when

$$C_1 \qquad \alpha \qquad \beta \qquad C_2$$

both atoms are present, regions α, β near the line joining C_1 C_2, which were valency regions for C_1 and C_2 when these atoms were alone, may cease to be valency regions when both are present. For take the case when the stability is due to the magnetic force produced by the rotation of the corpuscles within the atoms. Along the line C_1 C_2, the magnetic force due to C_1 and C_2 will be in opposite directions, and in the region near the middle of C_1 C_2 the resultant magnetic force would be very small, so that in this the equilibrium of a charged body would be unstable; thus α β would cease to be valency regions. This reasoning would not apply to the valency regions of C_1 on the side opposite to C_2, nor of those of C_2 on the side away from C_1, so that six valency regions would remain. Thus if we consider the tetrahedra formed by the valency regions round our carbon atoms, then if two carbon atoms are placed so that two vertices of these tetrahedra come together, the regions near these vertices will cease to be valency regions, and the compound formed would have to be of the type $\begin{matrix}H\diagdown & & \diagup H\\ H\!-\!\!-\!\!> C & C <\!\!-\!\!-\!H \\ H\diagup & & \diagdown H\end{matrix}$, the two carbon atoms being held together by forces of the M type. If the tetrahedra were placed so that two edges of the tetrahedra came together, we could show similarly that the four valency regions at the ends of the edge would be suppressed and the compound would be of the type, $\begin{matrix}H\\H\end{matrix}\!\!>C \;\; C\!<\!\!\begin{matrix}H\\H\end{matrix}$, while if two faces of the tetrahedra came together the valency regions in these faces would be suppressed, and the compound would be of the type H—C C—H.

WEEKLY EVENING MEETING,

Friday, March 17, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Sir Squire Bancroft.

Dramatic Thoughts: Retrospective—Anticipative.

In the Third Act of Hamlet, Shakespeare wrote these words :—

> "It so fell out, that certain players
> We o'er-raught on the way: of these we told him;
> And there did seem in him a kind of joy
> To hear of it.
> . . . . . . stooping to your clemency
> We beg your hearing patiently."

I cannot agree with a fluent orator who sneered at the past and jeered at the "good old times," adding his belief that the best time is to-day—except to-morrow. I would rather hope, whatever may chance to be our calling, the remembrance that we are not only heirs of the work which has glorified the past, but guardians of all that has dignified the present, may lead to even better, nobler efforts in the future—the boundless future. This is the feeling which has prompted what I have now to say. Let my preface be an assurance that I have no ambition to instruct; that is the privilege of those learned men of whom somebody said somewhere, they not only know everything about something, but something about everything. I shall be grateful if I succeed in arousing interest.

Instead of overwhelming you with apologies for daring to follow all the distinction which has preceded me, for I am in the wake of men who, chiefly in the wondrous world of science, have made their names illustrious by their genius—stamped as indeed many of them are on diplomas of immortality—I will try to express some random thoughts on matters far different from those talked of in this building as a rule, and will ask you to look upon this as a holiday night, to let us all forget for a little while the matter-of-fact: to let me waft you to a more once-upon-a-time world that you may live for one brief hour in stageland—the land of dreams, in those bewitching realms where Puck and Ariel reign. Were I a singer I would warble the words Sir Henry Bishop set to sweet music :—

> "Bid me discourse; I will enchant thine ear,
> Or like a fairy trip upon the green;
> Or like a nymph, with bright and flowing hair,
> Dance on the sands, and yet no footing seen."

Standing idly now in that field where for many years I was a daily labourer; leaving me as a looker-on to ponder sometimes on the sort of work in which the best and happiest share of my life passed away, I often see much to admire and sometimes not a little to find fault with; I will try to avoid wearying you with my reflections, and am fortified by the remembrance that when the brief hour we have to pass together ends, you will again be free. Time, however, is not so hard upon me as upon the young American student who competed for a prize in rhetoric, and to whom the stern professor, sitting in judgment said, "Sir, there is the platform, your time, five minutes, your subject, 'The Immortality of the Soul.'" With such an example before me, I will spare you reference to those early players, Thespis and his disciples, and myself the labour of re-learning the little I ever knew about the ancient drama and its far-off origin. One incident only will I recall from its archives. When Quintus Roscius passed away, and that must be nearly two thousand years ago, Cicero, who had been the great comedian's pupil, thus spoke of him—"Who of us was so hard of heart as not to feel the tenderest emotions from the death of Roscius. True, he died old; but, methinks, for the excellence and beauty of his art, he merited to be exempt from death."

I have, however, since my boyhood been a keen student of more recent theatrical literature, and, it seems to me, that the drama must ever be as much a part of the world as the very tide of the sea; so surely as that ebbs and flows, so surely is a curtain somewhere rising and falling on the acting of a play. The stage, indeed, is so venerable as to be at least entitled to respect. I have often thought it must be as brave as it is old, having for ages and for ages borne—not without dignity—the worst abuse and wildest calumny; remaining in the main faithful, strong and true to its chief end and purpose—the amusement of the human race. In every branch and phase of art which enriches us, pleasure surely ought to be its first attainment, although it should be remembered that the stage has the power of teaching while the spectator often thinks he is merely being entertained; there is nothing in all the world that can so deeply reach the heart, so profoundly stir the imagination, as acting in its supremest form; and some part of what is lofty in the drama may not be altogether lost even upon the poor player, whose duty it becomes to illustrate it. How sound was La Motte's belief that were the theatre to be shut up, the stage silenced and suppressed, the world, bad as it may be, would become far more wicked. While even Jeremy Collier, its bitter enemy and violent detractor, admitted that the wit of man could not invent anything more conducive to virtue or destructive of vice.

My business with the drama this evening is not to hold a brief in its defence. It speaks for itself—trumpet-tongued—and if life in this world were to be spent in parting the tares from the wheat in all

things, I doubt if even the Bible would quite escape the process. I am told there are spots on the sun. "The web of our life is of a mingled yarn, good and ill together; our virtues would be proud if our faults whipped them not; and our vices would despair if they were not cherished by our virtues." Words which remind me that England is not only the mother of the stage, at least in Europe, but the parent of the greatest dramatic writer the world has known; whose glory does not come from that sort of knowledge which teaching can impart, but from that sort of knowledge which no learning can ever teach; whose commanding power can, alike, transport with rapture or enthral with awe; it is easy to credit the legend that while writing the scene between the Ghost and Hamlet, the poet passed a long night alone in Westminster Abbey; his name inspires players with lasting gratitude; for his works have made their craft eternal and they must share the pride I feel to have been what William Shakespeare was—an actor.

Splendid as is the array which might be drawn from other lands, I contend it would be hard to name finer tragic players than Thomas Betterton, David Garrick, Edmund Kean, and Sarah Siddons; if to that great quartet, I have not added the name of John Philip Kemble, it is only because the palm must be given to his still greater sister. They possessed the power of acting which can so entrance the spectator as to almost turn shadow into substance. Addison said of Betterton: such an actor ought to be recorded with the same respect as Roscius among the Romans. Pope said of Garrick: he never had his equal and would never have a rival. Byron said of Kean: he was life, nature, truth, without exaggeration or diminution. Talfourd said of Siddons: she was the greatest actress of whom there is any trace in memory. The ashes of Betterton and Garrick with those of Henderson—only his second as an actor, while as a reader he surpassed him—of the silver-toned Barry, best of all the Romeos, with their gifted sisters in art, Mistress Bracegirdle, Mistress Oldfield, and Mistress Pritchard, rest—if I can correctly remember the words of an eloquent American—in that grandest of mausoleums where the proudest of nations garners the memories of its most honoured children. Yes, there, in the Abbey and its cloisters, alike with Kings and Queens, with warriors and statesmen, with poets and philosophers, with men of science and men of letters, those renowned players are now "such stuff as dreams are made of, and their little life is rounded with a sleep."

"Out, out, brief candle!"

It is no doubt just that the fame of the great tragedian should eclipse that of the great comedian. The pen held by that lover of the theatre, Leigh Hunt, has truly written on this subject. "Imagination is the test of genius; that which is done by imagination is more difficult than that which is performed by discernment or experi-

ence. It is for this reason, that the actor is to be estimated, like the painter and the poet, not for his representation of the common occurrences of the world, not for his discernment of the familiarities of life, but for his idea of images never submitted to the observation of the senses. Imagination is always more esteemed than humour; humour surprises and wins, but it never elevates; imagination surprises, wins and elevates too; it transports us through every region of thought and of feeling, and teaches us that we have something within us more than mortal."

On the other hand, the distinguished writer freely admits that mediocrity is more easily attained in tragedy than in comedy, and for my own part, I feel sure that the name of many an unworthy bombastic actor of tragedy is unjustly remembered long after the fame of even peerless comedians only exists, and how lamely, in the imperfect annals of tradition or in the records of the rare student of the stage. How few, for instance, are acquainted with the splendid skill of such players as Thomas King, William Lewis, John Bannister, Robert William Elliston—and many another of equal talent—certainly, of their epoch, among the most accomplished actors in the history of the English theatre. King, who was the first Sir Peter Teazle, was also the closest friend Garrick ever made of a comrade, and was on the London stage for the amazing period of more than half a century. Charles Lamb said his acting left a taste on the palate—sharp and sweet like a quince. Lewis possessed the most unceasing activity and rapidity both in speech and motion; his animal spirits were unrivalled and he carried sunshine about with him; he bounded like a greyhound and chattered like a jay; yet he began his career as a tragedian, so must, indeed, have known his business. It was said of him that he played on the very top of his profession like a plume. It was to the delightful and versatile Bannister—when as a stage-aspirant he sought the great actor's advice—to whom Garrick said he might humbug the public in tragedy, but begged him not to try to do so in comedy, for that was a serious thing. Of Elliston, Leigh Hunt went so far as to express the opinion, on account of his extraordinary versatility—considering also the perfection of many of his performances—that he was the finest actor of that day. In spite of such praise their names seem to be written only on the sand. Indeed, I share Colley Cibber's regret that "the momentary beauties flowing from harmonious elocution cannot, like those of poetry, be their own record; that the animated graces of the player can live no longer than the instant breath and motion that presents them, or at best can but faintly glimmer through the memory of a few surviving spectators." Equally eminent and more modern writers have used their pens in the admission that the death of an author is of little moment, for his books survive him, but that when a fine actor passes it matters much, as he leaves a void which must be filled up. All true: but, although his work is neither

carved in marble, nor lives on canvas, although our poor inheritance is but "The Glory and the Nothing of a name," there is a bright and buoyant compensation in the thought that no other calling enjoys the ecstasy which belongs, I think alone, to the actor in his moments of supreme triumph.

"Look at life, it is a comedy; think of it, it is a tragedy." By the way, you may know well that Voltaire in his anxiety not to imperil the success he had achieved in tragedy, when he wrote his first comedy did so anonymously. The main plot of a tragedy is generally the consideration of whether one or more of its principal characters shall, or shall not, commit murder; the main plot of a comedy, until recent years, being whether one or more of the couples concerned in it should or should not commit matrimony: and the curtain fell upon the expected sound of wedding bells. To jump for a moment to the present time, that is no longer the method; nowadays plays begin where they used to end; when the curtain rises, more frequently than not, the last strain of Mendelssohn's march has long since died away and we look upon what has occurred "for better or worse."

Tragedy, when true, must ever command our admiration, but as one loves the sunshine better than the shade, I pay my homage to the allurement, the enchantment of Anne Bracegirdle—the darling of the theatre in her day—to Nance Oldfield: perhaps the most beautiful woman who ever trod the English stage;

> "Each look, each attitude, new grace displays,
> Her voice and motion life and music raise."

To Catherine Clive: whose transcendent talents compelled Dr. Johnson to describe her as the best actress he ever saw: adding that what Kitty Clive did best she did even better than David Garrick, but could not do half so many things well. To Margaret Woffington: a most enchanting and very witty woman: whose brilliant career was achieved despite the drawback of a harsh, unmusical voice. She earned this tribute:

> "Nor was her worth to public scenes confin'd;
> She knew the noblest feelings of the mind;
> Her ears were ever open to distress,
> Her ready hand was ever stretch'd to bless."

To Dorothy Jordan: truly an extraordinary, an exquisite creature: superior to all her contemporaries in her particular line of acting. It was said that Mother Nature had formed her when in a happy and prodigal mood; and when really in the humour to make a delightful woman she can do it supremely. What would we not give to summon those Queens of Comedy from the Silent Land and see them act! But, alas, no wealth could buy for us a single echo of their once merry voices: nor kindle one spark of the divine fire which burnt in all of them.

Deep as is my respect, profound as is my admiration, for the leaders in "the palmy days," "the good old times," I know well that such sayings are the tiresome chorus attached to other callings than the stage—the lawyer, the soldier, the painter, even the bishop, is as much haunted by them as the actor, who from time immemorial has listened to the cuckoo-cry—"the drama is dead." I have had to stop my ears to its sad refrain ever since my earliest recollections of the theatre; when I was taken as a child to see the attractive Madame Vestris, to listen to the laughing Mrs. Nisbett, and to be conscious of the waning powers of the elder Farren. I might, at that time, have also seen the farewell performances, given too early, of that chieftain of his day, William Charles Macready, whose career both on and off the stage was of high repute: in spite of the fact that the theatre was not his sweetheart, for, strange to say, he was never passionately in love with his work. Had I seen him I might have been in a like position to an aged friend of mine who, quite recently, to my amazement gave me his personal views on the acting of Edmund Kean; being afflicted with a memory for dates I could not resist reminding the dear old man that he had barely reached the mature age of nine at the time of the lamented death of that dazzling genius. Some amount of such careless criticism still exists, and always will exist, but I do claim to remember, and with distinctness, the acting of Helen Faucit: the embodiment of Rosalind, Beatrice, Imogen and others of the most poetic creations in our tongue; of Charles Kean: whose fame as the pioneer of gorgeous Shakespearian revivals has long survived the venom of Douglas Jerrold's undignified attacks; of Samuel Phelps: with many masterly performances in simple but scholarly productions; of the ill-fated Gustavus Brooke: whose natural gifts were akin to those of Salvini; of Charles Fechter: my hero of romance; of Frederick Robson: who had he not been almost a dwarf might have excelled in tragedy, he may be best described as a blend of Edmund Kean and John Liston, for he was, indeed, "tragical-comical-historical-pastoral"; of Benjamin Webster: whose remarkable, varied powers as an actor were crippled by his cares as a manager of two theatres; of Charles Mathews: most captivating, unique and natural of comedians; of the old Haymarket company in the days of its strength; but although I cherish the recollection of these and other idols of my youth I must not forget Bacon's warning, "They that revere too much old time, are but a scorn to the new." I will not, therefore, pay so poor a compliment to the living as to praise only the dead, and shall dare—with no bated breath—to mention the names of five women who have reigned in their kingdom as Thalia's champions with a splendour equal to the great ones of the past—Marie Bancroft, Margaret Kendal, Ada Rehan, Ellen Terry, Matilda Wood (Mrs. John Wood); the very salt of the beautiful art they have adorned and justified: whose mere presence in their bright spring time, their affluent summer,

filled the scene : each as distinct from one another as Raphael from Rubens, as Watts from Whistler, yet each stamping the hall-mark of her own strong personality on every part she played, all being gifted with those flecks and gleams of genius which are pearls beyond price and purchase. They are actresses of whom it might indeed be said the deaf could hear them in their eloquent faces : while the blind could see them in their vibrant voices. How deep is the debt which never can be paid them for the cares they have lightened, for the sorrows they have lessened, for the very mine of sweet memories their names recall ; they have dragged creatures from out the covers of the books where they were born, making their hearts beat and their pulses throb, often embellishing raw material with exquisite embroidery, and have enshrined their joyousness in many a grateful memory throughout the English-speaking world.

It may be that for the too early withdrawal from triumphant scenes of the great gifts of one famous actress I was in part to blame —if blame there was. I must plead excuse in a vivid remembrance of pitiful words, written by a powerful pen, on the subject of lingering too long upon the stage : words which drew with terrible force the painful picture of a much-loved servant of the public clinging to the faded chaplet won as its idol in earlier days ; of clutching at the withered trophy after the time had arrived for its graceful surrender to youth and promise ; and before the admiration once so showered upon her should be replaced by indulgence : indulgence to be followed by the bitterness of compassion ; compassion, in its turn, by the anguish of what is worse than all—indifference. Indulgence—compassion—indifference. The mere utterance of such words causes one pain. Twilight in art—as in nature—must be sad ; surely a sweeter picture is the splendid sinking of an autumnal sun. The clever woman was right who compared glory to wine—as it could provoke both intoxication and thirst. Even of the illustrious Sarah Siddons. Hazlitt once wrote, "Players should be immortal, but they are not, Like other people they cease to be young, and are no longer themselves. It is the common lot. Any loss of reputation to Mrs. Siddons, is a loss to the world. Has she not had enough of glory ? The homage she has received is greater than that which is paid to queens. The enthusiasm she excited had something idolatrous about it : does she think we have forgot her ? Or would she remind us of herself by showing us what *she was not ?* "

These thoughts bring to my mind the strong consciousness, in all its force, that the stage will soon have to mourn the loss, through his intended retirement, of one who for many years has justly been regarded by his comrades as their chief, in words familiar to him "like a great sea-mark, standing every flaw." Throughout his splendid record of work he has been devoted and true to the art he has loved and lived by : upholding always its better aims, its nobler purpose : earning always the respect, the regard, the love of that

known yet unknown world—the public. By chance I recently came across some words which once more show how history repeats itself: they were written of Queen Elizabeth. "To her encouragement the theatre was still more directly indebted for the stamp of approbation that was at once discriminating and royal, and therefore productive of the most beneficial influence upon the fortunes of the stage." How closely the language applies to the great Queen whom we have lost; to the great actor we are about to lose; for it will ever be remembered that Henry Irving was the first member of the dramatic profession to receive from his sovereign a long coveted prize—the honour and dignity of State recognition: so placing his calling on a level with the rest of the world, no more to be looked at askance, but recognised as leading to a share of the distinctions enjoyed by his fellow men. No better citizen ever bent the knee in loyalty: so reminding us that in the troublous times of long ago the actors were among the first to rally round their King, when treason was near the throne, throwing aside the sock and buskin to take up arms as servants of His Majesty.

In far more eloquent words than I can command—words from the pen of Arthur Pinero—"the history of the theatre will enduringly chronicle his achievements, and tradition will fondly render an account of his personal qualities; and so, from generation to generation, the English actor will be reminded that his position in the public regard is founded in no small degree upon the pre-eminence of Sir Henry Irving's career and upon the nobility, dignity and sweetness of his private character." It may also be truly said of Irving, as of one of the most distinguished of his predecessors, "He who has done a single thing that others never forget, and feel ennobled whenever they think of, need not regret his having been, and may throw aside this fleshly coil like any other worn-out part, grateful and contented."

"His was the spell o'er hearts,
That only Acting lends,
The youngest of the sister arts,
Where all their beauty blends.
For Poetry can ill express
Full many a tone of thought sublime;
And Painting, mute and motionless,
Steals but one partial glance from time.
But, by the mighty Actor brought,
Illusion's wedded triumphs come,
Verse ceases to be airy thought,
And Sculpture to be dumb!"

With affection and esteem I lay my tribute at Henry Irving's feet: his remarkable campaign will take its place in the history of his country, for he is one of the rulers and leaders of men who has earned the privilege given but to few, and has become the property of the world.

I now approach the difficult part of my task in venturing to be less Retrospective and more Anticipative. For a long time now the stage has been strongly recruited from the ranks of culture and refinement. It was once my privilege to render practical encouragement to many promising novices, while among the aspirants of later years I have seen distinct hope of success in a difficult vocation; in the cheery words which accompany the loving cup at a Lord Mayor's feast, "I bid them all a hearty welcome." I do so in the belief that there will be as brilliant a future for the drama as there has been a glorious past, and I would like to take this chance to say how important a step towards such an end has been the founding by Mr. Tree, on his own initiative, of a Dramatic Academy: the cordial acknowledgments of all lovers of the stage are warmly due to him for his help and generosity. Never mind if there should be difficulties for a time to be surmounted; never mind if it is hard to at once find a large band of teachers; never mind the inevitable drawbacks to all new efforts; the start is good—more than encouraging— fraught with infinite value in the future: as the students have amply proved by the rich promise of their first performance. The French dramatic school, it should be remembered, is the outcome of the devoted labour of a century. Mr. Tree has told us how the idea was rebuked as absurd because acting cannot be taught: I echo his words. "This is a truism often uttered; but if you go through the various professions, which of them can be taught? Can painting be taught? Can music be taught? Can success at the Bar be achieved by teaching? What is the truth in these matters? You cannot teach a man to be an artist—that is a question of talent and natural aptitude. But you can prepare the ground plan—you can bring order out of chaos—you can regulate the conditions out of which your great artist may emerge, and thus remove the stumbling-blocks which cumber the path to Parnassus." As an eloquent postscript I add the words of a French writer, which were quoted on this subject at the Paris Conservatoire by that distinguished actor, Monsieur Le Bargy, "I teach not, I awaken."

Perhaps some advice to dramatic aspirants may be accepted from one who for many years shared the burden and the strain of theatrical management—beginning at a strangely early age. Its rewards, when they happily befall those who go upon the stage, are hardly earned and fully merited, for I know of no other career so arduous, so exacting: passing, as much of it always must be passed, both in failure and success, in the full glare of electricity and publicity: a remark which applies to the rank and file as well as to its leaders. Hard as I know it is to avoid that glare, to shrink from its seductive glitter, something in that direction may at least be wisely done: remembering always, instead of forgetting constantly, the charm which ever haunts the theatre—mystery. It is a sad mistake to break that charm, to

parade its secrets, and the gainer, in my judgment, would be he who sometimes shields himself behind the veil. When the young actor enters the stage-door, he soon learns that the palace or the hovel are alike, but paint and canvas; he should be careful, however, to keep the disillusion to himself, instead of being in a hurry to let his friends know that he has found his new world out. Let novices recollect that they have embarked upon a life which, so to speak, begins backwards—being one of the professions in which youth is an asset—sometimes, I fear, the only stock-in-trade; the outlook then is sad indeed. Let them start with a resolve to leave their calling richer than they found it, by striving to add a stone to the monument of its greatness, and to write, if not a page, at least a phrase in its history; for I contend that although the gifts and qualities essential to make a really great actor are as rare as those needed to excel in the other arts, moderate adaptability, backed up by patience, will earn a fair and useful position on the stage. Let Shakespeare's precepts to the players abide in their memory, and let this verse by Wordsworth live there also:

> "Keep, ever keep, as if by touch
> Of self-restraining art,
> The modest charm of not too much—
> Part seen, imagined part."

Let me remind them that the refined and cultured Barton Booth —to whose memory there is also a monument in Poet's Corner, although his bones rest elsewhere—argued that the longest life was too short for the endless study of the actor. Let them remember that Rubinstein said if he neglected one day's practice he knew it the next day, the critics knew it the day after, and the public knew it the day after that. Let them not be too elated when praised, nor too cast down when found fault with: accepting criticism, when it comes from a capable pen, as a valuable stimulant. Let them beware of the tendency of the day to overdo the necessary use of cosmetics—even the light of genius cannot shine through a mask. One final warning: let them believe that they would lose little but gain much in standing more aloof from some forms of notoriety; fewer interviews, fewer paragraphs, and fewer photographs, would in the end better serve them than their perpetual and irritating so-called advertisement; Shakespeare knew well the meaning of his words "All the world's a stage," and would not admire their corruption by any of its followers into "The stage is all the world."

My closing thoughts will concern a subject on which I find myself in part at variance with many abler minds, the question of a State-endowed theatre, and I will at once say that I do not believe in such a project for England. So far as I am able—for the clock, which takes the place of stageland's prompter's bell, warns me of the brief time at my disposal—I will give reasons for my non-belief, and

will add a few words on my entire belief in the establishment of a National or Repertory Theatre.

In the spring of last year a series of interesting papers on what effort could be made to help the British stage, appeared in a leading magazine, signed by authorities in the Church, Literature, Art in all its branches—including poor little Cinderella—and by men and women of light and leading. This splendid collection of autographs did not, in many cases, mean support to a given scheme but discontent with existing conditions and general agreement that something should be done to promote a better state of things; while it was admitted that, in speaking of a subsidised theatre, the point was in no way settled whether it should be helped, as in certain foreign cities, out of the reigning sovereign's privy purse, from the coffers of the exchequer, or conducted by the municipality.

Mr. Frederic Harrison, whose words on any subject claim respect, is in hearty sympathy with the plea for the foundation of a subsidised high-class permanent theatre; although inclined to the belief that there is more hope of the object being attained by private munificence than by State aid. He thinks: "The evil complained of is both deep and wide. The drama is suffering just as literature is suffering, or as public life is suffering, and even society. The evil is an impatience of continuous attention, of serious thought, of any hitch in our ease, our luxuries, or our indulgences. We are all afflicted with a sort of tarantula of restlessness, which makes us skip from one pleasant spot to the next, without quietly enjoying any one in peace. We hurry from one crush to the next, glance at one short story after another, drop in to see the new acrobat or skirt-dancer, smoke a cigarette, and arrange a party for to-morrow. The people who sit steadily through three hours of an intellectual drama is really very limited. The difficulties are enormous. The immense distances, the five or six millions who almost force long runs of plays on managers, the fact that in London there are every night some two hundred thousand casual visitors who simply want a little excitement."

Mr. Pinero used the voice of authority to say: "A fine play is the rarest product of any country. But where other countries are ahead of us—at least, I hold so—is that when a fine play *is* produced, they do something for it. They preserve it; they take a reasonable amount of pride in it; they do not allow it, when it has once been seen and admired, to be neglected, forgotten; they take good care that from time to time it shall be displayed as evidence of what they can do in that particular department of art and literature. And there you have one of the great uses—I do not by any means say the only use—of a theatre which, whether established by the State, or by a municipal corporation, or by private munificence, shall be independent of the purely commercial conditions which too frequently govern the drama in Great Britain."

A valuable opinion was also expressed by one—the remembrance of whose acting lingers with us yet, like the sweet fragrance from some dainty perfume—I mean Mary Anderson. To use the far better words of her illustrious fellow countryman: "When she passed, it seemed like the ceasing of exquisite music." That lady was "Delighted to hear of the movement on foot for the establishment of a State-aided Theatre and Dramatic School. Both have been sadly needed since the old stock company days ceased to be. While these existed, good honest training was the rule, and those who were fortunate enough to be brought up in such companies were generally well-rounded, smooth, pleasing in their work, even though they may not have been brilliantly endowed. Perhaps they followed tradition too slavishly, but the tradition was of the best, and gave them solid ground on which to stand. Considering, therefore, the sufferings of the would-be actor, who must paint his pictures directly before a critical public, and who, unlike his brother-of-the-brush, cannot sketch in or rub out what he has done in private—considering also what the public endures in witnessing his blind and often-times frantic efforts at effect, it would indeed, be a charity to both to found a State-aided theatre and dramatic school. What an incentive these would be to conscientious work! Nothing but good could come of such a venture; good to the public, whose amusement should be of the best, good to the young actor, who, having his work perfected and polished before presenting it to his audience, would come upon the stage with confidence and authority."

I wish to add some words by Mr. John Hare, whose labours as actor and as manager have for many years shed lustre on his profession, words spoken at last year's Royal Academy banquet in an eloquent plea for the endowment of a National theatre: "A theatre which should uphold the noblest traditions of the British stage, where the best and worthiest plays of British authors should be performed, and to which a sound school of gratuitous dramatic teaching should be attached. Such an institution would at once raise the dignity of the drama to the level it occupies in other great nations of the world, and would help to check those malignant growths which are poisoning and undermining our very existence, and making our stage a byword and reproach."

I agree with every word that I have quoted, excepting only those which advocate State aid. If I remember rightly, it is a French proverb which says: "Scratch a Russian, you will find the Tartar." So I verily believe, if you scratch many a Briton there will still be found the Puritan—both being somewhat barbarous in their different ways. I think the old nursery rhyme might run: "Fe-fi-fo-fum—I smell the blood of a Puritan!" But I quarrel with no man's views, and conversion is not my mission; to hurl rational ideas against a brick wall is a waste of time, why tilt your lance against a

scarecrow ; the only persons I have ever met who object to plays and refuse to admit that any good can come of them are persons who have lacked the courage to see and hear one. I have even, in years gone by, known respectable and respected bigots whose views on all artistic subjects were so dwarfed and imbecile as to allow them to see but little difference between the daub on a sign-board and the art of the most inspired Academician.

Time, the great healer, in his justice, in his mercy, has done much to dam the floods of fierce invective and to stem the torrents of contempt with which the actor has been so often and so long assailed, but prejudice and narrow-mindedness when ingrained die hard—so that, in this country, were ever State-endowed or municipal theatres authoritatively advocated the question would, I fear, become a party one and so degenerate into election squabbles over the outlay, down to its petty details of rates and taxes : to which a large proportion of warped but powerful Nonconformists would powerfully object to contribute ; the breeches-pocket of the Puritan taxpayer would be a hard lock to pick. Remember, too, the mass of good folk in this land of ours who, if they think at all, think everything bad : who drift aimlessly down the smooth stream of dull monotony, placidly ignorant even of earthquakes and barely conscious of momentous changes in public affairs. We live in Utopia in hoping to see the drama as cherished in our country as, happily for those lands, it is in France and Germany ; where love of the stage is an inborn instinct, and regard for the theatre so generally shown by the dignified externals of their playhouses ; in itself enough to stir an actor's pride, enforcing him to respect the art he follows when he finds it so respected.

What to my mind is needed for the welfare and renown of the drama is concentration. There are numbers of good actors but they are too scattered, too restless, too prone to move about, the public must be bewildered where to look for, how to find them ; some at least among them should be banded together and find a home under one roof ; I grant that life is so fierce, so hurried, that a large section of the surging, struggling mass of humanity which makes up this vast city craves only for a light and frivolous form of entertainment : let it be so : let there be as many theatres as may be needed for the purpose, but let their managers remember that public taste is capricious—sudden changes in it occur when least expected ; let us be grateful for the admirable work now being done for the stage by a few, who need no naming, and let more be done, that we may have one playhouse which shall not, in any circumstances, be entirely given over to unbroken and often interminable runs.

How far is our country from such a triumph ; How might this end be achieved ? It is much to say but I believe there are three possible means. First and best. By the munificence of a possessor of great riches ; I trust a British subject, who could and would earn

fame by the endowment of an English theatre for National purposes in perpetuity with one stroke of the pen. Could such a man be found ? I, for one, think the search might not be hopeless, if wisely, discreetly, pursued ; in so important a quest there should be no false step. I have been granted the privilege of reading a privately printed book—a veritable monument of labour—in which such a scheme is propounded ; embracing as it does, every material point—mainly the work of a man of letters distinguished alike as a critic and as a lover of the stage. With an earnest hope that its aspirations may be completely attained I commend the volume to all who desire to see the foundation of an English National Theatre.

There is yet a second way. By an already prosperous and established manager, if he would forego certain commercial gains, engaging leading members of his company for annual incomes in place of weekly salaries and granting them some share in the financial results of his enterprise ; while they on their part might lessen their chief's labours and relieve him of many anxieties : for instance, by taking in turn, as is so ably done at the *Théâtre Français*—which remains, in spite of some decay, the first theatre of the world—the duties of *semainier*, laudably vieing with each other when on the rota of weekly control, in capacity and thoroughness. The burden of management might otherwise be lightened, but this is neither the place nor the moment for detail. I doubt if it is sufficiently remembered that the director of an important theatre takes rank with other employers of labour as a practical benefactor, for he supports large numbers of homes and families in ease and comfort.

A third and final project. By a body of capable and enthusiastic actors forming themselves into a commonwealth ; to act as a council but choosing their leader from among themselves, for if the head of a theatre, however it may be endowed or founded, is to even hope to be successful, I contend he must be as much an autocrat as the captain of a ship. The history of the English stage tells us beyond all doubt and question that its ablest and loftiest work has ever been achieved by actor-managers ; the fact is proclaimed by the names of Garrick, Kemble, Macready, Mathews, Phelps, Kean, Webster, Wigan, Hare, Kendal, Irving—if in that list I would include my own name you will forgive me in the remembrance that it is also owned by one who shared my labours—and just as truly now is the best work being done by those actors who are at the helm to-day.

As we players, with the other crafts, pass down the ages, the remorseless figure of Time following at our heels with his relentless scythe, mowing us one by one from his path, successors happily and joyously, in all the splendour of youth, arrive to take on our work, as those of to-day replaced others whose turn was done with. Nearly three hundred years have rolled away since Philip Massinger,

the dramatist, wrote: "Mark how the old actors decay, the young sprout up." So will it ever be; the vineyard may keep its most luscious grapes for favoured years, the orchard may not always yield the pick of the basket, but the beautiful art of acting will live on; if the sacred fire burns dimly for awhile it will never expire, being "not for an age, but for all time." The drama is undying and stands as the most entrancing, winning, moving, gladdening, alluring thing ever conceived for the delight and recreation of mankind.

[S. B.]

WEEKLY EVENING MEETING,

Friday, March 24, 1905.

Sir William Crookes, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Sir Oliver Lodge, LL.D. D.Sc. F.R.S. *M.R.I.*, Principal of the University of Birmingham.

A Pertinacious Current; or, the Storage of High-tension Electricity by means of Valves.

It is well known to physicists and engineers that currents of electricity can be of three principal varieties. The first and oldest variety is a continuous or steady current, of constant strength in one direction, like a river. By such a current a great quantity of electricity can be conveyed from place to place, though under conditions that it is easily stopped by any trivial obstacle, either an accidental bad joint or a purposed switch or interrupter—which is a familiar arrangement for introducing into the stream an air-gap or other narrow non-conducting obstruction, and thereby completely stopping the flow, save at the first moment of attempted stoppage, when the impetus or momentum of the current succeeds for an instant in bursting through the obstacle, with spark and flame.

The second variety is an intermittent or jerky current; which is analogous to the supply of water by an ordinary intermittent pump, such as a fire-engine or a garden-engine without its air-chamber, from whose nozzle the water issues in jerks, unless there is some elastic reservoir or chamber of variable capacity in which it can be stored under pressure, and out of which it can emerge with fair regularity.

The third variety is the important case of the well-known "alternating current"; wherein there is no progression of electricity at all, but simply a surging or oscillation to and fro, maintained by a rapidly reversed force of propulsion, such as is seldom applied to liquids; though it is applied to solids in many forms of reciprocating machine, and in several other oscillating or vibratory examples, of which the best-known variety is concerned with musical instruments. An alternating current of liquid, however, occurs in Nature, on a large and slow scale, in the tides; and it may be set up on a small scale in a churn.

An alternating electric current is characteristically produced by nearly all the magnetic methods of exciting a current discovered by Faraday, i.e. by those methods which generate a current by means of a combination of magnetism and motion, as exemplified in the ordinary

dynamo. It is true that these currents can be rectified, and so transmitted in one direction over a portion of the circuit, by means of some kind of commutator; but such an arrangement never operates over the *whole* of the circuit; there is nearly always one part of the circuit, and that the generating part, where the quantity of electricity oscillates equally to and fro. The so-called "unipolar" machines are an exception. It is, however, possible to interpose something in the path of an alternating current so as to prevent the passage one way and permit it the other, that is to say, to introduce into the circuit a one-sided kind of conductivity, such as is possessed by a trap or valve, which permits ingress but prevents egress—a kind of gate, such as is sometimes used for public gardens or parks, whereby people can go out but not come in. Or like a mouse-trap, which lets creatures in but will not let them out.

If such an arrangement exists in an alternating-current circuit, it changes the current into an intermittent or jerky one, with the progress either wholly in one direction or more in one direction than in the opposite. Such an arrangement may be conveniently called an electrical *valve* or *trap*. By the use of such valves I have found it possible to store up electricity, supplied by intermittent jerks, in a reservoir, until the tension is raised to a high value; and it can then be allowed to leak or overflow in a constant continuous stream or trickle, which, though not transmitting a very large quantity of electricity, can nevertheless overcome very considerable obstacles, pertinaciously flowing in spite of opposition, like a stream down a steep hillside. This is what I call for the moment a pertinacious current. It could always be produced by means of an electrostatic machine—either the old-fashioned frictional machine, or a Holtz or Wimshurst inductive machine—and about the pertinacity of such a current there was no dispute; but unless such machine were of enormous size, the quantity propelled was very small, and the current was essentially a weak one. Moreover the generating machine was necessarily of a delicate laboratory description, such as could hardly be regarded as appropriate for engineering practice on a large scale.

It has been always theoretically possible also to produce a high-tension or pertinacious current by means of a voltaic battery of an enormous number of cells; and by some experimenters, such as De la Rue and others, a battery of this kind was actually employed. In the case of a voltaic battery the quantity put in motion is considerable, but the difficulty was to raise the propelling force to the required amount—usually it is very weak; and in order to imitate such effects as are easily producible by a large Wimshurst machine, some considerable fraction of a million would be the number of cells necessary. The expense and trouble of such a battery would be prohibitory to most people, and to most undertakings; especially since the cells have but a temporary and rather brief life.

By the use of electric valves, however, I find it possible to employ

a current generated by mechanical and magnetic means, to convert it into an intermittent current at very high pressure, and then to store the quantity thus propelled, in reservoirs supplied with valves which prevent the flow back; so that the whole quantity transmitted in successive impulses accumulates, until the reservoir becomes full and overfull, so that it overflows, giving a steady stream or trickle through great resistances, and maintaining the continuous high-tension current required. It is as if a reservoir were being charged by a water-ram, or by waves which splash up into it through a hole, the hole being provided with a valve whereby the water supplied is trapped and not allowed to flow back again each time in futile manner, but is kept stored and accumulated until the pressure has increased to an enormous extent: the process is, in fact, exactly like pumping air through a valve into a closed reservoir, by intermittent strokes of a pump, and then allowing the reservoir to leak through a small hole, as soon as the pressure has become sufficient.

On the plan customarily used for obtaining Leyden-jar sparks in spectrum analysis, etc., the jar is charged at every break of the coil, but the charge immediately subsides through the wire of the coil, and so the jar is perfectly empty in a minute fraction of a second after the discharging impulse; accordingly unless the overflow spark occurs instantaneously it will not occur at all. There is no accumulation of impulses, and only a short spark can be obtained. But when a valve is inserted, then the charges do not sink back through the generating coil, but accumulate, and the overflow spark length now may be very much greater.

The chief use to which I wished to apply this arrangement was to the dissipation of fog or smoke, or the deposition of metallic fume, and the principle of that application is shown by attaching to the jar a wire which leads to a point immersed in some fog or smoke in a bell jar or other vessel; and now, by a momentary excitation of the coil, the jar or reservoir is filled up to bursting-point with electricity, which at this high pressure continues to discharge or fizz from the point for some time, say ten seconds or thereabouts, by which time the fog has completely disappeared.

In order to fill a vessel with an atmosphere of fog or smoke, almost any plan serves; one way is to burn smouldering brown paper, but that is not at all a good plan, since the smoke is not dense, and being hot it hovers about at the top; another is to burn tobacco, which does better: in fact, very fairly well; another plan is to burn magnesium wire, which is a cleanly and good method, and the smoke being solid and white, it illustrates the process of dissipation very well; another is to make a chemical smoke by the use of hydrochloric acid and ammonia, or by burning sulphur in an ammonia atmosphere; indeed, there are plenty of plans known to chemists; but the method I prefer for the present purpose is to make artificially a mist or fog of water vapour. It may, for instance, be

composed of clean steam blown from a boiler into a bell jar or globe, so as to make a cloud, or clean country mist; and in that case when the point is electrified the drops are seen to coalesce, and therefore rapidly to grow in size, under the action of the electricity, until it becomes a Scotch mist or fine rain, too heavy for suspension, and so rapidly falls as it does in nature. Or the air of the jar may be rendered foul beforehand with dust or other impurity, or it may be contaminated with burnt sulphur, before admitting the steam, so as to imitate the effect of water-vapour mingled with the products of the combustion of coal; and then the country mist becomes a town fog —thick, dense, and yellow; but the electricity clears it just the same.

Instead of blowing steam in from a boiler, the moisture of the atmosphere itself may be used in the following way:—Take a large glass globe with a bottle neck, fit the neck with a plug through which a tube passes leading to a compression-pump, such as a bicycle-tyre pump, a strong bottle of water being interposed, so that any air supplied has to bubble through the water; it is well also to rinse the inside of the globe with water, because otherwise it becomes misty and is soon difficult to see through. Things being thus prepared, burn a scrap of sulphur inside the vessel, which may be conveniently done by having a little cotton wick previously soaked in melted sulphur, and then, when cold and dry, lighted for a few seconds in the mouth of the globe; only a trace of burnt sulphur is wanted, otherwise it will itself make a smoke; then put the bung into the neck of the globe and slowly pump air into it (which, by bubbling through the water, becomes fairly damp), until the pressure has risen as much as is safe. Wait a short time, say a quarter of a minute or so, for the heat of compression to escape, and then remove the plug. It will indeed blow itself out when slightly loosened, and the compressed air will instantly expand, thereby chilling itself and depositing its vapour in the visible form of mist or fog—town fog in this case, because of the presence of the sulphur-combustion products—the thick, dense, yellow variety. There it will hover and remain for a long time, completely filling the globe as an opaque cloud, which is very visible; but to study its detail and behaviour it may be illuminated by the beam of an electric arc, or in the daytime it may be well seen by looking through it at a window behind the globe. Then introduce a point—an insulated point—through the neck into the interior of the globe, and supply the electricity by momentary excitation of the coil. The instant the electricity is supplied the fog is seen to be in motion, black or dust-free spaces arise in it, the particles begin to coalesce and cling together, and in a very short time the whole of the fog has disappeared, being deposited upon the sides of the vessel as a dirty wet deposit, or, if dry smoke has been used, as a kind of black snow. With such a smoke as burnt magnesium, the effect of an imitation snow-storm in the air is very soon produced by the electricity, and the walls and floor become coated over with white.

Electricity and Mist.

As to the *kind* of electricity which it is best to discharge into a fog, it is a familiar experience that negative electricity escapes from points rather more easily than positive does. Hence that is one advantage in using negative ; but there is a further advantage. A fog or mist is itself usually more or less electrified, and the sign of the electricity with which it is charged is generally positive. Electroscopes at meteorological observatories indicate this general positive electrification of mist, and they further show that when the mist begins to drizzle and clear away, or turn into rain, the sign of the electrification is frequently reversed and becomes negative ; in fact, negative electrification is generally associated with rain.

Now it is difficult to say which is cause and which is effect in such a case, but if two things are accustomed to go together, then the artificial production of one may bring about the other. Certainly the easiest way of dispersing a cloud or mist or fog is to bring it down as rain ; and if it is artificially supplied with negative electricity, that is what is very likely to happen, for that is what certainly happens on a small scale in the laboratory. In the laboratory, however, it must be admitted that either positive or negative electricity will serve the purpose ; though, on the whole, negative electricity does rather the better, perhaps because it escapes more easily ; and large-scale experiments in open air, involving a considerable amount of capital expenditure, are still wanting. But there is every reason to suppose that the natural positive electrification of a fog will assist the discharge of negative electricity from points immersed in it, and that the electrification thus supplied will result in its condensation and dissipation.

The whole of the arrangement, if it is to be applied on a large scale with currents derived from engineering mechanism, interrupted automatically and transformed to high-tension intermittent currents at each post or discharging-station, depends on the efficiency of the electric valves employed.

These are of many kinds : any air-gap is liable to act to some extent in that direction, inasmuch as it can transmit electric pressures above a certain magnitude and must obstruct those below that critical magnitude : hence if it is interposed in the path of an alternating current of which the opposite pulses are propelled by electromotive-forces of different intensity, half of such pulses may go, and the other half may be stopped. That is the case, for instance, in ordinary experience with the usual spark gap in the secondary of the common Ruhmkorff coil : it acts, in fact, as a rectifier or valve when it is of sufficient length to check the current at "make," and yet is short enough to yield to the force of the current at "break." If its terminals, instead of being similar, have different surfaces—as, for instance, if one is a point and the other a plate—then its rectifying

or valvelike action can be exerted on pulses of opposite direction even if they are urged by forces of the same magnitude; and if the two terminals are put into a vacuous receiver instead of being left in common air, the ease with which they will transmit electricity either in one direction or the other depends upon the thoroughness of the vacuum; and in certain states of the vacuum the effect may be very marked, as has been known for some time.

Mr. Edison observed some years ago that if one of the terminals was hot and the other cold, the valve-tendency, or rectifying-action, was singularly perfect and effective at very low forces; for instance, if the incandescent loop of an ordinary Swan lamp be used as one terminal, and a piece of cold metal in the same bulb be used as the other, Sir William Preece found that such an arrangement could hardly convey a moderately propelled current in one direction, while in the other direction it could easily convey such a current even though driven with infinitesimal force; and an arrangement like this has been recently adopted by Professor Fleming to make excessively feeble alternating currents record themselves on an ordinary sensitive galvanometer, and thus to get an instrument metrically responsive to the faint alternating impulses received at a distant station in wireless telegraphy.

These devices deal chiefly or wholly with impulses of low tension. For high-tension work the mercury-lamp invented by Mr. Cooper-Hewitt, which excited so much interest a few years ago, when supplied with the proper appendages designed by him, acts in a surprisingly efficient manner.

It consists of a tube containing nothing but mercury vapour, with electrodes ingeniously arranged so that at a certain stage of exhaustion the current shall be transmitted with fair ease in one direction and barely able to go at all in the other, unless driven with much greater force; and these lamps are till lately the rectifying device or valve which in the fog experiments I have chiefly employed. They are, however, for some purposes not overportable: the strain on the glass is great, and they are rather liable to break; they are excellent for the purpose for which they were intended—as lamps—but as rough-and-ready valves for discriminating between opposite rushes of electricity on an extensive scale they leave much to be desired; so that I now employ a new and specially designed form of valve with special appliances for preventing excessive strain upon the glass.

Other Uses.

Other uses than smoke deposition can be found for electric valves in connection with a high-tension intermittent supply; for instance, they can be used in any portable arrangement for the sending end of wireless telegraphy, as well as for metrical purposes

at the receiving end; and they have been so applied by Dr. Muirhead in Kent and elsewhere. For, on our system of wireless telegraphy, there is an aerial Leyden jar to be charged—one capacity area elevated, and another conducting area near the ground, though not usually connected with it, and sparks have to be caused between these two, as in Hertz's old arrangement. Such a capacity is too great for a small coil to charge at each impulse of its secondary current—but if valves are inserted and a rapid break employed, the impulses of a number of sparks accumulate until the areas are filled to bursting, and then they overflow through the discharging knobs and give the required signal, the whole operation taking place in a fraction, though with a small portable coil and considerable distances to be reached, not a very small fraction, of a second.

Another use is for the production of X-rays, especially for visual purposes on a fluorescent screen; because by the reservoir and trap method of excitation an almost steady illumination can be maintained; thus giving a much brighter effect than can be produced by intermittent illumination, even though the property of the retina called "persistence of impression" does suffice to mask the really intermittent character of the light. For such purposes it is best to use a very large coil and a rapid break. I prefer to use either a revolving mercury break or a Caldwell; on the whole, I prefer Mr. Caldwell's modification of the Wehnelt or electrolytic break. It consists of a very small aperture or throttle introduced into the path of a 200-vol circuit, containing self-induction as well as the primary of the coil o1 coils, the said aperture or throttle being submerged under a conducting liquid, like dilute sulphuric acid, through which the primary current has to pass; and I make the apertures in a replaceable crockery hemisphere. The current transmitted through liquid in such a minute orifice is excessively intense; and the heat there developed, it must be supposed, almost instantaneously vaporises the liquid in the aperture, and thereby suddenly stops the current; but the instant the current stops the steam collapses, the broken liquid is reunited, and the current again flows—to be immediately stopped as before. The rapidity with which this action goes on is astonishing—hundreds of times a second—and since there is no mechanism nor anything requiring attention, until the hole wears too large by reason of the violence of the shocks, there can hardly be any automatic break simpler in character than this; though it is quite likely that other still more efficient plans will be devised. Indeed, already the method just exhibited in London by Mr. Isenthal, employing the reversal of a "Grisson" electrolytic condenser through the primary of a coil, may possibly be a better one.

At any rate, by some means or other, the primary current is made violently intermittent; the secondary is then likewise an intermittent current of vastly increased force, though less in quantity; and in its path some valves are placed, as many as are needed. By the use of

the valves a number of pulses are all stored in a Leyden jar or other suitable condenser able to stand excessive tension without overflowing, except in the direction and for the purpose desired. And thus along this overflow or discharge path the intermittence is superseded, and a continuous current of excessively high tension is maintained, as if it were coming from a battery of an enormous number of cells. Thus can be maintained a steady discharge from a series of points, with audible and persistent fizzy noise; thus also can an X-ray bulb of high exhaustion be kept steadily illuminated for the production of very penetrating rays. And if such a current be taken through a highly resisting conductor like a damp string, the string begins first to steam, then to glow, then to char and sparkle in places, then to catch fire and char throughout, then to break and interrupt the current, except in so far as it can jump the gap in a torrent of very noisy sparks.

[O. L.]

WEEKLY EVENING MEETING,

Friday, March 31, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Professor Joseph Wright, M.A. Ph.D. D.C.L. LL.D. Litt. D.

The Scientific Study of Dialects.

The subject of this discourse is the scientific study of dialects, a branch of linguistic research which has received considerable attention amongst German and Scandinavian philologists; but in this country the scientific study of the modern dialects is still in its infancy, and is likely to remain so until the average educated Englishman gets an accurate conception of what a dialect really is. An incident which happened to the lecturer some years ago will serve to illustrate the ordinary educated Englishman's ideas about a dialect. Having spent a great deal of time in writing an historical grammar of his own village dialect, a copy of the book was sent to a distinguished classical scholar, who regarded it as an elaborate philological joke, and regretted that so much valuable time should have been wasted in trying to reduce to system and order what was after all merely barbarisms, corruptions, and mispronunciations of the "Queen's English." If a distinguished classical scholar, well versed in the ancient dialects of Greece and Italy, could express himself thus about the philological value of modern dialects, there is little wonder that educated Englishmen who have devoted no attention to the scientific study of languages should have such vague notions of what a dialect really is. Most educated people seem to think that the lower classes have been endowed by nature with imperfect organs of speech, and that they are incapable of speaking even their own dialect with anything like system and consistency. In the course of the lecture Dr. Wright hoped to show that in reality it is the literary language which is full of irregularities, anomalies, and inconsistencies, and that there is a wonderful uniformity and regularity in the sound-system and grammar of the modern dialects. Before entering upon the subject proper, he discussed briefly the respective merits of modern dialects and literary languages in the study of the science of language in general and of comparative grammar and phonetics in particular. When a man wishes to become a comparative philologist the first thing he does is to learn a number of languages which belong to the same family, e.g., Sanskrit, Greek, Latin, Old Bulgarian, Old Irish, Gothic, etc. After he has acquired a practical knowledge

of such languages, he then begins to learn the relations in which they stand to each other, and to formulate the sound-laws which help to establish their relationship. By studying ancient languages in this manner philologists have been able to reconstruct more or less accurately the parent language of the Indo-Germanic people. It is only *more* or *less* accurately, because we possess but scanty records for ascertaining very accurately the precise pronunciation of each of the letters used in these languages. In other words, our philological knowledge of ancient languages is for the most part based upon *letter*-change instead of *sound*-change.

There are many people who have acquired a considerable book-knowledge of comparative philology, but who cannot be said to know much about the real science of language, through their having failed to learn to distinguish between *letters* and *sounds*. In a literary language it often happens that the same letter is used to represent several different sounds, as the letter *o* in literary English: *women*, *pot*, *north*, *no*, *bosom*, *do*, *love*, *world*. Or the same sound is represented by several different letters, as in literary English: *he*, *feel*, *dream*, *seize*, *people*, *thief*, *machine*.

It is true that literary English orthography is more defective in this respect than almost any other modern literary language, but it has been quoted as showing how important it is to learn to distinguish between sounds and the symbols used to represent sounds. In short, whoever studies the older periods of languages for philological purposes cannot expect to acquire the *precise* pronunciation of the words, owing to the deficiencies of the orthography in which they are written.

With the scientific study of modern dialects the case is entirely different; the investigator is not hampered with a traditional orthography which is often many generations behind the spoken language. He employs a strictly phonetic alphabet, in which the same symbol always represents the same sound, and the same sound is always represented by the same symbol. By setting to work in this manner it is possible for him to gain a clearer insight into the life and growth of language than by the study of any number of ancient languages.

Grammar.—Having said so much about the importance of dialect studies, the lecturer then stated and illustrated a few of the many phonological points upon which the dialects throw light in the history and development of the literary language. In the accidence of the dialects the points to which special attention was drawn, were the formation of the possessive case of nouns and pronouns; the verbal endings; and the great variety of ways in which the nominative case of the personal pronouns is expressed in the dialects. It was shown that many dialects have four forms to express *I*, and that these forms are never mixed up syntactically. It was also shown that the dialects have nine ways of expressing literary English *this*, twelve ways of ex-

pressing *that*, seven ways of expressing *these*, and thirteen ways of expressing *those*. Attention was also drawn to the peculiar manner of expressing the perfect tense in some dialects.

Lexicography.—According to A. D'Orsay, in "The Study of the English Language," p. 15, the common rustic uses as a rule scarcely more than 300 words. This is a gross mis-statement of facts, but it serves as an excellent example of how little educated people generally know about the vocabulary of the working classes in country places. If we take the largest modern English dictionary and exclude from it all literary words not found in books written between the years 1700–1900, and also exclude from it all scientific and foreign words introduced since 1800, it will be found that the total vocabulary of the Scottish and English dialects is considerably greater than the sum total of all the literary words that have been in use during the last two hundred years. The letters A–C of the English Dialect Dictionary contain 17,519 words, and it may be safely inferred that the six volumes of the Dictionary contain at least 100,000 words.

It often happens that the dialects have preserved old genuine forms of a word, which have disappeared from the literary language, e.g. *alablaster*, *kindom*, *kittle*, *apricock*, *crowner*, etc. At first sight one might think that it was the dialects which have corrupted the literary English forms, but such is not the case as is shown by the history of the words. It would be possible to collect from the English Dialect Dictionary hundreds of similar examples. The modern dialects have often preserved words which have disappeared from the literary language for at least a thousand years, and if it were not for the modern dialects we should not know what some of these words meant, even in Anglo-Saxon. Take, for example, the word *Crundel*, in Sussex and Hampshire. In these counties the word means "a ravine, a strip of covert dividing open country, always in a dip and usually with running water in the middle"; what is called in Scotland and the North of England a *gill*. In the Codex Diplomaticus, edited by Kemble, over sixty *Crundels* are mentioned, but the meaning of the word has always remained a puzzle to Anglo-Saxon scholars until the word was found in the modern dialects. In Sweet's Anglo-Saxon Dictionary it is defined as "a cavity," and, with a query, "a chalkpit," "a pond." In Bosworth-Toller's Anglo-Saxon Dictionary it is defined as "a barrow, a mound over graves to protect them." In Leo's Angelsächsisches Glossar it is defined as "a spring or well." And Kemble defines it as "a sort of watercourse, a meadow through which a stream flows." Another interesting word is *Tallet*, "a hay-loft, especially one over a stable; the unceiled space beneath the roof in any building; an attic." It is used in Cheshire, Staffordshire, Derbyshire, Warwickshire, Worcestershire, and all the West Midland and South Western counties. It is an early Celtic loanword from Latin *tabulatum*, "a floor, flooring, storey"; it occurs in old Irish as *taibled*, "a storey," and in North Welsh *taflod*, South Welsh *towlod*,

"a hayloft." Although this word has been in daily use for hundreds of years in eighteen English counties, it is not found in English literature until it was used by such writers as Hardy, Blackmore, and Baring Gould.

In the South Midland and Southern counties, there is a large number of old French words preserved in the dialects, which are not found in any period of English literature. And many of these words are now obsolete in French literature, and are only to be found in modern French dialects. On the other hand, in Northumberland, Cumberland, Westmorland, Yorkshire, Lancashire, Lincolnshire, and East Anglia, there are thousands of Scandinavian words which have been in common use for the last 800 years in these counties, but comparatively few of them have ever been used in English literature. In this connection some of the tests were indicated which enable philologists to ascertain whether a word is of Scandinavian or Anglo-Saxon origin. The dialect words themselves often indicate whether the area over which they are used is Scandinavian or genuine English. Thus, in those dialects where the Scandinavian element is strong, we find *ling*, *beck*, *lop*, *addle*, etc., used, but in the other parts of England the corresponding synonyms are used just as in the literary language, as *heather*, *burn*, *flea*, *earn*.

Ethnology.—If the British Association or any other learned body ever undertakes an ethnological survey of the United Kingdom, it will be found that the dialects will yield most valuable material for the purpose. Even if we possessed no traditions or historical records of the past, we should be able to show from the dialects alone that in Kent, the Isle of Wight, and Hampshire, there was once a large number of Frisians; that Wexford had many settlers from the English South-Western counties; that there had been a large influx of the Scotch into Ulster; that many Huguenots had settled in Norfolk; that there was once a Flemish colony in Monmouthshire and Glamorganshire; that far more Normans settled in the South Midland and Southern counties than in the rest of England; that the Scandinavian settlers in Lincolnshire and East Anglia were to a great extent Danes; and that the Scandinavian settlers in Northumberland, Durham, Cumberland, Westmorland, Yorkshire, and Lancashire were chiefly Norwegians.

Furthermore, if we exclude those districts of the United Kingdom bordering on the parts where a Celtic language or dialect is still spoken, or was spoken until comparatively recent times, as in Cornwall, it is a remarkable fact that, apart from proper names, there is not a score of Celtic words to be found in the modern English dialects.

Again, the dialects show conclusively that the political and linguistic boundaries of counties seldom coincide with each other; e.g., parts of Berkshire and Gloucestershire belong linguistically to Wessex; North Northumberland and North Cumberland to Scotland;

parts of West and South-West Yorkshire to Lancashire; and so on for many other parts of the country.

The above are only a few of the points in which the dialects furnish us with valuable material. There was no time left to touch upon the wealth of material to be found dealing with folk-lore of all kinds, customs, games, religious superstitions, and local traditions.

In conclusion, it may be added that the scientific study of dialects not only advances knowledge, but it brings us face to face with the life and character of the British workman.

[J. W.]

GENERAL MONTHLY MEETING,

Monday, April 3, 1905.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer, and Vice-President, in the Chair.

Joseph Joel Duveen, Esq.
Mrs. Green-Thompson,
Philip Hall, Esq.
Thomas Wright Hall, M.D.
Robert Liveing, M.D.
James Mark McDonnell, Esq.
Charles Mitchell, Esq.
J. Horace Reeves, Esq. M.A.
J. J. H. Teall, Esq. M.A. Sc.D. F.R.S.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same viz.:—

FROM

The Secretary of State for India—Kodaikanal Observatory Bulletin, No. 1. 4to. 1905.
Geological Survey of India—
Records, Vol. XXXI. Part 4; Vol. XXXII. Part 1. 8vo. 1904-1905.
Report of the Board of Scientific Advice, 1901–4. 4to. 1905.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta. Classe di Scienze Fisiche, Vol. XIV. 1° Semestre, Fasc. 4–5. 8vo. 1905.
Agricultural Society of England, Royal—Journal, Vol. LXV. 8vo. 1904
American Academy of Arts and Sciences—Proceedings, Vol. XL. Nos. 12–14. 8vo. 1905.
American Geographical Society—Bulletin, Vol. XXXVII. No. 2. 8vo. 1905.
American Philosophical Society—Transactions, N.S., Vols. XVII. Part 3; XVIII. XIX. XX. and XXI. Part 1. 4to. 1905. 8vo. 1905.
Proceedings, Vol. XLIII. No. 178.

Astronomical Society, Royal—Monthly Notices, Vol. LXV. No. 4. 8vo. 1905.
Automobile Club—Journal for March, 1905. 4to.
Bankers, Institute of—Journal, Vol. XXVI. Part 3. 8vo. 1905.
Boston Public Library—Monthly Bulletin for March, 1905. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XII. Nos. 9–10. 4to. 1905.
British Astronomical Association—Journal, Vol. XV. No. 5. 8vo. 1905.
Caton, Richard, M.D. (the Author)—The Harveian Oration, 1904. I. I-em-hotep and Ancient Egyptian Medicine. II. Prevention of Valvular Disease. 8vo. 1904.
Chemical Industry, Society of—Journal, Vol. XXIV. No. 5. 8vo. 1905.
List of Members, 1905. 8vo.
Chemical Society—Journal for March, 1905. 8vo.
Proceedings, Vol. XXI. Nos. 291–292. 8vo. 1905.
Chicago, Field Columbian Museum—Publications, Geological Series, Vol. II. No. 6; Zoological Series, Vol. V. 8vo. 1904.
Church, Professor A. H., F.R.S. M.R.I.—General Description of Sir John Soane's Museum. 8vo. 1905.
Diplock, B. J. (the Author)—The Invention and Utility of the Pedrail. 8vo. 1905.
Editors—American Journal of Science for March, 1905. 8vo.
Analyst for March, 1905. 8vo.
Astrophysical Journal for March, 1905. 8vo.
Athenæum for March, 1905. 4to.
Board of Trade Journal for March, 1905. 8vo.
Brewers' Journal for March, 1905. 8vo.
Cambridge Appointments Gazette, No. 18. 4to. 1905.
Chemical News for March, 1905. 8vo.
Chemist and Druggist for March, 1905. 8vo.
Dioptric Review for March, 1905. 8vo.
Electrical Engineer for March, 1905. fol.
Electrical Review for March, 1905. fol.
Electrical Times for March, 1905. 4to.
Electricity for March, 1905. 8vo.
Engineer for March, 1905. fol.
Engineering for March, 1905. fol.
Engineering Review for March, 1905. 8vo.
Horological Journal for April, 1905. 8vo.
Journal of State Medicine for March, 1905. 8vo.
Law Journal for March, 1905. 8vo.
London Education Gazette for March, 1905. fol.
London University Gazette for March, 1905. fol.
Machinery Market for March, 1905. 8vo.
Model Engineer for March, 1905. 8vo.
Motor Car Journal for March, 1905. 8vo.
Musical Times for March, 1905. 8vo.
Nature for March, 1905. 4to.
Nuovo Cimento for Feb. 1905. 8vo.
Page's Weekly for March, 1905. 8vo.
Photographic News for March, 1905. 8vo.
Public Health Engineer for March, 1905. 8vo.
Science Abstracts for March, 1905. 8vo,
Zoophilist for March, 1905. 4to.
Electrical Engineers, Institution of—Journal, Vol. XXXIV. Part 1. 8vo. 1905.
Florence, Biblioteca Nazionale—Bulletin, March, 1905. 8vo.
Franklin Institute—Journal, Vol. CLIX. No. 3. 8vo. 1905.
Geological Society—Abstracts, No. 808–809. 8vo. 1905.
Journal, Vol. LXI. No. 1. 8vo. 1905.
Göttingen, Academy of Sciences—Nachrichten, 1904, Mathematisch-Physikalische Klasse, Heft 6; Geschäftliche Mittheilungen, Heft 2. 8vo.

Harvard College Observatory—59th Annual Report. 8vo. 1904.
Plan for the Endowment of Astronomical Research. By E. C. Pickering. No. 2. 8vo. 1904.
The Astronomical Observatory of Harvard College. 8vo. 1905.
Johns Hopkins University—American Journal of Philology, Vol. XXV. No. 4. 8vo. 1904.
Literature, Royal Society of—Transactions, Second Series, Vol. XXVI. Part 1. 8vo. 1905.
Liverpool School of Tropical Medicine—Memoirs, V. Part 2; IX. and Miscellaneous II. 8vo. 1901–3; XII. XIII. XIV. 4to. 1904-5.
Trypanosome occurring in the Blood of Man. By J. E. Dutton. 4to. 1905.
Maudsley, Henry, M.D. LL.D. F.R.C.P. M.R.I. (the Author)—Shakspeare: "Testimonied in his own Bringingsforth." 8vo. 1905.
Meteorological Society, Royal—Journal, Vol. XXXI. No. 133. 8vo. 1905.
Mexico, Geological Institute—Parergones, Tomo I. No. 6. 8vo. 1904.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias y Revista, Tomo XIX. Nos. 11–12; XX. Nos. 11–12. 8vo. 1903–4.
Munich, Royal Academy of Sciences—Sitzungsberichte, 1904, Heft 3. 8vo. 1905.
Navy League—Journal for March, 1905. 8vo.
New Jersey Geological Survey—Vol. VI. 1904. 8vo.
Pharmaceutical Society of Great Britain—Journal for March, 1905. 8vo.
Photographic Society, Royal—Journal, Vol. XLV. No. 3. 8vo. 1905.
Robson, A. Mayo, Esq. F.R.C.S. M.R.I. (the Author)—Diseases of the Stomach. 8vo. 1904.
Royal Irish Academy—Proceedings, Vol. XXV. Section B, Nos. 1–2; Section C, No. 8. 8vo. 1905.
Royal Society of Edinburgh—Proceedings, Vol. XXV. No. 6. 8vo. 1905.
Royal Society of London—Philosophical Transactions, B, Nos. 236–237. 4to. 1905.
Year Book, 1905. 8vo.
Proceedings, No. 505. 8vo. 1905.
Smithsonian Institution—Miscellaneous Collections, Vol. XLVI. Nos. 1543–1544; Quarterly Issue, Vol. II. No. 2. 8vo. 1904.
Society of Arts—Journal for March, 1905. 8vo.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXXIV. Disp. 2. 4to. 1905.
United Service Institution, Royal—Journal for March, 1905. 8vo.
United States Department of Agriculture—Monthly Weather Review for Dec. 1904. 8vo.
Experiment Station Record for Jan. 1905. 8vo.
United States Patent Office—Official Gazette, Vol. CXIV. No. 9; Vol. CXV. Nos. 1–3. 4to. 1905.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1905. Heft 3. 4to.
Washington Academy of Sciences—Proceedings, Vol. VI. pp. 429–481. 8vo. 1905.
Weights and Measures, Superintendent of—Memorandum on the Construction and Verification of a new Copy of the Imperial Standard Yard, Part 1. 4to. 1905.
Western Australia, Agent-General—Geological Survey, Bulletin, Nos. 2–13, 15, 8vo. 1898–1904.
Monthly Statistical Abstract for Jan. 1905. 4to.

WEEKLY EVENING MEETING,

Friday, April 7, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Alfred Mosely, Esq., C.M.G.

American Industry.

Mr. Mosely gave an address upon the Mosely Industrial Commission to the U.S.A., and the lessons to be learned therefrom.

He spoke extempore, and commenced by giving his reasons for the commission, stating that, in his opinion, neither workers nor employers had quite realised the strength of the competition that was growing up in the United States and Germany, and emphasising the necessity of bringing home to the workers especially the dangers of the situation and the urgent need of acting upon the advice of the Prince of Wales to "wake up."

As one who had travelled considerably, he was convinced that the time had arrived for the reconsideration of our position from every standpoint—military, educational, and economic; that the old characteristics that had placed us in the front rank in time past—pluck, perseverance, honesty, and energy—were not now sufficient alone to enable us to maintain that position, but that modern scientific methods would have to be adopted, if we were to hold our place; and we must be prepared to drop preconceived notions, fiscal and otherwise, and to face the new conditions that present themselves to-day, apart from dogmas and prejudices of the past. In other words, we must be ready to meet present competition by, if necessary, altering our fiscal policy to meet the new conditions.

Mr. Mosely stated distinctly that, whilst strongly in favour of trade unions, he was not in favour of all that trade unionism did; but he was quite prepared to review the case in a broad-minded spirit, and weigh the good against the bad points of unionism, and to acknowledge the many benefits it had conferred on workers and the community at large, as well as condemn its many errors. Viewing it as a whole, he felt bound to say that those advantages largely outweighed the mistakes, and he considered that a heavy responsibility rested upon employers to help to guide the unions by frequent "round-table" conferences, for the purpose of discussing the many problems vital alike to capital and labour.

Mr. Mosely next referred at some length to the American system of education, and the beneficial effect that it was, in his opinion, exerting on trade and industry in the United States and on all questions affecting the uplifting of the masses. In that country he found an intense belief in education, from the highest to the lowest. Children remained longer at school than here—parents willingly making the sacrifice—and a thirst for knowledge was shown by the scholar that had no counterpart in this country. The child first passed through the American public school, thence to the college—equivalent to our primary and secondary schools respectively—and if he desired it, he could afterwards pass to the University, practically free of cost. With such opportunities for the masses it was little wonder that amongst those eighty millions of educated people there should be ample brains developed and available, and the result was shown in the unmistakeable leaps and bounds by which the country was advancing. The lecturer also thought the political situation in the States should teach us a lesson, inasmuch as upon all national questions affecting the people's welfare, the electors to a large extent spoke with one voice, putting mere party politics aside. The striking majority by which President Roosevelt was returned was a proof of this trait, and by subsequent events that judgment had been fully justified.

One of the main questions placed before the trade union delegates was: "How is it that America can afford to pay the workmen half-a-dollar (and often more) in wages, where we pay but a shilling, and yet compete with us in the markets of the world?" Mr. Mosely went on to sketch how, in American workshops, everything is systematised, standardised and specialised; how labour-saving machinery predominates; how employers welcome large earnings on the part of their workmen by means of piecework, rather than trying to fix a standard maximum wage for skilled labour, and then cutting down prices if that maximum be exceeded.

The lecturer enlarged upon the many drawbacks of the British system, under which manufacturers do not sufficiently specialise; new methods and ideas are so often opposed by the workpeople, and the employer has to struggle under the burden of heavy taxes, heavy rating of machinery, and a market always open to attack by the dumping here of surplus products from every other country in the world. He laid down that the time had come for a thorough investigation into the industrial position, as the problems present themselves to-day, and begged that all concerned would view these important problems entirely apart from politics, and solely on their own merits.

A series of interesting lantern slides were then exhibited, illustrating the trip taken by the Mosely commissioners through the United States, where they visited the principal industrial centres, such as New York, Albany, Schenectady, Niagara, Buffalo, Cleveland, Chicago, Dayton, Pittsburg, Washington, Philadelphia, etc. Many

of the delegates took additional trips to investigate their own special industries; but the majority returned to New York, there separating to prosecute inquiries on their own account. The whole trip was, it is believed, highly instructive, and much appreciated by the twenty-three delegates comprising the commission and representing the staple industries of the country. The results of their investigations were recorded in an interesting volume, published two years ago.

[A. M.]

WEEKLY EVENING MEETING.

Friday, April 14, 1905.

Sir William Crookes, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. Sc.D. F.R.S. *M.R.I.*

The Law of Pressure of Gases below Atmosphere.

[No Abstract.]

ANNUAL MEETING,

Monday, May 1, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1905, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Seventy-one new Members were elected in 1904.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1904.

The Books and Pamphlets presented in 1904 amounted to about 267 volumes, making, with 721 volumes (including Periodicals bound) purchased by the Managers, a total of 988 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. D.C.L. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.

SECRETARY—Sir William Crookes, D.Sc. F.R.S.

MANAGERS.

Sir William de W. Abney, K.C.B. D.C.L. D.Sc. F.R.S.
The Right Hon. Lord Alverstone, G.C.M.G. M.A. LL.D. F.R.S.
Henry E. Armstrong, Esq., Ph.D. LL.D. F.R.S.
Shelford Bidwell, Esq., M.A. D.Sc. LL.B. F.R.S.
Sir Alexander Binnie, M.Inst.C.E.
The Hon. Sir Henry Burton Buckley, M.A.
The Right Hon. Charles Scott Dickson, K.C. M.P. M.A. LL.D.
Francis Elgar, Esq., LL.D. F.R.S. M.Inst.C.E.
Maures Horner, Esq., J.P. F.R.A.S.
Ludwig Mond, Esq., Ph.D. F.R.S.
Sir Andrew Noble, Bart., K.C.B. D.Sc. F.R.S.
The Right Hon. The Earl of Rosse, K.P. D.C.L. LL.D. D.Sc. F.R.S.
Sir Thomas Henry Sanderson, G.C.B. K.C.M.G.

MANAGERS—*continued.*

Alexander Siemens, Esq., M.Inst.C.E.
Silvanus P. Thompson, Esq., B.A. D.Sc. F.R.S.

VISITORS.

William Arthur Brailey, M.D. M.A. M.R.C.S.
John Mitchell Bruce, Esq., M.A. M.D. LL.D.
Sir John George Craggs, M.V.O.
James Mackenzie Davidson, Esq., M.B. C.M.
Francis Fox, Esq., M.Inst.C.E.
Lord Greenock, D.L. J.P.
Charles Edward Groves, Esq., F.R.S.
A. Kirkman Loyd, Esq., M.P. K.C.
Sir Philip Magnus, J.P. B.A. B.Sc.
Carl E. Melchers, Esq.
Emile R. Merton, Esq.
George Johnstone Stoney, Esq., M.A. D.Sc. F.R.S.
John Jewell Vezey, Esq., F.R.M.S.
George Philip Willoughby, Esq., J.P.

WEEKLY EVENING MEETING,

Friday, May 5, 1905.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

PROFESSOR HENRY E. ARMSTRONG, Ph.D. LL.D. F.R.S. *M.R.I.*

Problems underlying Nutrition.

[No Abstract.]

GENERAL MONTHLY MEETING.

Monday, May 8, 1995.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S, Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir William de W. Abney, K.C.B. D.C.L. D.Sc. F.R.S.
Shelford Bidwell, Esq., M.A. Sc.D. LL.B. F.R.S.
The Rt. Hon. Lord Alverstone, G.C.M.G. M.A. LL.D. F.R.S.
Ludwig Mond, Esq., Ph.D. D.Sc. F.R.S.
The Rt. Hon. The Earl of Rosse, K.P. D.C.L. LL.D. D.Sc. F.R.S.
Sir Thomas Henry Sanderson, G.C.B. K.C.M.G.
Sir James Crichton-Browne, M.D. LL.D. F.R.S., *Treasurer.*
Sir William Crookes, D.Sc. F.R.S., *Honorary Secretary.*

William Robert Bousfield, Esq., K.C. M.P.
Arthur Thomas Franklin, Esq.
Richard Howlett, Esq., F.S.A.
Daniel Nicholson, Esq., F.R.G.S. F.Z.S.
Professor J. J. Thomson, D.Sc. LL.D. F.R.S.
Major-General Sir Alfred Edward Turner, K.C.B.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Frank Lawson for her donation of £50, and to Sir Thomas H. Sanderson, G.C.B., for his Donation of £5 5*s*., to the Fund for the Promotion of Experimental Research at Low Temperatures; also to Mrs. Barton for her Gift of a Portrait of Thomas Young, M.D. F.R.S. *M.R.I.*

It was announced from the Chair that the Legacy of £2000, bequeathed by the late Dr. Frank McClean, had been received.

The Rt. Hon. Lord Rayleigh, O.M. M.A. D.C.L. LL.D. Sc.D. F.R.S., was elected Honorary Professor of Natural Philosophy, and Professor J. J. Thomson, D.Sc. LL.D. F.R.S., was elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Report on Kodaikanal and Madras Observatories for 1904. 4to. 1905.
Report on Public Instruction in Bengal, 1903–4. 4to. 1904.
The Astronomer Royal—Greenwich Observations, 1902. 4to. 1904.
Photo-heliographic Results. 4to. 1904.
Annals of the Cape Observatory, Vol. XI. Part 2. 4to. 1905.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIV. 1° Semestre, Fasc. 6–8. 8vo. 1905.
American Academy of Arts and Sciences—Proceedings, Vol. XL. Nos. 15–17. 8vo. 1905.
American Geographical Society—Bulletin, Vol. XXXVII. No. 3. 8vo. 1905.
Astronomical Society, Royal—Monthly Notices, Vol. LXV. Nos. 5–6. 1905.
Automobile Club—Journal for April, 1905.
Bankers, Institute of—Journal, Vol. XXVI. Part 5. 8vo. 1905.
Belgium, Royal Academy of Sciences—Bulletin, 1905, Nos. 1–2. 8vo.
Mémoires: Collection in 4to, Tome I. Fasc. 1–2; Collection in 8vo, Tome I. Fasc. 1–3. 1904.
Birmingham and Midland Institute Scientific Society—Meteorological Observations, 1904. 8vo. 1905.
Boston Public Library—Monthly Bulletin for April, 1905. 8vo.
Boston Society of Natural History—Memoirs, Vol. V. Nos. 10–11; Vol. VI. No. 1. 4to. 1903–5.
Proceedings, Vol. XXXI. Nos. 2–10; Vol. XXXII. Nos. 1–2. 8vo. 1903–4.
Occasional Papers, VII.: Fauna of New Zealand, Nos. 1–3. 8vo. 1904.
British Architects, Royal Institute of—Journal, Third Series, Vol. XII. Nos. 11–13. 4to. 1905.
British Astronomical Association—Journal, Vol. XV. No. 6. 8vo. 1905.
Buenos Ayres—Bulletin of Municipal Statistics, Jan. 1905. fol.
Chemical Industry, Society of—Journal, Vol. XXIV. Nos. 6–8. 8vo. 1905.
Chemical Society—Proceedings, Vol. XXI. Nos. 293–294. 8vo. 1905.
Journal for April, 1905. 8vo.
Edinburgh, Royal College of Physicians—Laboratory Reports, Vol. IX. 8vo. 1905.
Editors—Aeronautical Journal for April, 1905. 8vo.
American Journal of Science for April, 1905. 8vo.
Analyst for April, 1905. 8vo.
Astrophysical Journal for April, 1905. 8vo.
Athenæum for April, 1905. 4to.
Author for April–May, 1905. 8vo.
Board of Trade Journal for April, 1905. 8vo.
Brewers' Journal for April, 1905. 8vo.
Chemical News for April, 1905. 4to.
Chemist and Druggist for April, 1905. 8vo.
Electrical Engineer for April, 1905. 4to.
Electrical Review for April, 1905. 4to.

Editors—continued.
Electrical Times for April, 1905. 4to.
Electricity for April, 1905. 8vo.
Engineer for April, 1905. fol.
Engineering for April, 1905. fol.
Homœopathic Review for April-May, 1905. 8vo.
Horological Journal for May, 1905. 8vo.
Journal of the British Dental Association for April, 1905. 8vo.
Journal of State Medicine for April, 1905. 8vo.
Law Journal for April, 1905. 8vo.
London Education Gazette for April, 1905. 4to.
London University Gazette for April, 1905. 4to.
Machinery Market for April, 1905. 8vo.
Model Engineer for April, 1905. 8vo.
Mois Scientifique for March, 1905. 8vo.
Motor Car Journal for April, 1905. 4to.
Musical Times for April, 1905. 8vo.
Nature for April, 1905. 4to.
New Church Magazine for April-May, 1905. 8vo.
Page's Weekly for April, 1905. 8vo.
Photographic News for April, 1905. 8vo.
Physical Review for March, April, May, 1905. 8vo.
Public Health Engineer for April, 1905. 8vo.
Science Abstracts for April, 1905. 8vo.
Terrestrial Magnetism for March, 1905. 8vo.

Electrical Engineers' Institute—Journal, Vol. XXXIV. Part 2. 8vo. 1905.
Florence, Biblioteca Nazionale—Bulletin for April, 1905. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. I. Disp. 4. 8vo. 1904.
Geographical Society, Royal—Journal, Vol. XXV. Nos. 4–5. 8vo. 1905.
Geological Society—Abstracts of Proceedings, Nos. 810–811. 8vo. 1905.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Sér. II. Tome X. Liv. 1–2. 8vo. 1905.
Johns Hopkins University—Studies, Vol. XXII.; Vol. XXIII. Nos. 1–2. 8vo. 1904–5.
Circulars, 1904, Nos. 2, 3, 5, 7, 8; 1905, No. 1. 8vo. 1904–5.
Life-Boat Institution, Royal National—Annual Report for 1904. 8vo. 1905.
Linnean Society—Journal: Zoology, Vol. XXIX. No. 191. 8vo. 1905.
Madrid, Reale Academia de Ciencias—Revista, Tomo II. Num. 1. 8vo. 1905.
Anuario, 1905. 16mo.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. XLIX. Part 2. 8vo. 1905.
Massachusetts Institute of Technology—Technology Quarterly, Vol. XVIII. No. 1. 8vo. 1905.
Mechanical Engineers, Institution of—Proceedings, 1904, No. 3. 8vo.
List of Members. 8vo. 1905.
Microscopical Society, Royal—Journal, 1905, Part 2. 8vo. 1905.
Montana University—Bulletin, 5 Ser. No. 3, Part 2. 8vo. 1905.
National Physical Laboratory—Report for 1904. 8vo. 1905.
Report from the Observatory Department for 1904. 8vo. 1905.
Navy League—Navy League Journal for April–May, 1905. 8vo.
New South Wales, Agent-General—Australia and New Zealand, 1903–4. By T. A. Coghlan. 8vo. 1905.
New Zealand, Registrar-General—Statistics of the Colony for 1903, Vols. I.–II. 4to. 1904.
North of England Institute of Mining Engineers—Transactions, Vol. LIV. Part 7; Vol. LV. Parts 2–3. 8vo. 1905.
Odontological Society—Transactions, Vol. XXXVII. No. 4. 8vo. 1905.

Pennsylvania University—Bulletin, 5 S. No. II. Part 2; No. III. Part 2. 8vo. 1905.
Catalogue, 1904–5. 8vo. 1904.
Peru, Cuerpo de Ingenieros de Minas—Boletin, Nos. 18–19. 8vo. 1904–5.
Pharmaceutical Society of Great Britain—Journal for April, 1905. 8vo.
Philadelphia, Academy of Sciences—Proceedings, Vol. LVI. Part 3. 8vo. 1905.
Photographic Society, Royal—Journal, Vol. LXV. No. 4. 8vo. 1905.
Physical Society of London—Proceedings, Vol. XIX. Part 5. 8vo. 1905.
Quekett Microscopical Club—Journal, Ser. 2, Vol. IX. No. 56. 8vo. 1905.
Recke, F., Esq. (the Author)—Energie und Materie. 8vo. 1904.
Rio de Janeiro Observatory—Bulletin for July–Sept. 1904. 8vo. 1905.
Rome, Ministry of Public Works—Giornale for Aug.–Oct. 1904. 8vo.
Royal Irish Academy—Proceedings, Vol. XXV. Section A, No. 3; Section B, Nos. 3, 5; Section C, No. 9. 8vo. 1905.
Royal Society of Edinburgh—Proceedings, Vol. XXV. No. 7. 8vo. 1905.
Royal Society of London—Philosophical Transactions, A, Nos. 380–381. 4to. 1904.
Proceedings, No. 506; Series A, No. 507; Series B, No. 507. 8vo. 1905.
Reports to the Evolution Committee, II. 8vo. 1905.
Reports of the Mediterranean Fever Commission, Parts 1–2. 8vo. 1905.
Saltarel, P. M., Esq.—Four Japanese Illustrated Books on Korea. 8vo. (1905.)
Sanitary Institute, Royal—Journal, Vol. XXVI. Parts 1–4 and Supplement. 8vo. 1905.
Saxon Society of Sciences, Royal—Preisschriften, No. XXXVII. 8vo. 1905.
Scottish Meteorological Society—Journal, Third Series, Nos. XX.–XXI. 8vo. 1905.
Selborne Society—Nature Notes for April–May, 1905. 8vo.
Smith, B. Leigh, Esq. M.R.I.—The Scottish Geographical Magazine, Vol. XXI. Nos. 4–5. 8vo. 1905.
Society of Arts—Journal for April, 1905. 8vo.
Statistical Society, Royal—Journal, Vol. LXVIII. Part 1. 8vo. 1905.
Stonyhurst College Observatory—Results of Meteorological Observations, 904. 8vo. 1905.
Tacchini, Prof. P. Hon.Mem.R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXXIV. Disp. 3. 4to. 1905.
Toronto University—Studies: Geological Series, No. 3; Psychological Series, Vol. II. No. 2; Papers from Chemical Laboratories, Nos. 48–49. 8vo. 1904–5.
United Service Institution, Royal—Journal for April, 1905. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XVI. Nos. 6–7. 8vo. 1905.
United States Patent Office—Official Gazette, Vol. CXIV. Nos. 4–8. 8vo. 1905.
Upsala University—Bulletin de l'Observatoire Météorologique, Vol. XXXVI. 4to. 1905.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen 1905, Heft 4. 4to.
Vienna Imperial Geological Institute—Verhandlungen, 1905, Nos. 1–2.
Western Australia, Agent-General—Monthly Statistical Abstract, Feb. 1905. 4to.
Western Society of Engineers—Journal, Vol. X. No. 1. 8vo. 1905.
Wisconsin Academy—Transactions, Vol. XIV. Part 2. 8vo. 1904.
Zoological Society of London—Report for 1904. 8vo. 1905.
Proceedings, 1904, Vol. II. Part 2. 8vo. 1905.

WEEKLY EVENING MEETING,

Friday, May 12, 1905.

The Earl of Rosse, K.P. D.C.L. LL.D. D.Sc. F.R.S., Vice-President, in the Chair.

Professor Ernest Fox Nichols.
Columbia University, New York.

The Pressure due to Radiation.

[abstract.]

The first speculations upon a possible pressure due to radiation were suggested by the behaviour of comet tails.

Early in the sixteenth century, Pierre Apian announced that the tails of comets were always directed away from the sun, and a century later Kepler maintained that this repulsion was due to the pressure of sunlight. On the corpuscular theory of light it seemed plausible that the finely divided and very attenuated matter supposed to constitute comet tails, might experience a repulsion of which ordinary bodies gave no evidence.

The three intervening centuries from Kepler's time to our own, exhibit a long and very interesting record of conflicting opinions, and the account of many curious and inconclusive experiments.

In 1900–1901 the first experiments giving undoubted evidence of the existence of a pressure due to radiation were announced independently from Moscow by Professor Lebedew, and from New Hampshire by Nichols and Hull.

Maxwell had earlier maintained that radiation pressure was a necessary consequence of the Faraday-Maxwell electromagnetic theory of light, and after him, Bartoli was convinced that the same result should follow from the laws of thermodynamics. In computing the ratio of the pressure to the intensity of the radiation producing it, Maxwell and Bartoli were in exact agreement.

In the experiments of Nichols and Hull in which both the pressure and the intensity of a beam of light were measured, the ratio was found to agree with the Maxwell-Bartoli theory to within one part in a hundred. As the limit of accuracy of the observations was of this same order, the experimental verifications of the Maxwell-Bartoli theory may be accepted as complete.

Nichols's and Hull's experiments were described by the lecturer,

and the pressure exerted by a concentrated beam from an arc electric lamp on one vane of a delicately suspended torsion balance, was shown to the audience; and likewise the character of the disturbing action due to the gases present in the balance chamber.

Lebedew and Nichols and Hull succeeded in detecting radiation pressure, only because they were the first to systematically eliminate the disturbing forces due to the residual gases. Thus the cause of failure of the well directed efforts of earlier observers to isolate radiation pressure from the relatively powerful and uncertain gas forces exemplified in the Crookes radiometer was made clear.

A vacuum tube built by Nichols and Hull to illustrate the repulsion of comet tails by the sun was also shown. The form of the tube was that of an hour-glass. A very fine dust, prepared by calcining puff-ball spores, was mixed with the sand, and the pressure of a very powerful beam of light directed horizontally against the stream, just below the neck, drove the finer dust particles backward, while the heavier sand grains fell vertically.

The verification of the radiation pressure theory affords a means of extending our knowledge of many celestial phenomena and of broadening our theories concerning them. In the first place, the hitherto mysterious behaviour of comet tails is satisfactorily explained. The Newtonian gravitation theory is seen no longer to express the whole mutual action between bodies, for if either or both of the bodies be at a temperature above that of their surroundings, a correction must be added to include the radiation pressure between them. If the bodies are massive, and not too hot, the correction is insignificant; but Professor Poynting has shown that for two spherical block bodies of unit density 8 inches in diameter, if at a temperature of 30° Centigrade, the radiation pressure would just balance gravitational attraction, and the bodies would be entirely indifferent to each other, however near or far apart they might be.

A law of action between two bodies, alone in space, which takes account of both gravitation and radiation pressure is—

$$F = - \frac{\pi^2 a^2 b^2}{r^2}\left(\gamma \frac{\rho_1 \rho_2 a b}{16} - \sigma (T_1^4 + T_2^4)\right)$$

in which F is the force between two spheres of radii, a and b, of densities ρ_1 and ρ_2, r units distance apart, and at temperatures T_1 and T_2 absolute; γ is the gravitation constant, and σ is four times the radiation pressure between two spheres of unit radius and at two units distance apart when $T_1 = 0^0$ and $T_2 = 1°$, or the reverse.

Hereafter, therefore, in dealing with flocks of small stones or meteorites in space, and in computing the forces of condensation in a heated nebula, radiation pressure must be taken into the account.

Furthermore, radiation pressure is reciprocal. As a body which stops radiation feels a pressure, so also a body which is sending out

radiation in a given direction receives a backward pressure from its own emission. Professor Poynting has shown that when a hot body radiating equally in all directions is in motion, the intensity of its radiation will, in accordance with Döppler's principle, be slightly greater in front than behind ; hence the radiation pressures fore and aft will no longer balance, and there will be a resultant pressure tending to retard the body's motion.

If such a body should retain its temperature long enough it must inevitably come to rest, and not keep moving on for ever as we have previously believed.

[E. F. N.]

WEEKLY EVENING MEETING,

Friday, May 19, 1905.

THE EARL OF ROSSE, K.P. D.C.L. LL.D. D.Sc. F.R.S.,
Vice-President, in the Chair.

SIR CHARLES ELIOT, K.C.M.G., lately H.M. Commissioner for the Protectorate.

The Native Races of the British East Africa Protectorate.

MY object tonight is to attempt to explain the present distribution of races in East Africa, and to touch on the far more difficult problem of how that distribution has arisen. You are doubtless aware that both the population and geography of this region are very different from those of West Africa. Its chief physical feature is that at a short distance from the coast is a belt of high grassy plateaus, rising to an altitude of five to ten thousand feet and descending in the west to the great equatorial lakes and the forests beyond them. But on the eastern side, the coast is divided from the good lands of the interior by a belt of jungle which has been pierced only by the Uganda railway. Hence, hitherto, the migrations of races and the transmission of foreign influence have tended to take a northerly or southerly direction, while there has been hardly any connection between the east and west; between the coast, for instance, and lake Victoria.

Our East African territories are the meeting place of many races. They have been carved out in obedience to considerations of politics, not of geography or ethnography, and hence they contain not only a dense and fairly uniform population round lake Victoria, but also the ends and margins of many surrounding districts with the most various inhabitants, such as the edge of the Congo territories, the end of the Sudanese swamps, the southern extremity of Somaliland, and portions of tribes who extend into Abyssinia. The whole country has little or no connection with the Negroes of West Africa. Its inhabitants may be succinctly described as a substratum of Bantu population, thick in the low parts but sparse in the high cool districts, which has been invaded from the north by the Somalis and Gallas, and from the north-west by tribes of somewhat disputable affinities but closely allied to one another, and including the Masai, Nandi, Turkana, and other less known names. It might possibly be argued that the Bantus are the invaders and that the other tribes represent the older inhabitants, but though we have very little in the way of history to guide us, everything indicates that the other theory is more probable.

Perhaps, before proceeding to a more detailed description, it will

not be amiss to make some general definitions in African ethnology, and inquire what are the great divisions under which the natives of the continent may be classified.

Whatever system of classification we may adopt for the population of Africa, all authorities would, I think, admit the existence of three well defined groups: the Hamites, the Negroes, strictly so-called, and the Bantus. This enumeration makes no pretence of being complete. On the contrary, it ignores several important tribes who are probably hybrids, and some who possibly represent independent stocks. But, still, the easiest way to give a clear preliminary statement of African ethnology, with a view to approaching the more difficult problems, is to describe these three groups.

The name Hamite, or Hamitic, is primarily applied to a group of languages of which ancient Egyptian is the most conspicuous representative. More or less distinctly related to ancient Egyptian are two other groups, the eastern and western Hamitic languages. In the west, we find the Berber or Kabyl dialects spoken in Morocco, and Tamashek spoken in the Northern Sahara. In the east, the most important languages are Somali and Galla, but there are also a number of less known tongues spoken near the Red Sea, such as Afar or Danakil. The speakers of these languages are as a rule easily differentiated from the other natives of Africa. They are in no sense Negroes, and the superficial observer is more likely to confound them with Arabs. In physique, they are mostly well-built, slim and tall, and dark brown rather than black in colour. They have a tendency, though not without conspicuous exceptions, such as the ancient Egyptians, to prefer a semi-nomadic life and cattle-herding to a settled existence in towns and agriculture. They are also inclined to split up into independent tribes with democratic institutions, and few of them, again with the exception of the ancient Egyptians, have developed anything like a state or a kingdom.

These tribes belong to Northern Africa, and have evidently no taste for the forests of the interior. In the west, they hardly pass beyond the Senegal river—that is about 15° north of the Equator—but in the east they have penetrated slightly to the south of the Equator itself. But there they are clearly the extreme outposts of populations whose natural head-quarters are in the north, and who have extended much further to the south on the east coast than on the west on account of the congenial nature of the country and the paucity of the inhabitants.

The second of the three classes of African natives which I have mentioned above is formed by the Negroes. This is a physical, not a linguistic group. We all know the chief characters of the Negro physique. They are people with black skins, woolly hair, flat noses, thick lips, and, though their general muscular strength is enormous, the calves of their legs are curiously undeveloped. The country which they inhabit may be very roughly defined as Africa north of

the Cameroons and west of the Nile, and they are usually present in considerable numbers, either as slaves or inhabitants, in the countries occupied by Arabs and Hamites. The Negro area is thus very large, comprising the Sudan and the great hump of Africa projecting into the Indian Ocean, but it will perhaps surprise some of my hearers to be told that there are very few pure Negro tribes in East Africa.

Linguistically, the Negroes do not form a unit, but speak the most diverse tongues which are probably to be numbered by hundreds. Few of them have been adequately studied by Europeans, and much research will be necessary before it will be possible to pronounce an opinion on their relations and classification. But the interesting point is, that whereas there is this babel of tongues in North-West Africa and considerable diversity in North-East Africa, there is almost complete linguistic uniformity south of the Equator, or, to be more accurate, south of a line running from the Cameroons along the Uelle river to lakes Albert and Victoria and then through the East Africa Protectorate. This surprising phenomenon is connected with the third of the three groups with which I started, namely the Bantus. Unlike the word Negro, this name refers primarily to language, not to physique, but though it would be most incorrect to say that all the people who speak the Bantu languages belong to one physical type, still there is found among them in many parts a type which is different from that of the Negroes and superior to it. Conspicuous instances of such a type are the Zulus and Kaffirs of South Africa.

But the most interesting point about the Bantus is the distribution of their languages over so wide an area—a phenomenon remarkable not only in Africa but in the whole world. As far as is known, with the exception of the languages of the Hottentots, Bushmen, and one tribe in German East Africa, all the natives of the southern half of the continent, from Cape Colony to the East Africa Protectorate and Uganda, speak languages which belong to one family, and exhibit less difference than do the various Aryan languages among themselves, though of course they are not mutually intelligible. There is hardly anything which can be compared to this linguistic area, if we except the diffusion of some European languages, such as English and Spanish, which has resulted from the colonisation of America and Australia in the last few centuries. The nearest parallel perhaps—though it is far from being an exact one—is the extraordinary diffusion of the Malay languages ; and the contrast with the multitudinous variety of Negro idioms in West Africa makes the phenomenon doubly remarkable. It would seem, however, that Africans abandon their languages very readily, and make little attempt to resist the encroachments of a vigorous foreign tongue. The Negroes in North America have entirely forgotten their African speech, and if it be thought that the exceptional strength of the influences brought to bear on them there, make this a hardly fair example, I would cite the case of Madagascar. The population of that large island, which measures nearly two

hundred and thirty thousand square miles, contains a large African element and several physical types of mankind. Yet authorities agree in saying that a single Malay language, with no differences greater than dialects, is spoken over the whole area, and it would seem that this was originally the language of a relatively small body of invaders. We may, therefore, conjecture that some strong influence must have been at work in southern and central Africa, which has succeeded in imposing such linguistic uniformity.

If we could tell what that influence was, we should be in a fair way to settle some of the most difficult questions of African ethnology and history, but we have as yet little but theories to guide us. But I think that in considering this mystery we should bear in mind that South Africa contains another mystery which has lately attracted considerable attention. This is, the existence in the country between the Limpopo and the Zambesi of a considerable number of ancient buildings, of which the ruins called Great Zimbabwe are the best known. The most recent authorities state that there are not less than three hundred distinct groups of ruins in Rhodesia, and there are probably more in Portuguese territory. These ruins are of different dates, and much obscurity still involves the whole subject; but it is quite clear that there must have existed in this district during many hundred years, and perhaps much longer, a forgotten civilisation far superior to anything else that we know of in Africa south of the Equator—a civilisation which was at first foreign, and afterwards, as it would seem, assimilated by some African race. It appears to me that there is a strong antecedent probability that there is a connection between this civilisation and the diffusion of the Bantu languages, and perhaps further researches in this quarter may throw some light on their origin. The main objections to seeking for that origin near the Zambesi are, that some authorities hold that the most archaic forms of the Bantu languages are those spoken in Uganda and near lake Victoria, and that many Bantu tribes have a tradition that they came from the north. The linguistic evidence as to the relative antiquity of these languages does not seem to me conclusive, and the most decided traditions as to a northern origin seem to prevail among the natives of our South African Colonies, and would be explained by a southward migration from the country near the Zambesi such as must have happened on any hypothesis. But I have no desire to dogmatise on a matter which is still so obscure. It only seems to me that if in one part of South Africa we have proof of the existence of a mysterious but long-continued ancient civilisation, it is probable that this civilisation is connected with the exceptionally wide diffusion within and all round the same area of a particular group of languages.

The chief foreign influence which has affected East Africa in the past has been that of the Arabs, or at least of the inhabitants of the Arabian peninsula. With the exception of the Portuguese no Europeans paid any attention to these regions until the latter part of the

nineteenth century, and the Portuguese made hardly any attempt to penetrate into the interior, and merely held a series of ports to facilitate their voyages to India. There seem to be no clear proofs of Egyptian, Malay, or Indian influence, though perhaps there may have been a slight and indirect connection between Egypt and Uganda through the Sudan. But the connection of the whole East African coast with Arabia is certain and continuous. It is highly probable that the gold miners who erected the temples of Zimbabwe and started that ancient civilisation came from Arabia, and, however that may be, we know that from the tenth century of our era onwards, a stream of colonists flowed from Oman and Maskat to the east coast and founded a long line of cities and fortresses, such as Makdishu, Lamu, Mombasa and Kilwa. When the Portuguese arrived, at the end of the fifteenth century, they found a series of independent towns, peopled by Arabs and possessed of a considerable degree of civilisation. After about two centuries of Portuguese rule, the Arabs of the coast invoked the assistance of the ruling house of Oman, and about the beginning of the eighteenth century expelled the Portuguese from all their ports except Mozambique. The East African coast then became a nominal dependency of Maskat, and this rather shadowy connection was made a reality in the early part of the nineteenth century, when Seyyid Said, the Sultan or Imam of Maskat, took up his residence at Zanzibar. Though the settlements of the Arabs were almost exclusively on the coast, they penetrated far inland in the prosecution of the slave trade, reached the basin of the Congo, and had a much better knowledge of the position of the great central lakes than European geographers of the same period. But their one occupation was slave trading. They made no attempt to introduce Mohammedanism or conquer the countries of the interior, but merely deported the inhabitants to the coast or elsewhere.

Yet another invasion of Eastern Africa from Arabia is represented by the kingdom of Abyssinia. The population of this country is mixed and largely Hamitic, both in speech and physique, but the kingdom which still survives in the empire of Menelik was founded by Semitic invaders from Arabia some time before the Christian era, and the Ethiopian language, which is still used by the Abyssinian church and survives in more modern dialects, is akin to the language of the Himyaritic inscriptions of southern Arabia.

Let us now return to the narrower limits of our Eastern African Protectorates, and consider their inhabitants in the light of what has been said. If one makes a journey right across these territories, say from the Indian Ocean to lake Albert, one of the most striking facts is the difference between white and black taste as to what constitutes an eligible residence. The low malarious shores of lake Victoria, and most swampy and steaming localities, are thickly populated by natives, whereas the high cool districts of the interior are very sparsely inhabited, and there are large districts, such as the range of the Mau,

where a caravan may march for days without seeing a single native. This scarcity of population in excellent country is partly due to the former persecution of Arab slave traders and the attacks of nomadic raiders which made the more timid tribes anxious to avoid the open country. But the main reason is no doubt that Africans are immune to many of the diseases which attack Europeans, and find in the low hot districts an abundance of food, both animal and vegetable, procurable without exertion.

Almost the whole circumference of lake Victoria and the islands in it are inhabited by Bantu-speaking races, but the part where the population most increased, and where native civilisation reached its greatest development, was the country west of lake Victoria, and lying between that great sheet of water and the smaller lakes Albert, Albert Edward, and Kivu. Here are the kingdoms of Uganda, Unyoro, Toru, Ankole, in British territory, and Karagwe in German. Of these, Uganda is the most important, but all appear to have attained to some form of organisation under one chief deserving the name of principality or kingdom, and to have possessed a religious and social system in a fairly high stage of development. In this, they offer a remarkable contrast to the tribes on the north and east of the lake. It is not until we come to the coast itself where the influence of the Arabs and the Mohammedan religion has been strong, that we find social and political conditions worthy of comparison with Uganda. Among the Bantus of this area, there is nothing that can be called a state or kingdom; great chiefs are rare, and many tribes seem to have advanced little beyond the stage where the village is practically the same as the family. On the other hand, it is only just to say that the worst abuses of Africa, such as cannibalism, are unknown among them. The higher civilisation of the countries beyond lake Victoria is no doubt due to their greater fertility and greater peacefulness, but there are interesting but unfortunately obscure traces of an ancient foreign influence in Uganda and the other western kingdoms. In most of them there are traditions of an aristocracy called Hima, Huma, Bahima, or some such name, who were of pastoral, not agricultural, habits. These people who still form the royal and superior caste, have not preserved any language of their own, but they not unfrequently present a distinct physical type, light in colour, and with features resembling those of the Hamites or the faces seen on ancient Egyptian monuments. It is probable that this type represents the result of an ancient invasion of some Hamitic tribe, perhaps the Gallas, who may have introduced new blood and some measure of civilisation. As far as physical features go, we might be disposed to connect the Bahima with the Masai and the tribes found on the east bank of the southern Nile, of whom more anon, but there is such a sharp distinction in customs that this is hardly possible. To take only one point, the people of Uganda are remarkable for being all clothed. You are no doubt

aware that there is a tendency in Africa to regard costume as an ornament rather than a covering, and the Nilotic tribes near Uganda are conspicuously nude, the men considering it a point of honour to go perfectly naked. The people of Uganda, on the contrary, wear flowing robes, and are very particular in their ideas of decency. It therefore seems probable that these naked tribes are a latter stratum, which has covered up all traces of the route by which the older invaders may have reached Uganda. It is possible that research near the northern end of lake Victoria will yield discoveries throwing some light on these invaders and their origin, for it is an interesting fact that one of the most precious treasures of the natives of North Kavirondo is a kind of blue glass bead, apparently of very ancient workmanship, and resembling similar beads found in Egypt and Nubia. They are popularly believed to fall from the sky during thunder-storms, but the real explanation would seem to be that these violent storms disturb the soil, and occasionally expose beads which are buried in it. Also, the traditions of various tribes attach importance to ornaments and weapons made of brass or copper, metals which can hardly have been produced in the country, or obtained from the coast, as communication between the coast districts and the interior is only of very recent date.

But though it is pretty certain that the people of Uganda are a mixed race, and have been affected both in physique and institutions by invaders from the north, it appears to me by no means equally certain that we should seek in these events the origin of the Bantu languages and of the races who speak them. The contrary hypothesis is just as possible, namely that the Bantus have spread northwards from the Zambesi country, and received a special development in Uganda from contact with another race. It is often supposed that all migrations are from the north to the south, and this is certainly the usual direction in the northern hemisphere. But if the explanation of such movements is the preference for warm equatorial climates, it is equally natural that Africans in the south of the continent should move northwards to the equatorial regions. In historical times the movements of the Zimbas and other tribes from the Zambesi up to Mombasa is an instance of such a direction, and at the beginning of this century the warlike expeditions of the Zulus are said to have extended to the great lakes.

In their indigenous culture, and, still more, in their exceptional power of assimilating European civilisation, the people of Uganda are unique in this part of Africa, and probably in the whole continent When discovered by Europeans, they had a social system culminating in a king and an elaborate court, and comprising nobles, middle classes and peasants. They built cities and constructed roads, two things which are conspicuously absent in other parts of East Africa. From the first their readiness to receive European instruction, both religious and other, was remarkable. Though it is less than thirty

years since the first missions were established in the country, nearly all the inhabitants are nominal Christians, and large numbers can read and write. A native parliament has been instituted, and native courts of justice. It is true that some of the laws are rather strange, and considerable discussion has been provoked by an enactment fixing the price of all wives at 13*s*. 4*d*., whatever their beauty or mental accomplishments may be, but this people certainly offer the best augury that we have for the capacity of progress in African races. It remains to be seen whether they will advance beyond a certain point, and also whether their present docility will prove permanent, or whether, as among the Japanese, whom they resemble in some ways, the assimilative period will be followed by a revival of national sentiment.

The other Bantu-speaking tribes of the British East African territories, are, with the exception of the Swahilis, of small importance. They have little in the way of native political and military organisation, and only a moderate aptitude for adopting the blessings of European civilisation. I imagine them to represent the extreme flow of the wave of Bantu immigration which spread from the west or south, and they are far removed, geographically as well as metaphorically, from the special civilisation of Uganda as well as from the military organisations of the southern Bantus.

The most important tribes on the coast are the Swahilis and the analogous race found near Lamu and called Bajuns. Both are hybrids, the Swahilis being a mixture of Arabs with all sorts of African blood, while the Bajuns claim to be the descendants of Persian colonists. Both speak dialects of the same Bantu language.

Living as they do on a long narrow strip of coast, and in scattered archipelagoes, the Swahilis had no chance of attaining any sort of political union, even had they desired it, but they have exercised a wide-spread influence, chiefly through their language. In point of wide distribution and utility over a large area, Swahili may fairly claim to be one of the great languages of the world, and it possesses a remarkable vitality and power of advancing at the expense of other languages. It is more or less spoken as a *lingua franca* from Aden in the north to Durban in the south, and from the Indian ocean to the waters of the Nile and the Congo. Opinions differ considerably as to the merits of the Swahili race, and this is not surprising, for they are not homogeneous and represent merely a mixture of Arab blood--generally a very small proportion—with the most various African races, not merely the inhabitants of the coast, but all sorts of slaves brought from the interior. Their characteristic profession is that of caravan porters, but, now that the construction of the Uganda railway has rendered unnecessary the large caravans which were common ten years ago, this occupation is decaying. Valour is not their strong point, and they make only very moderate soldiers and policemen. They have a fair aptitude for commerce, but are shopkeepers rather than merchants, and also make good boat boys,

sailors and fishermen. My own experience of them has been favourable, and they clearly stand on a much higher level than most of the native races, so that there is some hope that they may assist in raising and civilising the tribes of the interior. Unfortunately, their superiority seems to depend almost entirely on a continual admixture of Arab blood, and now that the slave trade is abolished and European settlement in Africa is commencing, Arabs tend to frequent the country less and less. This, in some ways, is a pity, for it is well known that hybrids between Europeans and Africans are not a success.

In religion the Swahilis are mostly Mohammedans, though a good many attend the Christian schools, and they have a pride in genealogy and some taste for literature, both derived from the Arabs. The literature consists of poems and stories, of little interest except as indications of the mental culture of the writers, but also of chronicles of the various cities. Some of these have been published, but others, which exist only in manuscript, will throw a curious light on the colonisation of the East African coast when they are made accessible.

Nearly akin to the Swahilis are the Bajuns, who inhabit the islands of the Lamu archipelago, which lie close to the mainland north of that town. Their language is a dialect of Swahili, and they differ chiefly in their much fairer colour—which is often a light yellow—and in claiming Persian descent. It would seem that there are good grounds for believing in the establishment of real Persian colonies on this coast, but they were probably much mixed with Arabs, and it is noticeable that Nabahan, which is the name of the Bajun princes, is also the name of a dynasty which reigned in Arabia in Oman. The Bajuns must at one time have had a civilisation of some importance, for they can point to forts and ruined cities of considerable size, and the political history of their various communities is not without interest. Besides being constantly at war with their neighbours, they had an internal triangular duel between the princes, the common people, and the Somalis of the mainland. If was from fear of the latter that the Bajun civilisation was mainly confined to the islands, as, though the Somalis raided the coast, they never took to the sea; but in some cases the populace combined with the Somalis against the aristocracy and established a dual administration, consisting of representatives of the two nations, which in time led to further trouble.

In thus briefly reviewing the Bantu-speaking races of East Africa, I have treated them as the substratum of East African population affected by the invasion of Arabs, Hamites and Nilotic tribes. It would be wrong, however, to convey the impression that we have any right to assume that this substratum is primary or original. We know hardly anything of the natives of the coast, as opposed to the Arab settlements, before the arrival of the Portuguese, and for the interior we have no history at all but merely native traditions which rarely cover

more than a hundred years, though in a few cases genealogies may go back further. But even in this short period we know that there have been many movements of tribes, and that the present distribution is not likely to be even approximately original, though we can form no idea of how many strata of population there may have been. For the early history of central and southern Africa—that is, the history of more than two thousand years ago—as far as I know only two data are available. The first is, that unknown builders, almost certainly foreign emigrants who came to work gold mines, constructed very considerable erections of stone in Rhodesia; the second is, that both classical Greek authors, as well as the monuments of ancient Egypt, record that there were pygmies near the sources of the Nile, and of course the continued existence of these pygmies has been demonstrated of late years. It is certain that in many parts of Africa there are dwarfish races who stand on a lower level both physically and mentally than the surrounding natives. This small stature is most remarkable in the pygmies of the central equatorial forests, but the bushmen of the Cape and some of the inhabitants of Portuguese West Africa are also distinctly shorter than the average of ordinary humanity. In East Africa there are natives who have no independent tribal existence, but who live among and yet distinct from their more powerful neighbours on friendly terms, but still on a footing of recognised inferiority. Such a population is found among the Masai and Somalis, and perhaps elsewhere. Sometimes they are hunters, who provide game or ivory for the superior tribe, sometimes they are smiths who make its weapons, sometimes they are sorcerers. I have not in my own experience seen any of these races who could be called dwarfs, but they are distinctly not big men and are somewhat mean looking, and, except when their special talents, such as their wonderful skill in hunting, are concerned, they seem deficient in general intelligence. It is reasonable to assume that they represent an earlier stratum of population, which has partly been absorbed by other races and partly remained distinct in this curious servile relation.

There is one point about these people to which I would direct your special attention, and that is, that hardly any of them have a language of their own. Almost the only known exception in this respect are the Bushmen. But the others, both the pygmies of the central forests and the various smiths and hunters found living among the Somali and Masai speak a degraded form of the language used by those around them, in which the ordinary grammar and pronunciation are modified. This seems to me a fact which we should bear in mind in all speculations about primitive languages. There is a tendency to assume that languages spoken by tribes in a low state of civilisation, and indicating by their grammar and vocabulary a low state of intelligence, are primitive languages. I would, however, suggest that the instance of these African tribes makes it possible that such languages

are not primitive, and do not represent the earliest efforts of incipient speech, but are rather corruptions and degradations of more elaborate idioms. If, for example, a savage language is found to possess no numerals beyond two or three, it seems to me probable that this is not a primitive attempt to count, but rather that people who could not count at all have tried to copy the systems of numeration used by more advanced races, and have succeeded only to a very limited extent. I would not deny that language viewed historically shows a process of development, but we have absolutely no information respecting the historically earliest forms of speech, nor is it likely we shall ever have any, for they must have flourished ages before the use of writing. But the nearest parallels which we can find, those of writing and literature, make it probable that the growth of language has been far more irregular and spasmodic than is generally recognised, and that it was not developed with even approximate uniformity all over the world. In the case of writing, practically all alphabets are traceable to one, or at the most to two sources, whence the art has spread over the whole world; and people like the ancient Hindus, though they perhaps attained a higher intellectual level than the Egyptians and Assyrians, never invented a system of writing of their own. In the case of literature, the production of great works has generally been extremely capricious and confined to short spaces of time, such as the Augustan and Elizabethan periods. I can hence imagine that the development of language has been equally capricious —or perhaps we should say inscrutable—in regard to both place and time; that in special localities and special eras new types of language have been suddenly created, which were more perfect and more quickly mature than we are apt to think probable. These languages would be adopted by inferior races, but inevitably be corrupted and degraded because the finer parts of their mechanism would not be understood; and this I believe to be the explanation of a great many idioms spoken at present by uncivilised tribes. It is possible, though it would be difficult to demonstrate, that some of these dwarfish African tribes have never had a language of their own, and only have possessed the gift of speech in so far as they borrowed it from their neighbours.

Whether the pygmies are really the aborigines of Africa and a type of primitive man is too large a question for discussion here. The analogy of extinct animals gives no ground for imagining that the ancestors of the human race were smaller than their descendants, and I would rather suppose that the pygmies represent a very early but feeble race of mankind, who have always been prone to seek shelter and protection from the attacks of their stronger brethren, and found it most effectually in the forests of central Africa.

But my immediate purpose is rather to point out how in East Africa this substratum of Bantu-speaking people, much mixed no doubt and containing many physical types, but still united by a certain similarity of customs as well as of language, has been invaded

by Hamites and also by tribes such as the Masai. I may, however, allude to one peculiar feature which has contributed to complicate the ethnology of this part of Africa, and that is the colonies of runaway slaves called in Swahili, Watoro. The slaves owned by Arabs on the coast in former times, who were numerous out of all proportion to the work required of them, were drawn not only or chiefly from the neighbourhood but from every accessible part, from the Zambesi in the south to the Congo in the west. Those who from time to time escaped, founded colonies or republics where all other runaway slaves were welcome. Such was Fudadoyo, not far from Melindi, and on a larger scale, the district of Gosha on the south bank of the Juba. This is an excellent instance of how rapidly the population of Africa may change, from causes which we could never guess if we did not happen to know what actually occurred. A hundred years ago this district of Gosha was probably inhabited exclusively by Gallas. Now it is inhabited chiefly by Somalis, but the fertile country on the river bank is occupied by a mixed Bantu-speaking population, a large proportion of whom are said to come from Nyassaland, about a thousand miles to the south.

The Hamites in East Africa fall into two classes, the Gallas and Somalis. According to their traditions both came from Arabia, and it is clear that the Gallas represent the earlier invasion which has been pushed southward and westwards by the pressure of the Somalis behind. The main difference between the two tribes is, that the Somalis have adopted Mohammedanism, and with it acquired a certain amount of fanaticism and Arab civilisation ; the Gallas, on the other hand, are either Pagans or Christians, and they have been influenced by Abyssinia rather than by Arabia. At present, they are a receding race. In the time of the Portuguese they were the dominant power on the coast, but they were attacked by both the Somalis and the Abyssinians, until in 1872 their power was finally crushed by a coalition between the Somalis and the Arab or Swahili chiefs of the coast. At present, the name Galla is generally restricted to the remnant of them who live near the river Tana, inoffensive herdsmen, distinguished chiefly by their finely cut features, but in the north of our territories is found the large tribe of Boran Gallas. As no frontier has yet been recognised in this district, it remains to be seen whether the Borans will fall ultimately within the Abyssinian or the British sphere. It would appear that the Abyssinians conquered them six or seven years ago, and now claim them all as their subjects. They are a pastoral people, and live mainly on curdled milk. They keep large herds of cattle and some camels. They have also mules and ponies, and are, as far as I know, the only natives of East Africa who ride, this form of exercise being otherwise unknown, even to the most civilised tribes. Though a physically fine race, they are described as lazy and cowardly, so that their political extinction between the Somalis and Abyssinians is not strange.

The Somalis are an interesting and remarkable race. In their general physique and habits they resemble the Gallas, but they are far more vigorous, and have unusual aptitudes both for war and for trade. They combine the qualities of savage and civilised life. When at home they are nomads, chiefly remarkable for the quickness of their movements, having nothing which can be called cities or villages and little apparent taste for the arts of life. Like the Gallas, they keep cattle and camels, and, as our campaigns against them have proved more than once, they know very well how to utilise the military advantages which their country possesses. But though they have no towns of their own, at least in British East Africa, they continually visit Zanzibar and the various Arab and European settlements in the Protectorate. On these occasions, though somewhat inclined to be turbulent, they clearly rank with the Indians and Arabs and are superior even to the Swahilis. As merchants, particularly as buyers of cattle, they possess great talent and they show a fondness for litigation and a skill in using the law to their own advantage which cannot be paralleled among other natives. To my mind, they are the most interesting element in the population of East Africa, and the most enigmatical, for it is not always the most distant regions which are the least known, and at the present day there are probably few countries so little explored and so little influenced by Europeans as Somaliland and, one may add, Arabia. Only the southernmost portion of these tribes fall within the limits of the British East Africa Protectorate, but they occupy the whole country from the gulf of Aden to a little south of the Equator, which is divided between the small British protectorate known as Somaliland and the large but rather nominal sphere of Italian influence. Like the Arabs, they are divided into numerous tribes whose relations and sub-divisions are exceedingly complicated. The greater number of those in East Africa belong to the Ogaden division, and have a Sultan; but he exercises little authority, the more important chiefs having a sufficient number of followers to prevent the rise of anything like a real central power. It would appear that though the Mad Mullah sent emissaries to the East African Somalis, they were not disposed to join him.

I must pass on to the last group of tribes which I propose to consider, namely, the Masai, Lumbwa, Nandi, Suk, and Turkhana. These people are clearly allied to one another, but it is rather hard to find any common name for them which does not imply too much theory. Perhaps we are justified in calling them Nilotic, in the sense that they are akin to tribes which still inhabit the banks of the southern Nile, and whose origin is apparently to be sought in the countries near the Sobat. A glance at the map will show that from this district to the Rift valley, and thence a little way south into German East Africa, there runs a broad band of population clearly distinguished from the Bantu-speaking races in physique, language and customs.

Physically they are tall, thin men, with features less regular perhaps than those of the Hamites, but still not characteristically Negro, and sometimes almost Caucasian. Their languages are sharply distinguished from Bantu, and not clearly allied to any known group. They also agree in several remarkable customs. The men go stark naked, though the women are carefully dressed; there is often a recognised class of warriors who live differently from the rest of the population; and, though some of the tribes are settled, there is a strong tendency towards a pastoral and nomadic life. Other remarkable customs found both on the Nile and in East Africa are the habit of resting in a standing position on one leg, drinking the warm blood drawn from living animals, and shaving the heads of women.

It is eminently probable that these tribes represent a hybrid between the Galla and Negro races which may have been formed at an unknown period in the countries north and north-west of the Sobat; and it would appear, from Sir Samuel Baker's account of his travels on the southern Nile, that the Galla have frequently invaded these countries in historical times. The language and customs of the Dinkas of the southern Sudan clearly connect them with this group, and the same strain continues down the banks of the Nile in the Latuka, Bari, and Acholi; but the large tribe of the Madi are linguistically at any rate different, and perhaps represent a West African tribe which has advanced to the eastern side of the Nile. The Acholi spread eastwards, and closely akin to them in language are the non-Bantu part of the population of Kavirondo, and to the east of these spread the Nandi, Lumbwa, Masai, and other tribes whom I have mentioned. It will perhaps be best to confine our attention to the Masai, who are the most important and powerful of them. The others resemble them in many points, but have not developed the tribal military organisation in such perfection.

The chief peculiarity of the Masai is this remarkable military organisation, which has proved a most efficient instrument for successful raiding, if not for territorial conquests. The young men between the ages 17 and 27 or 30 are not allowed to marry, but live in separate villages apart from the married people. They subsist entirely on meat, blood and milk, and do not eat vegetables. In particular, they are forbidden to smoke or touch intoxicants. Their only occupations are warfare and looking after cattle. To herd donkeys, on the other hand, is a great disgrace. At the age of about 30 the warrior marries and settles down, and is then regarded as an old man. The Masai are nomadic in so far that they change their residence very easily, and are accustomed to spend part of the year in the valleys and part on the mountains, but they have also wooden kraals, to which they return year after year. They differ from most tribes in not killing or eating game, their energies being reserved entirely for warfare and not dissipated in hunting. It is remarkable that they never founded any sort of state analogous to Uganda and the other

western kingdoms, but this is doubtless explained by their peculiar organisation which placed the centre of gravity among the young men. The chiefs, who are elective, have to retire from the ranks of the warriors on being appointed, and though they may give advice, have little power of restraining the young men, and are regarded as having retired from the most serious business of life. They do not appear to have ever attempted to acquire a position analogous to that of the kings of Uganda and Unyoro ; and, outside the circle of warriors, the most important person was not the chief but the medicine man, who could foretell the result of expeditions.

But though the Masai never founded any state, they were a formidable power in East Africa in the middle of the last century. We know that they sacked Vanga on the coast, south of Mombasa, and reached the middle of German East Africa. In the north they raided at least as far as the Tana. They successfully asserted their independence againt the Arab slave traders, and took tribute from all travellers, including Europeans. Thomson, the first explorer of Masailand, who traversed their country in 1883, describes how they entered his camp and ordered about the whole caravan as if they had been the masters and he their slave. But bad times came upon them. They were driven backwards on the south by the warlike tribes of German East Africa, and on the north by the Somalis. Rinderpest attacked their cattle, and small-pox human beings. Then their immediate neighbours, who had no reason to love them on account of their raids in the past, fell upon them and greatly reduced their numbers. At present, they are variously estimated at from 12,000 to 25,000 souls in our Protectorate, but there must be more than this in German territory.

The Masai have had a strong influence on the surrounding Bantu-speaking races of the protectorates, who imitate their ways as far as they are able or dare, and have probably received a considerable admixture of their blood. This is especially the case with the people of Kikuyu, the high, fertile, wooded district which lies to the east of the Rift valley. These people speak a Bantu language, and are agriculturists, but they approach the Masai in physique and are more energetic and intelligent than the Wakamba or Wanyika. In times of trouble, when pasturage is scant, or a tribe gets broken up on account of internal dissension, the Masai have a tendency to settle down, and this has probably happened repeatedly in the Kikuyu country, with the result that a hybrid race has been formed.

I must not leave the Masai without alluding to a theory put forward recently by a German writer, Captain Merker, who was for some years an administrator in East Africa, that they are a Semitic people who separated from the Jews at some remote period before the latter occupied Palestine. This theory is supported by a series of traditions collected from the Masai, which show an astonishing resemblance to the earlier portions of the Bible. The most competent

investigators in our Protectorate have not been able to confirm the existence of these traditions, and I confess that I share their scepticism, and think that the stories which Captain Merker has collected are merely distorted versions of what natives have heard from missionaries, or perhaps from Mohammedans. But even if the traditions are old and genuine, the probabilities are enormously in favour of their being due not to any Semitic relationship, but to contact with Abyssinia, where there is not only an ancient Christian Church but also an ancient Jewish colony.

It is a curious fact, which has been noticed by several travellers, that the natives of East Africa have very few religious observances. It is, of course, not true that they have no religion at all, as is sometimes said, but it is a fact that whereas to the west of lake Victoria, in Uganda, Unyoro and other countries, there were in pagan times temples, priests and sacrifices of the usual African type, all these external signs of religion are wanting to the north and east of the lake. Mohammedanism is professed in the coast towns, but not very fervently, and by the Somalis with occasional bursts of fanaticism, but also it would seem not with habitual devotion. It has never penetrated inland or produced any effect on the Masai or other warrior races. I attribute this absence of external religious signs mainly to the predominance in these regions of nomads or semi-nomadic tribes. Nomads are not perhaps naturally irreligious, but they have not the time or the materials for impressive ceremonies. The example of northern Asia shows this. The Turks and Mongols when they settle down, are generally conspicuous for their devotion to Mohammedanism or Buddhism, but their original religious systems were slight and vague, and those of them who are still nomadic are extremely lax in their observances.

Two classes of religious ideas, other than Christian and Mohammedan, prevail among the inhabitants of East Africa. One is the worship of a sky-spirit, called Eng-ai among the Masai. The Gallas worship a similar spirit under the name of Wak, though it is not clear what the connection between the religious ideas of the two tribes may be. They agree, however, in having hardly any ceremonies except prayer. Among the Gallas, I strongly suspect that these prayers are due to Christian influence, as the petitions which are said to be used every day recall the language of the Lord's Prayer, and also the specimens published were collected in or near Abyssinia. The prayers of the Masai and kindred tribes seem to be genuine native compositions, in which the appeals to the sky god are mixed up with allusions to the heavenly bodies. Among the Masai and the other Nilotic tribes, there appears to be hardly any idea of existence after death, or of ancestor worship. Medicine men, who all belong to one family, are buried and are believed to turn into snakes, but they say that common people die like cattle, and corpses are simply thrown into the jungle to be eaten by hyænas. Among the Bantu-

speaking tribes, on the other hand, a different system of religious ideas prevails, based upon ancestor worship, surviving in a very fragmentary form, but still distinctly traceable. As I have already mentioned, these tribes respect and imitate the Masai, and therefore we find that they often use the Masai name for the deity Eng-ai, but the Bantu names, such as Muungu or Milungu, seem to really mean ghosts who are deified or at least require to be propitiated. These tribes are also accustomed to throw corpses away in the jungle, perhaps owing to Masai influence, but together with this survives the practice of burying and making offerings on the grave. Also the Bantu-speaking tribes are cursed with a belief in witchcraft from which the Nilotic tribes are free. This superstition is more terrible in its consequences than it sounds, for it means that every disaster, such as a death in a family, is attributed to evil magic, and when such disaster occurs it is customary to consult a witch-finder, who indicates some unfortunate person, generally a woman, as the culprit, and recommends that she be put to death.

In conclusion, there is one consideration of practical interest and importance connected with the facts which I have submitted to you. It is generally admitted that the Negro inhabitants of Africa stand on a lower level than Europeans or Asiatics, and raise themselves above that level with difficulty. The contrast takes an acute form in the United States, where we have a large and increasing population of Negroes, who show no tendency either to blend with the white race or to rise to the same standard. But in East Africa the conditions are entirely different. We have a mixed population which is continually blending and forming hybrids, and it is noticeable that all the most intelligent and progressive races are cross-breeds. The Swahili and the people of Uganda and Kikuyu are all clearly hybrids, and it is almost equally certain that the Nilotic tribes are so too. If we could survey a thousand years of East African history, we should probably see that it presents no such thing as a persistent and continuous racial type. This appears to me to offer a hopeful prospect for the future. The natives of East Africa stand on different levels, and in some cases the level is low; but there is every prospect that it may be raised by fusion in the future, as it has so often been in the past. A cross between Africans and Europeans is not to be desired, for, I believe, it has nowhere proved successful, and we have no proof that a cross between Indians and Africans is likely to be more successful; but there is every reason to hope that, in the future, peace and the cessation of tribal wars will produce favourable specimens of mixed population similar to the people of Kikuyu and the Swahilis.

[C. E.]

WEEKLY EVENING MEETING,

Friday, May 26, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Professor Julius Wilhelm Brühl, Ph.D. Sc.D. *Hon. Mem. R.I.*, Professor in the University of Heidelberg.

The Development of Spectro-Chemistry.

Associated as I am with Great Britain in my capacity as a Member of the Royal Institution, it is a special pleasure to me this evening to sketch to you the development of a branch of scientific study, the early history of which was enacted in this country—a country to which for many years I have been bound by close ties of sympathy. Many of you know already, and the others will see this evening, that I am in especial measure indebted to the science of this country for stimulus and encouragement in my own studies. It is a source of deep satisfaction to me to testify here to the gratitude which I owe to British science.

I.

§ 1. Last August it was my privilege to attend the Meeting of the British Association on the classic ground of Cambridge, and one sunny afternoon I found my way into that peculiarly effective example of collegiate architecture, the Chapel of Trinity College. The organ was playing Bach's Passacaglia, and I sat down quietly at the foot of the marble statue which bears the inscription :

Newton.

Qui genus humanum ingenio superavit.

The empty chapel was filled with harmony and with memories of the great man who once had sojourned there. And my thoughts wandered back to the past.

In 1666, almost two-and-a-half centuries ago, Isaac Newton, then a young bachelor, had decomposed a beam of sunshine, discovered the diverse refrangibility of the coloured rays, and explained the phenomena of dispersion.

The founder of scientific optics was also the first to perceive a connexion between differences in the composition of various natural bodies and their power of transmitting light.

Newton observed that oils, amber, sulphur, and other combustible bodies possess great refractive power, and he made the remarkable

statement that the diamond must also be combustible, because it refracts light so powerfully. This statement is indeed remarkable when we remember that in those distant times no one had any idea of the chemical composition of the diamond, or any conception of the nature of combustion.

But centuries of patient work were needed in order to recognise clearly the connexion between the chemical composition of different bodies and their power of refracting and dispersing light in different ways, i.e. of producing differently constituted spectra. To account for this connexion has become the task of "Spectro-Chemistry."

§ 2. Newton was also the first to fix a standard for measuring the refractive power of bodies. Starting from the emanation theory of light which he had himself founded, he diminished the square of the refractive index, n, by unity and regarded

$$n^2 - 1$$

as the expression for the refractive power. This power, reduced to constant density, i.e. divided by the specific gravity d of the body :

$$\frac{n^2 - 1}{d}$$

was called by Newton the "absolute refractive power."

§ 3. It was fully a hundred years later that Laplace, in his celebrated "Mécanique céleste," laid down the principle that the expression for refraction, derived from Newton's emanation-theory, must *for one and the same substance* be unaffected by changes in density caused by temperature and pressure—unaffected, that is, by the accidental density of a substance.

§ 4. The hitherto purely hypothetical formula for refraction acquired further scientific importance from the researches of Biot and Arago (1806), and Dulong (1826), on the refractive powers of gases and vapours. These, the first quantitative measurements of refractivity, seemed actually to confirm the theory that Newton's formula denotes a constant quantity which always remains unaffected by the accidents of temperature and pressure.

But with the triumph of the modern wave-theory of light, Newton's expression for refraction, $\frac{n^2 - 1}{d}$, lost its theoretical importance. New experiments soon showed that even its supposed independence of temperature and pressure did not in fact exist.

II.

§ 5. In 1858, John Hall Gladstone, who was for some time Professor in this Institution, began his splendid series of optical researches, which he pursued with great success for over forty years. At first in collaboration with the Rev. T. Pelham Dale he investigated the dependence of refractivity on temperature in the case of

fluid substances. The important fact was at once established that Newton's expression for refraction, $\frac{n^2 - 1}{d}$, is not constant, but varies considerably with the temperature. On the other hand, it was found that the more simple ratio

$$\frac{n - 1}{d}$$

remains practically constant.

Now this ratio, just like the Newtonian constant confirmed by Biot and Arago, and by Dulong, applies also to gases and vapours. As, in the case of gases, the refractive index is very little different from unity, the numerical value of $\frac{n^2 - 1}{d}$ is almost exactly twice that of $\frac{n - 1}{d}$.

III.

§ 6. Soon after 1860, Hans Landolt came forward with his optical researches. He began by confirming the results of Gladstone and Dale. He proceeded a step further, however, by following the example of Berthelot, and comparing the refractivity not of equal, but of molecular quantities of the substances. If P represents the molecular weight, the product $\left(\frac{n - 1}{d}\right)$ P is the *molecular refraction.*

§ 7. Landolt examined particularly the fundamental question whether a different grouping of the same number of atoms of the same elements—which is the cause of isomerism—has any influence on the optical properties of bodies.

He established the important fact that only the relative weight of the elements is of influence on the molecular refraction of a compound, while the different grouping of the atoms has no appreciable effect; and this made it possible to determine the atomic refractions of the elements. The atomic refraction of carbon, for instance, was obtained by comparing the molecular refractions of two compounds which differed only by one atom of carbon; and in a similar manner the atomic refractions of the remaining elements were determined.

With the aid of these constants it was now possible to calculate *a priori* the molecular refraction of many organic compounds from the elements composing them, and Landolt showed that the calculated molecular refractions agreed very well with those determined by experiment.

§ 8. Gladstone, in the course of his researches, was able to confirm Landolt's results in many cases. But he also found a considerable number of substances in which the observed molecular refraction was completely at variance with that obtained by adding the atomic refractions together. The exceptions were so numerous, that they really seemed to overthrow the whole law of summation.

IV.

§ 9. Shortly before 1880, when I was studying the literature of chemical optics, a brief note published by Gladstone in the *Journal of the Chemical Society* for May 1870, excited my attention and curiosity. The author there discusses the exceptions to Landolt's rule of summation. He shows firstly that in all such cases the molecular refraction is never found to be too small, but always too great. Then he shows that whole classes of compounds behave in this abnormal fashion.

§ 10. All optically abnormal compounds proved to be rich in carbon. Gladstone, therefore, examined the effect which a gradual increase of carbon in the composition of a body exerted on its refractivity. He found that there actually was an increase in the excess of the experimental as compared with the calculated molecular refraction, but the increase was not regular enough to explain the anomalies.

The saturated hydrocarbons, or *paraffins*, of the general composition C_nH_{2n+2}, showed *normal* molecular refraction.

Also the *olefines*, containing two atoms less of hydrogen, were found normal by Gladstone.

On the other hand, the hydrocarbons containing six atoms less of hydrogen, viz., the *terpenes*, gave molecular refractions about 3 units larger than would correspond to their composition.

With the aromatic hydrocarbons, such as benzene, toluene, etc., containing eight atoms less of hydrogen, this abnormal excess amounted to 6 units :—

Paraffins	(C_nH_{2n+2})		Normal	
Olefines	„	$-H_2$	„	
Terpenes	„	$-H_6$	„	$+3$
Benzene and Derivatives	„	$-H_8$	„	$+6$

With still further decrease in the quantity of hydrogen contained (i.e. with further increase of carbon), there resulted greater and greater refractive increments.

The last member of the series, however—pure carbon without any hydrogen, represented by the diamond—proved to be perfectly normal in its optical properties.

V.

§ 11. François Arago, when sketching in his celebrated "Éloges" the life and work of Thomas Young, relates that the latter was led to his fundamental discovery of the interference of light by nothing more extraordinary than soap-bubbles. In this connexion Arago remarks that it is one of the most precious gifts to be able to wonder at the right time.

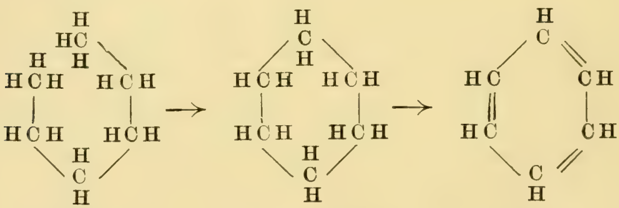
Gladstone's observations just mentioned had been known almost

ten years without anybody's surprise being excited. When his short note came into my hands, I was fortunate enough to begin wondering at the right time.

It seemed to me really extraordinarily remarkable that all optically abnormal substances, without exception, gave a too *high* molecular refraction. It was no less astonishing to me that the saturated hydrocarbons were optically normal, but became more and more abnormal at successive withdrawals of hydrogen—while pure carbon, uncombined with hydrogen, is again completely normal.

But I was most particularly struck by the quantitative amount of the abnormality in the case of benzene compounds, especially their refractive increment of *six* units. The number 6 fascinated me. I could not help thinking that therein lay the key to the mystery, and I lost no time in making use of it.

§ 12. According to Kekulé's ingenious hypothesis we can imagine benzene, C_6H_6, to have arisen from the saturated hydrocarbon hexane, C_6H_{14}, by successive removal of hydrogen.



(Hexane) C_6H_{14} (Hexamethylene) C_6H_{12} (Benzene) C_6H_6

Thus altogether four pairs of hydrogen atoms have been removed. The elimination of the first pair was made the occasion to form another *simple* carbon bond, like those already present in hexane, and with it the ring was closed. The splitting-off of the other three pairs of hydrogen atoms, on the other hand, resulted in the formation of three *double* bonds of carbon atoms—a kind of bond which does not occur in the optically normal hexane.

Now Gladstone had found that benzene exhibits a refractive increment of 6 units. Reading this I was struck in a moment by the thought: might not this abnormal refractive increment of benzene be due to its double carbon bonds, which are absent in the optically normal hexane?

And if this were so, I went on to reason, since *three* double bonds in benzene correspond to a refractive increment of 6 units, therefore *one* double bond must entail the increment of 2.

These ideas received no support whatever from the then known facts. For Gladstone had stated expressly that the olefines, i.e. open-chain hydrocarbons, containing *one* double carbon bond, were optically *normal*.

However, I did not allow myself to be discouraged ; and, behold ! my expectations were confirmed by the very first experiment. The olefine examined not only proved to be optically abnormal, but gave the predicted refractive increment of 2 units, corresponding to the presence of *one* double carbon bond.

Gladstone, therefore, as I had supposed, was mistaken in this case. Further experiments proved that not one of the olefines was optically normal. Without exception they gave the refractive increment of 2 units, one-third of that of benzene.

I next proceeded to examine the diolefines—substances which contain *two* double carbon bonds. Here also, in conformity with expectation, a constant refractive increment was found, double as large as that of the olefines and two-thirds of that of benzene :—

Paraffins	(C_nH_{2n+2})		Normal	
Olefines	,,	$-H_2$	,,	$+2$
Diolefines	,,	$-H_4$	,,	$+4$
Benzene compounds .	,,	$-H_8$	,,	$+6$

The dimensions of our subject this evening prevent the detailed demonstration of these important facts by experiment. I will only show you that the spectrum of a saturated hydrocarbon (a paraffin) is distinguishable at a glance from that of a substance containing double bonds.

On this screen we project the electric spectrum of metallic calcium. First we cause the rays of light to pass through a prism filled with paraffin-oil. Then we exchange this prism for another, filled with a substance containing atoms linked by double bonds. (Experiment.)

In the second case you observe, firstly, a much greater deviation of the whole spectrum, i.e. greater *refraction*, and secondly, far wider intervals between the coloured lines of the spectrum, i.e. greater *dispersion*, which is usually correlative to the refraction.

§ 13. Thus quantitative experimental confirmation was obtained for the view that abnormal refractive increments which increase with the diminution of hydrogen contained in the substances, are caused by the presence of double carbon bonds.

At the same time, however, the experiments yielded a second result of fundamental importance. The olefines contain 2, and the diolefines 4 atoms of hydrogen less than the paraffins. Similarly the refractive increment of the olefines is 2, and of the diolefines 4.

Benzene, C_6H_6, contains 8 atoms of hydrogen less than the corresponding paraffin, hexane, C_6H_{14}. The increment of benzene, however, amounts not to 8, but to 6 ! Thus in the formation of benzene from hexane, 2 atoms of hydrogen have been eliminated without influence on the refractive increment of the product.

But in the formation of benzene from hexane, 2 atoms of hydrogen have been employed to close the ring (see diagram on p. 126).

The withdrawal of these two atoms and the closing of the ring have therefore taken place without causing any optical anomaly.

In the formation of the olefines and diolefines from the paraffins, however, there is no closing of the ring. These substances are of open-chain structure, and *every* removal of 2 hydrogen atoms corresponds here to the creation of a double carbon bond :—

$$H_3C \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot CH_3 \qquad CH_2 = CH \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot CH_3$$

Hexane (C_6H_{14}) — Hexylene (C_6H_{12})

$$CH_2 = CH \cdot CH_2 \cdot CH_2 \cdot CH = CH_2$$

Diallyl (C_6H_{10})

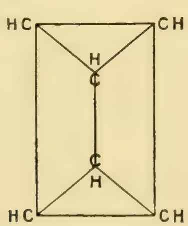
Hence, also, the refractive increment of the olefines and diolefines is directly proportional to the number of hydrogen atoms removed from the paraffin.

From all this it follows that the removal of hydrogen atoms causes optical anomalies only where double carbon bonds are created by the process. *The splitting-off of hydrogen which results in a closing of the ring is, on the other hand, without abnormal optical influence, and produces no refractive increment.*

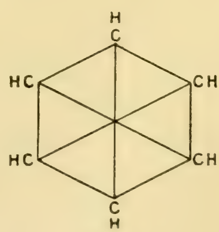
This latter principle, which has since been confirmed many times by experiment, has proved of the same importance as the first in the investigation of the chemical structure of bodies.

§ 14. A few examples will show how these two principles can be utilised for the discovery of chemical structure.

Besides the formula already mentioned for benzene—that suggested by Kekulé—several others have been proposed, e.g. those by Ladenburg and Claus :—



(Ladenburg)



(Claus)

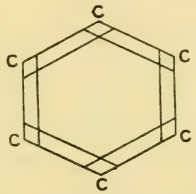
Neither of these graphic formulæ is reconcilable with the results of spectro-chemical investigation, because the neighbouring carbon atoms contained in them are associated only by single cycloid or ring-closing affinities, and not by any so-called double bonds. Substances of this kind should be optically normal, while benzene and its derivatives are as a matter of fact abnormal. Kekulé's formula

for benzene is really the only graphic representation of its structure in a single plane which is confirmed by chemical optics.

Thus it can be at once determined by optical methods whether a given body belongs to the paraffinoid, olefinoid, or cycloid products; whether these products contain double bonds or not; and if so, how many.

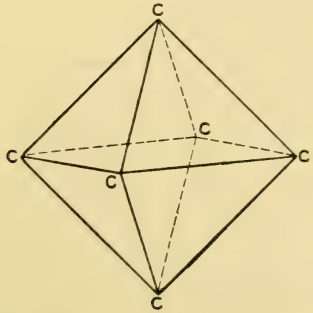
Now, too, we can imagine why the diamond, i.e. pure crystallised carbon, is, as already mentioned, optically normal. We obtain an idea of the mineral's chemical constitution, and of the way in which the atoms of carbon are perhaps combined in the sparkling gem.

For the reasons already stated, the diamond cannot possibly contain any double bonds; a combination, say, in the form



with one atom of carbon at each of the six corners, and with each atom connected with its neighbour by a double bond, is altogether impossible.

Imagine, however, at each of the six corners of a regular octahedron, a single molecule of marsh-gas, CH_4, i.e. altogether C_6H_{24}, and then imagine all the 24 hydrogen atoms successively removed, so that each carbon atom is connected with each of its neighbours only by a single bond, and thus all six atoms of carbon are united together in a single whole. Then you obtain, as the most simple representation of the molecule of the diamond, a regular octahedron, with one atom of carbon at each of its six corners, while the edges represent the mutual bonds :—



Several simple molecules of this kind may be combined into one crystallised particle of the spectrochemically normal diamond.

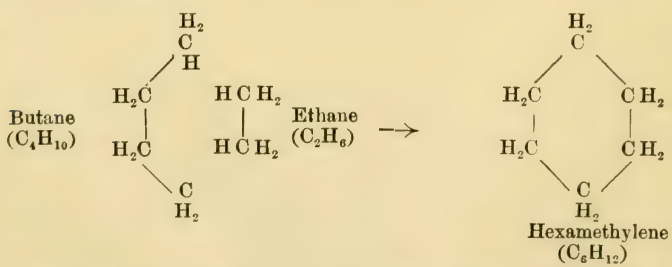
§ 15. Thanks to the explanation of the optical behaviour of benzene, with the resultant discoveries, it all at once became possible to understand the causes of the spectro-chemical abnormality of whole classes of bodies, such as the olefines, diolefines, terpenes, aromatic compounds, etc., and light was cast on the chemical constitution of whole classes of bodies.

At the same time, however, it at once became apparent why both Landolt and Gladstone had succeeded in observing complete optical normality in very numerous substances of the most various types—alcohols, acids, ethers, hydrocarbons, etc. And now it was understood why in such bodies the molecular refraction is determined solely by the component elements, while the different grouping of the atoms, i.e. the isomerism, remains without any appreciable optical influence.

All the bodies of this kind proved to be either paraffins, i.e. saturated hydrocarbons, or simple derivatives of the same. But the paraffins, as we now know, are always optically normal, because they contain no double carbon bonds. For this reason all such simple derivatives of the paraffins must also be normal. Their molecular refraction will thus always correspond to the elements of which they are composed, however the atoms may be grouped, i.e. chemical isomerism is here also without influence.

§ 16. For the same reason, however, all cycloid (ring-shaped) closed formations, if they contain no double carbon bonds, must be optically normal, for those bodies also may be conceived as originating in the simple replacement of hydrogen by paraffin fragments, and may therefore be regarded as combined paraffins.

Thus we can imagine the hexamethylene already mentioned not only as formed from hexane by removal of *two* hydrogen atoms from the ends, but also as arising from ethane and butane, i.e. from two paraffins, by the removal of *four* hydrogen atoms and welding together of the remains :—



As a combined paraffin, hexamethylene must be normal, as is also confirmed by experiment, and here we see again, as in the case of the

diamond, that a progressive removal of hydrogen and increase of carbon need not lead to the slightest optical anomaly.

At the same time there arises here a case of the optical influence of isomerism, for hexylene, which has already been mentioned, with the same formula (C_6H_{12}) as hexamethylene, but in structure an olefine :—

$$CH_2 = CH \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot CH_3$$

possesses the familiar refractive increment of 2 units. This example again shows how the spectro-chemical behaviour of a body discloses its chemical structure by enabling us to distinguish with certainty between an optically normal cycloid (or ring-substance), and an isomeric open-chain olefinoid formation, which is optically abnormal.

VI.

§ 17. Carbon can thus act variously upon light according to the manner in which its atoms are combined. We can therefore transfer the refractive increment of the double bond to the atom itself.

In the diamond, and in all paraffinoid carbon compounds, the atomic refraction of carbon equals 5 ; it is therefore equal to 10 for two carbon atoms. The double bond increases the refraction by 2, so that for two carbon atoms with a double bond the refraction amounts to 12. The atomic refraction of *one* carbon atom with a double bond is therefore equal to 6, i.e. 20 per cent. greater than that of the atom with the single bond :—

	Atomic Refraction.
1 Carbon atom C (diamond and paraffins) . .	5
2 Carbon atoms 2C (diamond and paraffins) . .	10
Double bond	2
2 Carbon atoms with a double bond $C = C$ . .	12
1 Carbon atom with a double bond $C =$ . .	6

§ 18. Carbon, being a quadrivalent element, can also appear with *triple* bonds :—

$$R.C \equiv C.R$$

Experiment has shown that carbon with a triple bond also acquires a special atomic refraction.

Thus it becomes possible to establish the presence of this kind of bond in substances, and to distinguish it from the double and simple bonds—a further criterion of structure.

§ 19. In consequence of these discoveries it became highly probable that all multivalent elements, such as carbon, possessed an atomic refraction varying with the kind of bond, while the univalent elements, such as hydrogen, display constant optic values because atoms such as theirs can only be linked with a simple bond.

Later researches have confirmed this. The univalent halogens give, like hydrogen, constant atomic refractions, both in the elementary state and in their compounds. The multivalent elements, on the other hand, such as oxygen and nitrogen, display different optical values, according to the kind of bond.

In the course of such researches the behaviour of oxygen as a quadrivalent element, which had been previously conjectured, was established with certainty, and afterwards confirmed synthetically by Collie, Tickle, and others.

§ 20. The theory which accounted for the optical abnormalities of certain classes of bodies, making them in fact abnormalities no longer, has proved extraordinarily fruitful. It formed the starting-point of all subsequent discoveries in the subject, and indeed we may describe the progress of this branch of science during the last 25 years as based essentially on this conception.

For not until we had fathomed the mystery of the benzene refractive increment 6, was it possible to know for certain that the variable valency of the multivalent elements is always of determining influence on the optical behaviour of bodies. Thus for the first time a spectrochemical method was called into being for the study of chemical structure, and the foundations were laid of what we now call "Spectro-chemistry."

VII.

§ 21. We must now return once more to the formula for refractivity. Newton's expression $\left(\frac{n^2-1}{d}\right)$ P had proved not constant for the temperature in the case of fluid bodies, and was, therefore, replaced by Gladstone and Dale's more satisfactory ratio $\left(\frac{n-1}{d}\right)$ P. For 20 years and more this did admirable service. As, however, the number of observations kept on increasing, even this formula betrayed imperfections which finally led to its abandonment. It is impossible here to follow the argument in detail, and we must be content with the remark that comparisons of bodies in different states of aggregation failed to yield satisfactory constants. The values of $\left(\frac{n-1}{d}\right)$ P for a fluid or solid substance always came out considerably greater than for the same substance in the state of gas or vapour.

§ 22. Then by a happy chance two physicists, L. Lorenz, of Copenhagen, and H. A. Lorentz, of Leyden, came forward simultaneously in 1880 with a new expression for refraction. One of them started from the ordinary theory of light, the other from Maxwell's electromagnetic theory of light based on Faraday's views, and they

both reached the same result, viz., that the true measure of refractivity is furnished by the expression

$$\left(\frac{n^2-1}{n^2+2}\right)\frac{P}{d}.$$

Experimental tests showed that this theoretical expression was in fact, for all bodies, practically unaffected not only by temperature and pressure, but also by the state of aggregation.

Chemical tests confirmed the utility of the new optical standard, since the operation of all the laws before mentioned was observed to be even more exact when the new constant was applied.

§ 23. Moreover, the expression for refraction proved valuable in another respect. It was found to be very suitable for measuring the *dispersive* power of bodies.

If n_v and n_r denote the refractive indices for the limits of the visible spectrum, i.e. for violet and for red light, the difference of the refractivities for these end-rays of the spectrum :

$$\left(\frac{n_v^2-1}{n_v^2+2}-\frac{n_r^2-1}{n_r^2+2}\right)\frac{P}{d}$$

is the measure of the power of different bodies to *disperse* light—to broaden out the spectrum. This ratio proved to be constant as regards temperature, pressure, and state of aggregation.

Gladstone had already observed that dispersion, like refraction, was connected with the chemical nature of bodies. Quantitative relations were, however, only obtained when a constant for refractivity had been found. And then from the molecular dispersions of compounds the atomic dispersions of their elements were deduced.

We cannot enter here into the relations which were thus shown to exist between the chemical composition of substances and their power to disperse light. We need only remark that the case as a whole is analogous to that of refraction. Dispersion is, however, a still more sensitive and more constitutional property, and therefore in many cases it is specially adapted as an aid to research on chemical structure.

VIII.

§ 24. It only remains to add a few remarks on the applications of spectro-chemistry in science and in practical life.

I have already shown the principles on which spectro-chemical methods of examination in general can be applied to the solution of scientific problems, to the discovery of the chemical structure of single substances or whole classes of bodies.

Now there are a large number of substances, some of them artificially built up by synthesis out of their elements, some of them occurring in the vegetable and animal kingdoms, or even in inorganic nature, the structure of which is of remarkable delicacy and insta-

bility. Among them are, for instance, the so-called "tautomeric" compounds, hydrogen peroxide, and many other unstable compounds. Substances of this kind are of a very special interest, for in consequence of their tendency to change, they are the principal cause of metamorphoses, the unceasing circulation of matter, the eternal birth and decay that goes on in nature.

Research into the atomic structure of such bodies by purely chemical methods is often very difficult, and not seldom impossible, because, owing to their sensitive organisation, chemical interference leads either to changes in the grouping of the atoms, which cannot always be controlled, or even to total decomposition.

In such cases it is of course of the greatest value to be able to examine the constitution of the bodies without affecting them chemically; and spectrochemistry, as we have seen, gives us the means of doing so. By observing the behaviour of light on its passage through the various substances, we gain an insight into their structure without in any way disturbing it

§ 25. In the last ten years the spectro-chemistry of the nitrogen compounds has also made remarkable progress. Nitrogen is of the greatest importance as an essential constituent of the proteids, the alkaloids, and many other animal and vegetable products. But its high valency and the extraordinary variety of combinations into which it can enter with other elements, surround it with special complications. Regardless of these, however, the spectro-chemical examination of nitrogen compounds has already yielded useful results, especially in the study of the alkaloids. It is to be expected that this optical method will also be of use in the chemistry of the albuminoids, the study of which is now being prosecuted with so much vigour.

§ 26. One class of substances of increasing importance both to science and to chemical industry is that constituted by the natural and artificial perfumes. An overwhelming majority of them consists of derivatives of the terpenes. We have already mentioned that Gladstone, in this subject also a pioneer, was the first to study the optical behaviour of the terpenes. Since then the explanation of the structure of these bodies and of a large number of rich natural perfumes derivable therefrom has been rendered easier by the use of spectro-chemical methods. Similar assistance has been rendered to the synthetic preparation of valuable scents, such as ionone, the artificial scent of violets. In every scientific laboratory and in every rationally conducted chemical factory where work is being done on perfumes, the spectrometer is now an indispensable testing instrument, and hence also an implement in industrial production.

IX.

§ 27. When scientific research opens up new methods of observing nature, it is generally not long before a use is found for these methods

in practical life. The need is soon felt of perfecting, and at the same time simplifying, the scientific apparatus. Efforts in this direction have not been wanting in the case of the spectrometer, and they have been crowned with the most brilliant success.

Professor Abbe, the distinguished physicist who died not long ago, and after him Dr. Pulfrich, constructed spectrometers on the principle of total reflection. These instruments are distinguished from those formerly in use by their extraordinary simplicity and convenience, and they allow also of much more rapid work.

Such instruments, known as total-reflectometers, have been made for the most exact scientific measurements, and also for medical and technical purposes. Special forms are in use for the examination of fats and oils, milk and butter ; to determine the amount of salt contained in salt solutions ; the amount of alcohol and extractive matter in beer ; for the examination of blood and albuminoids in pathological fluids, etc. Several of these ingeniously contrived instruments give not only the refractive index and the dispersion of a substance immediately, without any calculation, but also directly the percentage of dissolved matter, e.g. of alcohol and extractives in beer.

A number of such instruments from the celebrated factory of Carl Zeiss, of Jena, are here exhibited at my request.

I have now reached the end of my remarks. I have reviewed the development of spectro-chemical research since Newton's time, and we have seen that, although different nations have taken part in the work, a specially large share has fallen to British investigators. For this reason, as I said at the beginning of my discourse, it has been a special pleasure to me to be able to treat of such a subject before the Royal Institution.

There is a saying of Montesquieu's which I venture to hope has its application to this evening : "Quand vous traitez un sujet, il n'est pas nécessaire de l'épuiser, il suffit de faire penser."

From Newton to our own day—a length of time which our planet takes to complete 250 revolutions round the sun—that is the period through which we have sped in 60 minutes. It will be readily understood that on such a rapid trip we have only been able to stop at one or two view-points, and have been obliged to content ourselves with a hasty survey. I thank you all for having ventured yourselves with me on such a hurried excursion, and I shall be happy if it has been conducted without mishap.

[J. W. B.]

WEEKLY EVENING MEETING,

Friday, June 2, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

George Henschel, Esq.

Personal Recollections of Johannes Brahms.

From the title of my discourse it will be gathered that in speaking to-night of Johannes Brahms I do not propose to refer—at any rate not in a critical sense—to his works. Outside of Germany and Austria, the master's native and adopted countries, these works are known and loved nowhere better, nor more widely, than here in England. Yea, as regards Brahms' place among the composers of the world, I do not hesitate to assert that even in the Vaterland nothing, in my opinion, has been written or published, which in soundness of judgment, discrimination and appreciation, can compare with Mr. W. H. Hadow's admirable article, "Brahms and the Classical Tradition," published soon after the master's death in the 'Contemporary Review.' Brahms however never having visited this country, and the number of those who knew him at all intimately, being a very small one even on the other side of the channel, I venture to hope that these personal recollections of Brahms, and excerpts from a journal I kept when travelling with him in the seventies, will not be unwelcome to the many who, though more or less familiar with Brahms the composer, would fain know a little more of Brahms the man. His work, as Mr. Hadow rightly remarks, is still too near us for any certain or dogmatic estimate of its value.

But, I may add, the heart, the soul of man know of no time. The infinite variety of their outward manifestation will ever and anew remain an interesting, a fascinating study.

In commencing now to read my reminiscences I would beg you to kindly and indulgently remember, when I come to the reading of the Journal, that it was written well-nigh thirty years ago.

It was on the occasion of the Lower Rhenish Musical Festival at Cologne, in May 1874, that I first met Brahms. For weeks beforehand my mind had been occupied by the thought of seeing face to face the great composer, whose name was then on every musician's lips as that of a man whose genius Robert Schumann had publicly proclaimed in the glowing language of an inspired prophet. And I well remember my embarrassment and the sensation it gave me, when at last I was permitted to shake hands with him after the rehearsal of Handel's "Samson," in which oratorio I had been engaged to sing

the part of "Harapha." A few kind and encouraging words, however, soon put me at my ease, and I could give myself up to scrutinising Brahms' personal appearance.

He was broad-chested, of somewhat short stature, with a tendency to stoutness. His face was then clean shaven, revealing a rather thick, genial underlip; the healthy and ruddy colour of his skin indicated a love of nature and a habit of being in the open air in all kinds of weather; his thick brown hair fell down nearly to his shoulders. His clothes and boots were not exactly of the latest pattern, nor did they fit particularly well, but his linen was spotless.

What, however, struck me most, was the kindliness of his eyes. They were of a light blue, wonderfully keen and bright, with now and then a roguish twinkle in them, and yet at times of almost child-like tenderness. Soon I was to find out that that roguish twinkle in his eyes corresponded with a quality in his nature, which would, perhaps, be best described as good-natured sarcasm. To give a few illustrations of that here: In the afternoon of that day, a friend of mine, a rather celebrated composer, had asked Brahms to be allowed to play to him, from the MS., his latest composition, a violin concerto. Brahms consented to hear it, and seated himself some little way from the piano. Mr. So-and-so played his work with great enthusiasm and force, the perspiration—it was a very warm day—streaming down his face.

When he had finished, Brahms got up, approached the piano, took a sheet of the MS. between his thumb and middle-finger, and, rubbing it between them, exclaimed, "First-rate! I say, where do you buy your music paper?"

In the evening I found myself sitting with Brahms in a *Kneipe*—one of those cosy restaurants, redolent of the mixed perfumes of beer, wine, coffee, and food, so dear to Germans in general, and to German artists in particular—in the company of four or five prominent composers of the day, who had come from their different places of abode to attend the festival.

The musical proceedings of the day had been the chief topic of conversation, when suddenly one of the "Herren Kapellmeister," pointing toward me (some new songs of mine had figured on the programme of the morning's concert), exclaimed: "Now, just look at that lucky fellow Henschel! He can both sing *and* compose, and we"—describing with his hand a circle which included Brahms—"we can compose only."

"*And not even that*," it came instantly from Brahms, whilst his countenance bore the expression of the most perfect innocence. It was not until the spring of the following year (1875) that I met Brahms again. In the meantime, some letters had passed between us, resulting in my being engaged to sing at some concerts of the Society of the Friends of Music in Vienna—of which Brahms, at that time, was the conductor; and it may be imagined how great an

inspiration it was for a young musician like myself, to sing under the direction of Brahms, and to be, for some weeks, in daily and intimate intercourse with him.

We went for a walk together every day, mostly in the Prater, the favourite out-of-door resort of the Viennese, and it seemed a matter of no small gratification to Brahms to find himself recognised and respectfully greeted everywhere we happened to drop in for an occasional rest.

The numerous public gardens where gipsy bands played, especially attracted us, and it was a delight to notice the increased spirit those brown sons of the Puszta put into their music in the presence of the master who had done so much toward opening up to their beloved tunes a wider sphere of popularity.

The performance of Bruch's "Odysseus," was the last that Brahms conducted for the society, having resigned his post early in the year. It took place in the morning, and was followed by the solemn ceremony of presenting Brahms with an illuminated address of farewell, acknowledging his great achievements as conductor of the society, and expressing the society's and the chorus's regret at his resignation. A local poet of some fame who, because he lived in that district of Vienna known as the inner town, was rather naughtily called by Brahms "the poet of the inner town," delivered a very eulogistic oration, which Brahms, who looked rather bored, merely acknowledged with the curt words, "Thank you very much." Then, taking under his arm the folio containing the address, he walked away. He afterward told me that such official proceedings were exceedingly distasteful to him.

It was in the following year that I began my diary. My profession brought me into frequent contact with the master, who, to my gratification, seemed to have permitted the young, enthusiastic musician to have, in intimate hours, an occasional deeper insight into the workings of his mind than was vouchsafed to the outer world, against which he appeared to be fortified with the "*æs triplex*" of irony, sarcasm and indifference. I was anxious to preserve the many interesting things he had to say on musical and other matters, and religiously jotted down my recollections in the evening of each day spent in Brahms' company. I have not attempted to embellish or improve upon the style, if style there be, of these cursory notes, but give them, translated literally from my MS. written in pencil and with no corrections whatever, thus indicating the utter absence, at that time, of any desire on my part to let them see the light of publicity.

The Diary.

Münster, *Westphalia*, *Feb.* 3, 1876.—Brahms arrived yesterday. I am glad my hoarseness is gradually disappearing, for the thought of singing at the concert, day after to-morrow, those high notes in

his "Triumphal Hymn" for double chorus and baritone solo (Op. 55, published in 1872) rather troubled me. I asked him if eventually he would object to my altering some of the highest notes into more convenient ones on account of my cold, and he said: "Not in the least. As far as I am concerned, a thinking, sensible singer may, without hesitation, change a note which, for some reason or other, is, for the time being, out of his compass, into one which he can reach with comfort, provided always the declamation remains correct, and the accentuation does not suffer."

Feb. 6.—Yesterday was the concert. Brahms played his Pianoforte Concerto in D minor wonderfully. I especially noticed his emphasising each of those tremendous shakes in the first movement by placing a short rest between the last note of one and the first little note before the next. During those short stops he would lift his hands up high and let them come down on the keys with a force like that of a lion's paw. It was grand. The glorious but horribly difficult "Triumphal Hymn," conducted by Brahms, went splendidly. It was a veritable triumph for the executants, as well as for the composer. The joy and gratification expressed in Brahms' face at the end, when acknowledging the acclamations of audience, chorus and orchestra, was evidently caused as much by the consciousness of having written a fine work as by its reception.

Coblenz on the Rhine, Feb. 26.—Brahms and I were the soloists at the orchestral concert which took place last night under Maszkowski's conductorship. The day before was the final full rehearsal "Generalprobe" (to which, in most places in Germany, the public are admitted). Brahms had played Schumann's Concerto in A minor, and missed a good many notes. So in the morning of the day of the concert he went to the concert hall to practise. He had asked me to follow him there a little later, and to rehearse with him the songs (his, of course), he was to accompany me at the evening's concert. When I arrived at the hall I found him quite alone, seated at the piano, and working away, for all he was worth, on details of Beethoven's "Choral Fantasia" and Schumann's Concerto. He was quite red in the face, and, interrupting himself for a moment on seeing me stand beside him, said with that childlike, confiding expression in his eyes: "Really, this is too bad. Those people to-night expect to hear something especially good, and here I am likely to treat them to a hoggish mess. I assure you, I could play to-day, with the greatest ease, far more difficult things, with wider stretches for the fingers, my own concerto for example, but those simple, diatonic runs are exasperating. I keep saying to myself: 'But, Johannes, pull yourself together, do play decently.' But no use, it's really horrid."

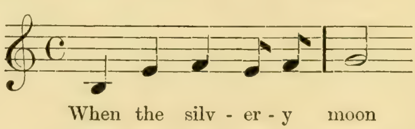
After our little private rehearsal of the songs, Brahms, Maszkowski, who had in the meantime joined us, and I, repaired to Councillor Wegeler's, Brahms' host, in accordance with an invitation to inspect

the celebrated and really wonderful wine-cellars of his firm, and to partake of a little luncheon in the sample-room afterwards. Toward the end of the repast, which turned out to be a rather sumptuous affair, which Brahms relished as much as any of us, a bottle of old Rauenthaler of the year '65 was opened, with due ceremony, by our host. It proved indeed to be a rare drop, and we all sat in almost reverential silence, bent over the high light-green goblets, which we held in close proximity to our respective noses. Wegeler at last broke the silence with the solemn words: "Yes, gentlemen, what Brahms is among the composers, this Rauenthaler is among the wines." "Ah, then, let's have a bottle of Bach now!" exclaimed Brahms as quick as lightning.

The concert went off well, as did the supper afterward. Brahms was in particularly high spirits. The many proofs of sincere admiration and affection he had received during his stay in Coblenz had greatly pleased and touched him, and he went so far as to make a speech—a very rare thing with him.

Wiesbaden, Feb. 27, 1876.—Yesterday Brahms and I left Coblenz. We were quite alone in our compartment, and I had the happiness of finding him, in regard to his own self and his way of working, more communicative than ever. Beginning by speaking of the events of the past days, we soon drifted into talking about art in general and music in particular.

"There is no real *creating*," he said, "without hard work. That which you would call invention, that is to say, a thought, an idea, is simply an inspiration from above, for which I am not responsible, which is no merit of mine. Yea, it is a present, a gift, which I ought even to despise, until I have made it my own by right of hard work. And there need be no hurry about that, either. It is as with the seed-corn; it germinates unconsciously and in spite of ourselves. When I, for instance, have found the first phrase of a song, say,



I might shut up the book there and then, go for a walk, do some other work, and perhaps not think of it again for months. Nothing, however, is lost. When afterward I approach the subject again, it is sure to have taken shape; I can now begin really to work at it. But there are some composers who sit at the piano with a poem before them, putting music to it from A to Z until it is done. They write themselves into a state of enthusiasm which makes them see something finished, something important, in every bar."

Straight from the station we hurried to the rehearsal for the concert.

Brahms played his own Concerto in D minor magnificently.

After the concert we went to the house of the Princess of Hesse-Barchfeld to supper. Although Brahms, Ernst Franck, the genial composer and conductor, who had come over from Mannheim, and I, were the only non-aristocratic guests present, the affair was very charming and *gemütlich.* Brahms' neighbour at table was the very handsome and fascinating wife of a celebrated general, and this fact, together with the fiery Rhine wine, had a most animating effect on Brahms. After supper, the greater part of the company had a lively game of billiards, and just before leaving, the princess presented Brahms with a handsome box of ebony, to the lid of which a laurel wreath of silver was attached. Each leaf of the wreath had the title of one of Brahms' works engraved on it. He was delighted, though much amused at finding on one of the leaves "Triumphlied," that colossal "Song of Triumph" for double chorus and orchestra, and on the very next one to it, "Wiegenlied," the sweet little lullaby of eighteen bars.

Berlin, Feb. 28, 1876.—Just arrived home from Wiesbaden. Spent another interesting day there with Brahms yesterday. In the morning he went with me to the Landgravine Anna of Hesse, a princess of considerable musical talent, whom, however, as he told me, he mostly admired for her simple and modest, yet extremely cordial and affable manners. Otherwise he does not particularly care for personal intercourse with the "highest spheres of society," as he calls it.

Sassnitz, on the Island of Ruegen, Saturday, July 8, 1876.—Arrived here last night. The diligence was delayed by one of the heaviest thunderstorms I can remember, and did not pull up at the little hostelry which also contains the post-office, until half-past eleven; but in spite of the late hour, Brahms was there to welcome me, and we had an hour's chat in the little coffee-room. Then he returned to his lodgings down in the village, whilst I came up here to the hotel on the Fahrnberg, where, however, Brahms is going to have his midday and evening meals regularly.

Brahms is looking splendid. His solid frame, the healthy, dark brown colour of his face, the full hair, just a little sprinkled with grey, all make him appear the very image of strength and vigour. He walks about here just as he pleases, generally with his waistcoat unbuttoned and his hat in his hand, always with clean linen, but without collar or necktie. These he dons at *table d'hôte* only. His whole appearance vividly recalls some portraits of Beethoven. His appetite is excellent. Evenings he regularly drinks three glasses of beer, always, however, finishing with his beloved kaffee.

July 10.—Yesterday Brahms showed me the manuscript of an unpublished song, and of the first movement of a requiem, both by Schubert, enthusiastically commenting on their beauty. The first two issues of the Bach Society's publication of cantatas were lying

on his table, and he pointed out to me how badly the accompaniments were often arranged for the piano. In the endeavour to bring out as nearly as possible every individual part of the orchestration in its right relation to the whole, the arrangement had well-nigh become unplayable for any but a virtuoso. "The chief aim," he said, "of a pianoforte arrangement of orchestral accompaniments must be to be easily playable. It does not matter at all if the different parts move correctly and in strict accordance with the rules of counterpoint."

Then we went together through the full score of Mozart's "Requiem," which he had undertaken to prepare for a new edition of Mozart's works. I admired the great trouble he had taken in the revision of the score. Every note of Süssmayer's was most carefully distinguished from Mozart's own.

Of the value of the metronome we also spoke, a conversation which was vividly recalled to me years later, when I had come to live in London, and Mr. Otto Goldschmidt (Jenny Lind's husband, and then conductor of the Bach Choir) had requested me to write to Brahms, asking him if the metronome marks at the head of the different movements of his "German Requiem," which Mr. Goldschmidt was about to perform, should be strictly adhered to.

This was Brahms' answer :—

"My dear Henschel," he writes, "I hardly know what answer to give. I think here, as well as with all other music, the metronome is of very little value. As far as my experience goes, most composers have, sooner or later, withdrawn their metronome markings. Those which can be found in my works—good friends have persuaded me to put them there, for I myself have never believed that my blood and a mechanical instrument go very well together.

"The so-called *tempo elastico* is, moreover, not a new invention. *Con discrezione* should be added to that, as to many other things in this world.

"Is this an answer? I know no better one. But what I do know is that, I indicate my *tempi*—without metronome—modestly, to be sure, but with the greatest possible care and clearness.

"Remember me kindly to Mr. Goldschmidt, and say, please, there is only one thing in the coming performance of my Requiem I dislike thinking of, and that is that the soprano solo, 'Ye now are sorrowful,' will not be sung by the conductor's wife. I do wish I could have heard that once from her!"

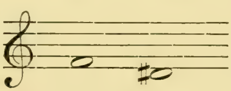
July 11.—I bought a strong hammock yesterday, and Brahms and I went into the lovely beech-wood and hung it up between two trees, on a spot from which, through the foliage, we could see the sea far below us. We both climbed into the hammock simultaneously, an amusing, though by no means an easy task to accomplish. After having comfortably established ourselves in it, we enjoyed a very cosy, agreeable hour or two of *dolce far niente*. Brahms was in an

angelic mood, and went from one charming, interesting story to another, in which the gentler sex played a not unimportant part.

That angelic mood, however, he was very near losing when I asked him if he thought he'd ever go to England on a visit. He said, "No, I shall not easily be persuaded to go to England. I have a great aversion, anyhow, to concerts and similar disquietudes. It has nothing whatever to do with the question whether I like English politics and English globe-trotters or not (the latter, by the way, being nearly outdone now by the North Germans, from Berlin especially). The rumour that I have a special dislike for English concert-rooms is very silly. Into no concert-room I ever go with pleasure; but people must see how it is easier for me, being caught once in a while in the trap of an invitation from Germany or Holland, than undertaking the long journey to England, followed, probably, by a restless and fatiguing stay. No, you really would do me a favour by explaining the matter from time to time as it is."

A little later that afternoon we resolved to go on an expedition to find *his* bull-frog pond, of which he had spoken to me for some days. His sense of locality not being very great, we walked on and on across long stretches of waste moorland. Often we heard the weird call of bull-frogs in the distance, but he would say, "No, that's not *my* pond yet," and on we walked. At last we found it, a tiny little pool in the midst of a wide plain grown with heather. We had not met a human being the whole way, and this solitary spot seemed out of the world altogether.

"Can you imagine," Brahms began, "anything more sad and melancholy than this music, the undefinable sounds of which for ever and ever move within the pitiable compass of a diminished third?



"Here we can realise how fairy tales of enchanted princes and princesses have originated. Listen! There he is again, the poor king's son with his mournful C flat."

(It is interesting to note that in Brahms' songs dating from this period this interval frequently occurs.)

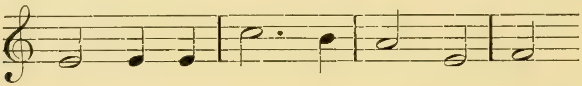
We stretched ourselves out in the low grass—it was a very warm evening—lighted cigarettes, and lay listening in deepest silence, not a breath of wind stirring, for fully half an hour. Then we leaned over the pond, caught tiny little baby frogs, and let them jump into the water again from a stone, which greatly amused Brahms, especially when the sweet little things, happy to be in their element again, hurriedly swam away, using their nimble little legs most gracefully and according to all rules of the natatory art. When they thought themselves quite safe, Brahms would tenderly catch one up again in

his hand, and heartily laugh with pleasure on giving it back its freedom.

During our walk homeward, we spoke almost exclusively of musical matters, and he said : " You must practise more gymnastics, my dear, four-part songs, variations, string quartets, etc. ; that will be beneficial to your opera, too." (I was engaged at that time in writing a tragic opera, " Gerda.") As we parted for the night, he called after me : " Come for me to-morrow morning to go bathing ; and bring new songs, your ' Gerda ' score, or other beautiful things." He was very fond of teasing. So this morning I brought him three new songs of mine.

The afternoon was again spent in the hammock, and on the way home we came to talk of Wagner's trilogy, " The Ring of the Nibelungs." I had just spoken of some, to me, especially beautiful places in the first act of " The Valkyrie," and of the fresh and breezy song of Siegfried in " Siegfried," " From the wood forth into the world fare."

" Certainly," he said, " these are fine things, but I can't help it, somehow or other, they do not interest me. What you just hummed



is no doubt, beautiful ; and when Siegmund in the ' Valkyrie ' pulls the sword out of the tree, that's fine too ; but it would, in my opinion, be *really* powerful and carry one away, if it all concerned—let us say, young Bonaparte, or some other hero who stands nearer to our sensibilities, has a closer claim to our affection. And then that stilted bombastic language." He took a copy of the text-book. " Listen :

> " By Brynhild's rock
> Your road shall be bent ;
> Who roars yet round it,
> Loge warn him to Valhall !
> For with doom of Gods
> Is darkened the day ;
> So—set I the torch
> To Vallhall's towering walls."

He recited the words with greatly exaggerated pathos. " If I read this to a counting-house clerk, I am sure it would make a tremendous impression. ' So—I set the torch ' . . . *I* do not understand this kind of thing. What really does happen with the ring ? Do *you* know ? And those endless and tedious duets ! Look at even Goethe's ' Tasso,' a masterpiece of the first rank. Every word in it is pure gold ; yet the long duets in it, though fine reading, prevent the play from being interesting as a drama."

July 12.—I went to Brahms' rooms last night. He had been reading, but, putting away his book, gave me a cordial welcome and began looking through my new manuscript songs. He took up the one in E flat, "Where Angels hover," and said, "Now there is a charming song. In some of the others you seem to me too easily satisfied. One ought never to forget that by actually perfecting *one* piece, one gains and learns more than by commencing or half-finishing a dozen. Let it rest, let it rest, and keep going back to it and working at it over and over again, until it is completed as a finished work of art, until there is not a note too much or too little, not a bar you could improve upon. Whether it is beautiful also is an entirely different matter, but perfect it *must* be. You see, I am lazy, but I never cool down over a work, once begun, until it is perfected, unassailable."

Thus he continued speaking, drawing, in the most amiable way, my attention to little defects here and there, so that I sat happy and silent, careful not to interrupt this to me so precious lesson.

July 13.—I asked him yesterday if he had thought of going to the inauguration performance of "The Nibelungs' Ring" at Bayreuth in August. "I am afraid," he said, "it's too expensive. I have repeatedly heard 'Rheingold' and 'Walkure' at Munich, and confess it would greatly interest me, but—well, we'll think of it." Then, taking up the volume of Hauptmann's letters I had lent him, and pointing to one of them, he said: "Just look; do you see these asterisks instead of a name?" I did, and read the whole sentence, which described a certain composer, indicated by the asterisks, as a rather haughty young man. "That's me," said Brahms. "When I was a very young man I remember playing, at Göttingen, my 'Sonata in C' to Hauptmann. He was not very complimentary about it; in fact, had much fault to find in it, which I, a very modest youth at that time, accepted in perfect silence. I afterwards heard that this silence had been interpreted and complained of, as haughtiness. I confess, the more I read of these letters, the clearer it becomes to me that they were written with a certain consciousness of importance. Beethoven would have laughed if any one, seeing in one of his letters a remark on any subject whatever, had taken this as an absolute proof of the justice of such a remark. But there are people—take for instance, Varnhagen—who, never having accomplished anything really great themselves, sit down at their writing desks in a peevish, sulky temper, pulling to pieces—even when praising—everything they can lay hold of. To twaddle about Bach or Beethoven, as is done in the letters to Hauser, in a feuilletonistic way is wholly unnecessary: they stand too firm for that kind of thing."

July 14.—Last evening we sat downstairs in the coffee room, having supper, when suddenly someone in the adjoining dining-hall began to play Chopin's Study in A flat on the piano. I sprang up, intending to put a stop to it, and exclaiming "Oh, these women!"

when Brahms said, "No, this is no woman." I went into the hall to look and found he was right. "Yes," he said, "in this respect I am hardly ever mistaken; and it is by no means easy to distinguish, by sense of hearing alone, a feminine man from a masculine woman."

July 15.—Yesterday morning I took Brahms the orchestral score of Wagner's "Gotterdammerung," In the afternoon he said to me, "*Why* did you bring it to me?" (He had particularly asked me for it!) "The thing interests and fascinates one, and yet, properly speaking, it is not always pleasant. With the 'Tristan' score it is different. If I look at that in the morning, I am cross for the rest of the day."

I well remember my wondering at the time just what meaning Brahms intended to convey by these words. My old friend, Herr Max Kalbeck, Editor of the *Neues Tagblatt*, in Vienna, who published excerpts from my diary in that paper some years ago, makes the following comment on them:—

"This sentence needs an explanation, as it could easily be interpreted as meaning that 'Tristan,' in contrast to the 'not always pleasant' 'Ring of the Nibelungs,' had pleased Brahms very much, so much, indeed, that it made him cross, out of envy. We know, from personal experience, that Brahms, though warmly acknowledging the many musical beauties of the work, had a particular dislike for 'Tristan,' and as to envy, he never in his life envied anyone. In Wagner he admired, above all, the magnitude of his intentions and his energy in carrying them out. The Bayreuth Festival Theatre he hailed as a national, all-German affair. We believe the chief reason why Brahms never went to Bayreuth is to be found in the circumstance that the performance always happened at a season when he, after long and arduous creative work, was wont to give himself up entirely to the recreation of an out-of-door life in the country."

In celebration of the sixth anniversary of the declaration of war between France and Germany, we ordered a bottle of champagne. Brahms had talked himself into a tremendous patriotism, and told me that his first thought, when the war was declared, was to go to Mme. Schumann, who resided, without the protection of a man, at Baden-Baden.

"So great was my enthusiasm," he said, "that I was firmly resolved to join the army, after the first great defeat, as a volunteer, fully convinced I'd meet my old father there to fight side by side with me. Thank God! it turned out differently."

Yesterday I was with Brahms from noon until eleven at night without interruption. He was in excellent spirits. We had our swim in the sea together, and again amused ourselves and each other by diving for little red pebbles. After the midday dinner, Brahms was lying in my room, in the hammock which I had secured between window and door, while I read to him Meilhac's amusing comedy,

"L'Attaché." After the usual coffee at a coffee-house on the beach, we went for a long stroll in the Park, near Crampas, the nearest village. We spoke, among other things, of Carl Loewe. Brahms thinks highly of his ballads and Servian songs. "However, with us in Vienna," he said, "Loewe is, to my regret, much overrated. One places him, in his songs, side by side with, in his ballads above, Schubert, and overlooks the fact that what with one is genius, with the other is simply talented craft. . . . "

"In writing songs," he cautioned me, "you must endeavour to invent, simultaneously with the melody, a healthy, powerful bass. You stick a little too much to middle parts. In that song in E flat, for instance (he again referred to 'Where Angels hover'), you have hit upon a very charming middle part, and the melody is very nice, too, but that isn't all, is it? And then, my dear friend, no heavy dissonances on the unaccentuated parts of the bar, please. That is weak. I am very fond of dissonances, you'll agree, but on the heavy, accentuated parts of the bar, and then resolve them easily and gently."

Speaking of Schubert's setting of Goethe's songs, he said, "Schubert's Suleika songs are to me the only instances where the power and beauty of Goethe's words have been enhanced by the music. All other of Goethe's poems seem so perfect in themselves that no music can improve them." (An opinion, by the way, which I could not share; to me there is no sentiment expressed in words which music, i.e., the right music, may not enhance.)

Passing from music to literature, he remarked: "Paul Heyse used to be one of the most charming men imaginable. He was beautiful and exceptionally amiable, and I hardly know of any one, who, suddenly entering a room, would illuminate it, so to speak, by his personality, as did Heyse.

"Bodenstedt is greatly overrated: his poetry is my special aversion. Geibel, on the other hand, seems to me not appreciated enough."

Perhaps I may be allowed here to interrupt the reading of the diary for a moment, and to draw your attention to the discretion and judiciousness with which Brahms selected the words for his songs.

If we look at the texts to his vocal music, of which there exists a vast mass, we shall find that the sources—individual or national—from which he drew his inspiration, have in themselves been inspired. All his vocal compositions—from the "Requiem" down to the simplest song—are set to beautiful, significant, worthy poems, truly a wonderful lesson to young composers.

For if one of the chief aims of art be to elevate, i.e. to raise mankind for the time being above the commonplace routine of life, above paltry, everyday thoughts and cares, in short, from things earthy to things celestial, surely such aim should be discernible even in the smallest form of the expression of art.

Just as the incomprehensible greatness of the divine power, and

the beauties of nature reveal themselves as convincingly in a little primrose as in the huge trees of the Yosemite Valley, in the sweet prattling of a little brooklet as in the roaring thunder of the Niagara, in the lovely undulations of the Kentish hills as in the awe-inspiring heights of the Himalayas, so beauty of soul, honesty of purpose, purity of mind, can shine as brightly in the shortest song as in the longest symphony.

No really true artist then, in the realm of music, will debase his muse by wedding it to any but true poetry, by putting music to words as far removed from poetry as a molehill from Mount Parnassus.

It seems, however, no easy thing, especially for younger people, to distinguish the real from the sham, sentiment from sentimentality. If the latter be superficial, aimless pity, affected, unwholesome, false emotion, sentiment, on the other hand, is true emotion, is the genuine feeling that grows out of the contemplation of a thing; and it is that sentiment, which is a fit object for poetic and musical expression, which we ought to look for even in a little song. A true artist's spirit will not allow itself to be moved by versified penny-a-liner newspaper reports, like the capsizing of a little pleasure boat with two lovers in it; or the death of a poor, emaciated seamstress, ready to join her lover in heaven. A true artist's choice will be the poetic embodiment of sentiment.

The standing of the pale, hungry little boy outside the window of a confectioner's shop, and observing inside the shop the rich, ruddy little boy eating his fill, that is not poetry, even if put into faultless verse and rhyme, but simply a fact, and a sad one, too, the contemplation of which might in a fine poetic mind produce the most beautiful sentiments of compassion with the sufferings of our fellow creatures, of tenderness, of love; but to let the poor little chap go straightway to heaven, to the fortissimo accompaniment of triplets on the last page of an up-to-date ballad, that is sentimentality and cruel mockery into the bargain.

I well remember what fun Brahms and I had in later years, when I showed him some specimens of the typical popular modern English ballad, and how we laughed—especially over the sad ones!

But to return to the rest of the diary.

After supper, we sat, quite alone in the dark, on the terrace of the Fahrnberg. Soon our conversation took a more serious turn. He spoke of friendship, and of men, and how, properly speaking, he believed very little in either.

"How few true men there are in the world!" he exclaimed. "The two Schumanns, Robert and Clara—there you have two true, beautiful *Menschenbilder* (images of man). Knowledge, achievement, power, position—nothing can outweigh this: to be a beautiful Menschenbild. Do you know Allgeyer, in Munich? He is one, too." And then he began to talk with touching warmth of the time when, in Allgeyer's house at Karlsruhe, he wrote his "Mainacht," and the

D-minor movement of his " Requiem." . . . . " I sometimes regret," he said to me, after some moments of silence, " that I did not marry. I ought to have a boy of ten now ; that *would* be nice. But when I was of the right age for marrying, I lacked the position to do so, and now it is too late."

Speaking of this had probably revived in him reminiscences of his own boyhood, for he continued : " Only once in my life have I played truant and shirked school, and that was the vilest day of my life. When I came home, my father had already been informed of it, and I got a solid hiding. But still," he said, " my father was a dear old man, very simple-minded and unsophisticated, of which I must tell you an amusing example.

" You know he was double-bass player in the Municipal Orchestra of Hamburg, and also copied music.

" He was sitting in his room at the top of the house one day, with the door wide open, busily engaged copying music, when in walked a tramp, begging. My father looked up at him quietly, and, in his nice Hamburg dialect, said, ' I cannot give you anything, my dear man. Besides, don't you know that it is very wrong of you to come into a room like this ? How easily might you not have taken my overcoat which is hanging in the hall ! Get out ! ' The tramp humbly apologised and withdrew. When a few hours later my father wanted to go out, the overcoat, of course, was gone ! " He then touched upon his relations to the members of his family, and told me he still supported his old stepmother. With his sister he had little in common ; their interests had always been too far apart. Between his brother, whom he had likewise supported, and himself, there existed no intercourse whatever.

The other day I happened to hum the andante from his " Quartet in C minor." He seemed rather to like my doing so, for when it came to the place :—

he accompanied my humming with gentle movements of his hand, as if beating time to it. At last he smilingly said : " Well, I am not at all ashamed to own that it gives me the keenest pleasure if a song, an adagio, or anything of mine, has turned out particularly good. How must those gods, Bach, Mozart, Beethoven, etc., have felt whose daily bread it was to write things like the ' Matthew Passion,' ' Don Giovanni,' ' Fidelio,' ' Ninth Symphony ! ' What I cannot understand is how people like myself can be vain. As much as we men, who walk upright, are above the creeping things of the earth, so these gods are above us. If it were not so ludicrous it would be loathsome to me

to hear colleagues of mine praise me to my face in such an exaggerated manner."

Thus he went on; it was no longer modesty, it was humility, and I took good care not to disturb his mood by a single word.

Soon, however, he smiled again, and remarked, among other things, that he considered the agitato from his still unpublished "Quartet in B flat" the most amorous, affectionate thing he had written.

When we parted that night, he said: "You *will* write me from Bayreuth, won't you? I know you will rave about it, and I don't blame you. I myself must confess 'Walküre' and 'Gotterdammerung' have a great hold on me. For 'Rheingold' and 'Siegfried' I do not particularly care. If I only knew what becomes of the Ring and what Wagner means by it! Perhaps the cross? Hebbel, in his 'Nibelunge,' has dared it, and perhaps it was Wagner's meaning too. I am by no means a fanatic devotee, but that, at least, would be an idea—thus to indicate the termination of the reign of the gods."

July 18.—At luncheon, as it was my last day, we had a bottle of champagne between us. In the afternoon, the other guests having partly retired to their rooms, partly gone on excursions, Brahms played the accompaniments to some songs for me. Since our arrival this was the first time that he had touched the keyboard and that I had sung. I began with Brahms' "Mainacht," then came a Schubert song, and then Beethoven's cyclus, "To the Absent Beloved." When we had ended we were surprised to find that all of the adjoining rooms had filled with listeners. Mine host of the Fahrnberg was greatly touched, and thanked Brahms for the honour he had done to his house.

In the train to Berlin, July 19.—This morning, at five o'clock, I left Sassnitz. Strangely enough, it again poured in torrents as on the night of my arrival. A horrid, chilly morning. Brahms was up at the Fahrnberg a little before five, and, to my delight, accompanied me in the diligence as far as Lancken, some three miles from Sassnitz. There he got out, we shook hands, and parted. For a long time I looked after him out of the carriage window in spite of the still pouring rain. It was a picture never to be forgotten. As far as the eye could reach, nothing but moor, and clouds, and—Brahms.

Here closes the journal. During the twenty-one years of undisturbed friendship, our intercourse had to be more by letter, our meetings fewer and farther between; the channel and, later the Atlantic separating us bodily.

There have lived great artists who have been small men. In Brahms, both the artist and the man aspired to great and lofty ideals.

He never aimed at gaining for himself—through glittering, dazzling play with tones—the quickly fading crowns of popular favour.

He never coveted fame and applause. There were, next to his art, other things his whole being comprised with a strong love. Himself of a childlike disposition, he loved children. To make children, poor or rich, happy, was to himself pure happiness. He loved the poor, to whom his heart went out in sympathy and pity. Where he could comfort in silence those who suffered in silence, those who struggled against undeserved misfortune, the sick and the helpless, there the man, so modest, sparing and unpretentious in his own wants, became a benefactor, ready for sacrifice.

He has left us a precious inheritance: the noble example of a rare truthfulness and simplicity in art and life; of a relentless severity toward himself; of a hatred of self-conceit and pretence; of a high-minded, inflexible, unwavering artistic conviction. To him may be truly applied Goethe's fine words in the Epilogue to Schiller's "Lay of the Bell"—

> "With mighty steps his soul advanced
> Toward the ever true—good—beautiful."

[G. H.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1905.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

E. Hurry Fenwick, Esq., F.R.C.S.
Oswald Lewis, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. L. F. Everest, M.A. LL.D. *M.R.I.*, for his Gift of a Portrait of Colonel Sir George Everest, C.B. F.R.S.

It was announced that the legacy of £200, bequeathed by the late Mr. John Cohen, had been received.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same viz. :—

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WEEKLY EVENING MEETING,

Friday, June 9, 1905.

Sir William Crookes, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Sir William H. White, K.C.B. LL.D. D.Sc. F.R.S. M. Inst. C.E. *M.R.I.*

Submarine Navigation.

Submarine navigation has engaged the attention of inventors and attracted general interest for a very long period. Its practical application to purposes of war was made about 130 years ago. The main object of that application was to threaten, or if possible destroy, an enemy's battleships engaged in blockade by means of under-water attacks, delivered by vessels of small dimensions and cost, which could dive and navigate when submerged. From the first, submarines were admittedly weapons favoured by the weaker naval power; and as a consequence their construction found little favour with our naval authorities. Under the conditions which prevailed a century ago in regard to materials of construction, propelling apparatus and explosives, the construction of submarines necessarily proceeded on a limited scale, and the type practically died out of use, almost at its birth. Enough had been done, however, to demonstrate its practicability and to make it a favourite field of investigation for inventors, some of whom contemplated wide extensions of submarine navigation. Every naval war gave fresh incentive to these proposals, and led to the construction of experimental vessels. This was the case during the Crimean War, when the Admiralty had a submarine vessel secretly built and tried by a special committee, on which, amongst others, Mr. Scott-Russell and Sir Charles Fox served. Again, during the Civil War in America, the Confederates constructed a submarine vessel, and used it against the blockading squadron off Charlestown. After several abortive attempts, and a considerable loss of life, they succeeded in destroying the Federal *Housatonic*, but their submarine with all its crew perished in the enterprise.

It is impossible to give even a summarised statement of other efforts made in this direction from 1860 onwards to 1880; but one cannot leave unnoticed the work done in the United States by Mr. Holland, who devoted himself for a quarter of a century to continuous experiment on submarines, and eventually achieved success. The Holland type was first adopted by the United States Navy, and was subsequently accepted by the British Admiralty as the point of de-

parture for our subsequent construction of submarines. In France also successive designs for submarines were prepared by competent naval architects, and a few vessels were built and tried. The *Plongeur*, of 1860, was a submarine of large size, considerable cost, and well-considered design; but her limited radius of action and comparatively low speed left her for many years without a successor on the French Navy List. The high relative standing attained by the French Navy as compared with our own, in consequence of the vigorous action of the Emperor Napoleon III. in developing steam propulsion and armour protection for sea-going ships, no doubt greatly influenced French policy at that time, and delayed development of submarine construction. When conditions were altered in consequence of the Franco-German War of 1870, and the position of the French Navy in relation to the British became less favourable, it was natural that the question of submarine construction should assume greater importance in France. In the interval, moreover, great advances had been made in materials of construction and in means of propulsion available for submarines. The extended use of steel, and the practical applications of electricity gave to designers greater facilities than existed previously, and public interest in the construction of submarines and small swift vessels was increased by the writings of the *jeune école*, who strongly condemned the continued construction of armoured "mastodons."

The modern development of submarines for war purposes is chiefly due to French initiative. During the earlier stages of this development progress was extremely slow. The *Gymnote* was ordered in 1886 and the *Gustave Zédé* in 1888, and her trials continued over nearly eight years, large sums of money being spent thereon. In 1896 competitive designs for submarines were invited, but no great activity was displayed in this department of construction until the Fashoda incident two years later. Since that time remarkable developments have been made in France, considerable numbers of submarines have been laid down, rival types have been constructed, and many designers have been engaged in the work. Up to the present time about seventy submarines and submersibles have been ordered; in July 1904 the total number of completed vessels was twenty-eight; and at the end of 1907 it is estimated that France will possess sixty completed submarines, with a total displacement of nearly 13,600 tons. The first French submarine of modern type, the *Gymnote*, was 56 feet long, and of 30 tons displacement. The latest types are nearly 150 feet long and of 420 tons displacement. The cost of a French submarine designed in 1898 was about 26,000*l*. The estimated cost of the latest and largest vessels is about 70,000*l*. The French have pursued no continuous policy in this development, but have alternated between vessels of comparatively large, and others of much smaller displacement. This course had much to recommend it, no doubt, as it brought many accomplished naval architects into competition; but the lack of a continuous and progressive policy has resulted in dissatisfaction and difficulty, and this is frankly acknow-

ledged by French authorities. Two years elapsed after the date when the French resolutely undertook the construction of submarines before the British Admiralty ordered five vessels of the Holland type from Messrs. Vickers, Maxim and Co., who had acquired the concession for the use of the Holland Company's patents. These first vessels in essentials were repetitions of the type which had been tried and officially approved by the authorities of the United States Navy. It was agreed that all improvements made by the Holland Company should be at the service of the British Admiralty through the English *concessionaires*. In this manner the Royal Navy at once acquired advantages attaching to the long experience and great skill of Mr. Holland; and with that advantage there was associated the possibility of utilising their own technical resources and those of Messrs. Vickers, Maxim and Co. For five years a continuous policy has been followed in the development of our submarines, all of which have been constructed at Barrow-in-Furness. There has been a great development in size, speed and general efficiency, resulting necessarily in correspondingly greater cost per vessel. Information of an official and authoritative character relating to submarines is freely published in France and the United States, but for British submarines, corresponding official information is scanty. It has for years been the rule to give in the Navy Estimates full particulars of dimensions and costs for all other classes of British warships; but for submarines a policy of secrecy is adopted that is most unreasonable and unnecessary. From the best sources of information accessible, it appears that the growth in size, with a correspondingly increased cost has been even more rapid here than in France. Our first five submarines are 63 feet in length, 120 tons in displacement, with gasolene engines of 160 horse-power for surface propulsion, giving a speed of 8 to 9 knots. The electric motors for submerged propulsion are estimated to give a speed of about 7 knots. The contract price for each vessel in the United States was about 34,000*l*., and that is about the price paid for our earliest vessels. The latest type of which particulars are available are said to be about 150 feet in length, 300 tons in displacement, and with gasolene engines of 850 horse-power for surface propulsion, giving a surface speed of 13 knots and a radius of action of 500 miles. The under-water speed is 9 knots, and the radius of action when submerged about 90 miles. No official particulars have been published as to the contract price for these vessels, which is certainly an undesirable course to adopt, seeing that for other, and admittedly sufficient reasons these contracts have not been subject to competition as yet. It may be hoped that the Admiralty will reconsider this matter and treat submarines similarly to other vessels.

In French official classification a distinction is made between submarines and submersibles, and this terminology has been the cause of some confusion. Both classes are capable of diving when required, and both can make passages at the surface. In this surface condition a considerable portion of the vessel lies above the water-

surface and constitutes what is technically called a "reserve of buoyancy." In the submersible this reserve of buoyancy and the accompanying freeboard is greater than in the submarine type, and in this respect lies the chief difference between the two types. The submersible has higher freeboard and greater reserve of buoyancy, which secure better seagoing qualities, and greater habitability. The deck or platform is situated higher above water, and to it the crew can find access in ordinary weather when making passages, and obtain exercise and fresh air. Recent exhaustive trials in France are reported to have established the great superiority of the submersible type when the service contemplated may involve sea passages of considerable length. The French policy, as recently announced, contemplates the construction of submersibles of about 400 tons displacement for such extended services, and proposes to restrict the use of submarines to coast and harbour defence for which vessels of about 100 tons displacement are to be employed. All recent British submarines would be ranked as submersibles according to the French classification, and it is satisfactory to know, as the result of French experiments, that our policy of construction proves to have distinct advantages. In addition to these two types of diving- or submarine-vessels the French are once more discussing plans which have been repeatedly put forward and practically applied by M. Goubet, namely, the construction of small portable submarine vessels which could be lifted on board large ships and transported to any desired scene of operations. In the Royal Navy for many years past, it has been the practice to similarly lift and carry second-class torpedo or vedette boats about 20 tons in weight. Lifting appliances for dealing with these heavy boats have been designed and fitted in all our large cruisers and in battleships, and a few ships have been built as "boat-carriers." The first of these special depôt ships in the Royal Navy was the *Vulcan* ordered in 1887–8, the design being in essentials that prepared by the writer at Elswick in 1883. The French have also built a special vessel named the *Foudre* which has been adapted for transporting small submarines to Saigon, and performed the service without difficulty. Whether this development of small portable submarines will take effect or not remains at present an open question, but there will be no mechanical difficulty either in the production of the vessels themselves or in the means for lifting and carrying them. M. Goubet worked out with complete success designs for vessels about 26 feet long and less than 10 tons displacement, with speeds of 5 to 6 knots, the trials of which have been very fully described, but French authorities have not adopted the type, and no decision seems to have been taken to introduce it. In this country no similar action has been taken, and our smallest submarines weighing 120 tons cannot be regarded as "portable." Indeed, some leading British authorities on submarines have indicated that experience is adverse to the construction of vessels in which not more than two or three men would form the crew, and on that ground have condemned

the construction of these small submarines. They would necessarily be of slow speed and very limited radius of action, while their efficient working would depend upon the nerve and skill of only two or three men working in a very confined space.

Progress in mechanical engineering and in metallurgy has been great since Bushnell constructed and used his first submarine in 1776, during the war between the United States and this country. These advances have made it possible to increase the dimensions, speed and radius of action of submarines; their offensive powers have been enlarged by the use of locomotive torpedoes; and superior optical arrangements have been devised for discovering the position of an enemy while they themselves remain submerged. But it cannot be claimed that any new principle of design has been discovered or applied. From descriptions left on record by Bushnell, and still extant, it is certain that he appreciated, and provided for the governing conditions of the design in regard to buoyancy, stability, and control of the depth reached by submarines. Indeed, Bushnell showed the way to his successors in nearly all these particulars, and—although alternative methods of fulfilling essential conditions have been introduced and practically tested—in the end Bushnell's plans have in substance been found the best. The laws which govern the flotation of submarines are, of course, identical with those applying to other floating bodies. When they are at rest and in equilibrium they must *displace* a weight of water equal to their own total weight. At the surface they float at a minimum draught and possess in this "awash" condition a sufficient freeboard and reserve of buoyancy to fit them for propulsion. When submarines are being prepared for "diving" water is admitted to special tanks, and the additional weight increases immersion, and correspondingly reduces reserve of buoyancy. In some small submarines comparative success has been attained in reaching and maintaining any desired depth below the surface simply by the admission of the amount of water required to secure a perfect balance between the weight of the vessel and all she contains, and the weight of water which would fill the cavity occupied by the submarine when submerged. For all practical purposes and within the depths reached by submarines on service water may be regarded as *incompressible*; the submarine should, therefore, rest in equilibrium at any depth if her total weight is exactly balanced by the weight of water displaced. If the weight of the vessel exceeds by ever so small an amount the weight of water displaced, that excess constitutes an accelerating force tending to sink the vessel deeper. On the contrary, if the weight of water displaced exceeds by ever so small an amount the total weight of the vessel, a vertical force is produced tending to restore her to the surface. Under these circumstances, it is obvious that if the admission or expulsion of water from internal tanks (or the extrusion or withdrawal of cylindrical plungers for the purpose of varying the displacement) were the only means of controlling vertical movement, it would be exceedingly

difficult to reach or to maintain any desired depth. This difficulty was anticipated on theoretical grounds, and has been verified on service—in some cases, with considerable risks to the experimentalists —the submarines having reached the bottom before the vertical motion could be checked. It has consequently become the rule for all submarines to be left with a small reserve of buoyancy when brought into the diving condition. Submergence is then effected by the action of horizontal rudders controlled by operators within the vessels. Under these conditions, submergence only continues as long as onward motion is maintained, since there is no effective pressure on the rudders when the vessel is at rest. The smallest reserve of buoyancy should always bring a submarine to the surface if her onward motion ceases, and, as a matter of fact, in the diving condition that reserve is extremely small, amounting to only 300 lbs. (equivalent to 30 gallons of water) in vessels of 120 tons total weight. This is, obviously, a narrow margin of safety, and necessitates careful and skilled management on the part of those in charge of submarines. A small change in the density of the water, such as occurs in an estuary or in the lower reaches of a great river, would speedily obliterate the reserve of buoyancy and cause the vessel to sink if water was not expelled from the tanks. Moreover, variations in weight of the submarine (due to the consumption of fuel, the discharge of torpedoes or other causes) must sensibly affect the reserve of buoyancy, and arrangements must be made to compensate for these variations by admitting equal weights of water in positions that will maintain the "trim" of the vessel. Additional safeguards against foundering have been provided in some submarines by fitting detachable ballast. The more common plan is to make arrangements for rapidly expelling water from the tanks either by means of pumps or by the use of compressed air. In modern submarines, with locomotive torpedoes, compressed air is, of course, a necessity, and can be readily applied in the manner described if it is desired to increase their buoyancy.

The conditions of stability of submarines when diving, are also special. At the surface, owing to their singular form, the longitudinal stability is usually much less than that of ordinary ships. When submerged, their stability is the *same in all directions*, and it is essential that the centre of gravity shall be kept below the centre of buoyancy. This involves no difficulty, because water-ballast tanks can be readily built in the lower portions of the vessels. Small stability in the longitudinal sense, however, necessitates great care in the maintenance of trim, and in the avoidance of serious movements of weights within the vessels. Moreover, when a vessel is diving under the action of her longitudinal rudders, she is extremely sensitive to changes of trim, and great skill is required on the part of operators in charge of working the rudders. As the under-water speed is increased, the pressure on the rudders for a given angle increases as the *square* of the velocity, and sensitiveness to change of trim becomes

greater. This fact makes the adoption of higher under-water speed a matter requiring very serious consideration. Some authorities, who have given great attention to the construction of submarines, have been opposed to the adoption of high speeds under water, because of the danger that vessels when diving quickly may reach much greater depths than are desirable. Causes of disturbance which might be of small importance when the under-water speed is moderate, may have a greatly exaggerated effect when higher speeds are reached. Cases are on record where modern submarines in the hands of skilled crews have accidentally reached the bottom in great depths of water, and have had no easy task to regain the surface. For these reasons, it is probable that while speeds at the surface will be increased, under-water speeds will not grow correspondingly. Indeed, the tactics of submarines hardly appear to require high speed under water, seeing that it is an important element in successful attack to make the final dive at a moderate distance from the enemy. It is authoritatively stated that in our submarines complete control of vertical movements has been secured by means of skilled operators, and that a constant but moderate depth below the surface can be maintained. Proposals have been made and successfully applied to small submarines for automatically regulating the depth of submergence by apparatus similar to that used in locomotive torpedoes. For the larger submarines now used such automatic apparatus does not find favour, and better results are obtained with trained men.

The possibility of descending to considerable depths has to be kept in view when deciding on the form and structural arrangements of submarines, which may be subjected accidentally to very great external pressure. It is absolutely necessary to success that, under the highest pressure likely to be endured, there shall be rigidity of form, as local collapse of even a very limited amount might be accompanied by a diminution in displacement that would exceed the reserve of buoyancy. This condition is not difficult of fulfilment, and the approximately circular form usually adopted for the cross-sections of submarines favours their resistance to external pressure.

Under former conditions, there was difficulty in remaining long under water without serious inconvenience from the impurity of the air. Now, by suitable arrangements and chemical appliances, a supply of pure air can be obtained for considerable periods, sufficient indeed for any operations likely to be undertaken.

The use of gasolene engines for surface propulsion has many advantages. It favours increase in speed and radius of action, and enables submarines to be more independent and self-supporting. Storage batteries can be recharged, air compressed and other auxiliary services performed independently of any "mother" ship. At the same time, it is desirable to give to each group of submarines a supporting ship, serving as a base and store depôt, and this has been arranged in this country as well as in France. With gasolene engines, care must be taken to secure thorough ventilation and to avoid the

formation of explosive mixtures of gas and air, otherwise accidents must follow.

Little information is available as regards the success of "periscopes" and other optical instruments which have been devised for the purpose of enabling those in command of submarines to obtain information as to their surroundings when submerged. In this department, secrecy is obviously desirable, and no one can complain of official reticence. From published accounts of experimental working abroad as well as in this country, it would appear that considerable success has been obtained with these optical instruments in comparatively smooth water. It is also asserted that when the lenses are subjected to thorough washing by wave-water, they remain efficient. On the other hand, the moderate height of the lenses above water must expose them to the danger of being wetted by spray even in a very moderate sea, and experience in torpedo-boats and destroyers places it beyond doubt that the resultant conditions must greatly interfere with efficient vision. In heavier seas, the comparatively small height of the lenses above water must often impose more serious limitations in the use of the periscopes and similar instruments. Improvements are certain to be made as the result of experience with these optical appliances, and we may be sure that in their use officers and men of the Royal Navy will be as expert as any of their rivals. But when all that is possible has been done, it must remain true that increase in offensive power and in immunity from attack obtained by submergence will be accompanied by unavoidable limitations as well as by special risks resulting from the sacrifice of buoyancy and the great reduction in longitudinal stability which are unavoidable when diving. These considerations have led many persons to favour the construction of so-called *surface-boats* rather than submarines. They would resemble submersibles in many respects, but the power of diving would be surrendered, although they would be so constructed that by admitting water by special tanks they could be deeply immersed and show only a small target above the surface when making an attack. There would be no necessity in such surface vessels to use electric motors and storage batteries, since internal combustion engines could be used under all circumstances. Hence it would be possible without increase of size to construct vessels of greater speed and radius of action and to simplify designs in other important features. It is not possible to predict whether this suggestion to adopt surface-boats rather than submersibles will have a practical result; but it is unquestionable that improvements in or alternatives to internal combustion engines will favour the increase of power in relation to weight, and so will tend to the production of vessels of higher speed. The comparatively slow speed of existing submarines as compared with destroyers and torpedo-boats of ordinary types admittedly involves serious limitations in their chances of successful attack on vessels under way, and higher surface speeds are desirable.

Concurrently with the construction of submarines, experiments have been made in this country and abroad to discover the best means of defence against this method of attack. Here again authentic details are necessarily wanting, since the various naval authorities naturally wish to keep discoveries to themselves. It is very probable, however, that published accounts of tests between swift destroyers, vedette boats and submarines are not altogether inaccurate, and according to these accounts the periscopes of submarines have been found useful by assailants as the means of determining the position of the submarines, and aiding their entanglement. Comparatively limited structural damage to a submarine in the diving condition may be accompanied by an inflow of water in a short period, which will result in the loss of the vessel. The accident to Submarine A 1, which was struck by a passing mail steamer, illustrates this danger. It is reasonable to accept the published reports that large charges of high explosives exploded at a moderate distance may have a serious effect against submarines, and cause them to founder. Their small reserve of buoyancy in the diving condition makes them specially liable to risks of foundering rapidly, and little more than a crevice may practically fill the interior with water in a very short time when the vessel is submerged even to a moderate depth. On the other hand, reports which have appeared of the manœuvres in France and elsewhere, when attacks have been made by submarines on vessels at anchor or under way, show a considerable percentage of successes. Such exercises are valuable no doubt for purposes of training, but under peace conditions it is necessary to avoid the risks of damage to submarines, which might easily become serious if the defence were pressed home as it would be in war. When the officers and crews of submarines know that they will be treated more considerately than in real warfare, they will naturally take chances, and make attacks involving possible destruction under the conditions of a real action. In short, naval manœuvres in this department, while they may be useful in increasing the skill and confidence of officers and men in the management of submarines, can be no real test of fighting efficiency.

Submarines and airships have certain points of resemblance, and proposals have been made repeatedly to associate the two types, or to use airships as a means of protection from submarine attacks. One French inventor seriously suggested that a captive balloon attached to a submarine should be the post of observation from which information should be telephoned to the submarine as to the position of an enemy. He evidently had little trust in periscopes, and overlooked the dangers to which the observers in the car of the balloon would be exposed from an enemy's gun-fire. Quite recently a proposal has been made by M. Santos Dumont to use airships as a defence against submarines; his idea being that a dirigible airship of large dimensions and moving at a considerable height above the surface of the sea, could discover the whereabouts of a submarine, even at some depth

below the surface, and could effect its destruction by dropping high explosive charges upon the helpless vessel. Here again, the inventor, in his eagerness to do mischief, has not appreciated adequately the risks which the airship would run if employed in the manner proposed, as submarines are not likely to be used without supporting vessels. Hitherto, submarines themselves have been armed only with torpedoes, but it has been proposed recently to add guns, and this can be done, if desired, in vessels possessing relatively large freeboard. No doubt if gun armaments are introduced, the tendency will be to further increase dimensions and cost, and the decision will be governed by the consideration of the gain in fighting power as compared with increased cost. As matters stand, submarines are practically helpless at the surface when attacked by small swift vessels, and it is natural that advocates of the type should desire to remedy this condition. Surface boats, if built, will undoubtedly carry guns as well as torpedoes, and in them the gun fittings would be permanent, whereas in submarines certain portions of the armament would have to be removed when vessels were prepared for diving.

Apart from the use of submarine vessels for purposes of war, their adoption as a means of navigation has found favour in many quarters. Jules Verne in his "Twenty Thousand Leagues Under the Sea," has drawn an attractive picture of what may be possible in this direction, and others have favoured the idea of combining the supposed advantages of obtaining buoyancy from bodies floating at some depth below the surface with an airy promenade carried high above water. Not many years ago an eminent naval architect drew a picture of what might be accomplished by utilising what he described as the "untroubled water below" in association with the freedom and pure air obtainable on a platform carried high above the waves. These suggestions, however, are not in accord with the accepted theory of wave motion, since they take no note of the great depths to which the disturbance due to wave-motion penetrates the ocean. The problems of stability, incidental to such plans, are also of a character not easily dealt with, and consequently there is but a remote prospect of the use of these singular combinations of submarine and aerial superstructures. There is little likelihood of the displacement of ocean steamships at an early date by either navigable airships or submarines, and the dreams of Jules Verne or Santos Dumont will not be realised until much further advance has been made in the design and construction of the vessels they contemplate.

[W. H. W.]

GENERAL MONTHLY MEETING,

Monday, July 3, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Baroness Gray

was elected a Member of the Royal Institution.

The Special Thanks of the Members were returned to Sir Andrew Noble, Bart., K.C.B. F.R.S., for his donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures; also to Mr. Rollo Appleyard for his Gift of a Portrait of the late Professor J. D. Everett, F.R.S. *M.R.I.*

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Chemist and Druggist for June, 1905. 8vo.
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Electrical Review for June, 1905. 4to.
Electrical Times for June, 1905. 4to.
Electricity for June, 1905. 8vo.
Engineer for June, 1905. fol.
Engineering for June, 1905. fol.
Journal of the British Dental Association for June, 1905. 8vo.
Journal of State Medicine for June, 1905. 8vo.
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Page's Weekly for June, 1905. 8vo.
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Public Health Engineer for June, 1905. 8vo.
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Zoophilist for June, 1905. 4to.

Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. II. Disp. 1. 8vo. 1905.
Franklin Institute—Journal, Vol. CLIX. No. 6, June, 1905. 8vo.
Fraser, Colonel A. T., M.R.I. (the Author)—The Drift of Buddhism. 8vo. 1905.
Geneva, Société de Physique—Mémoires, Vol. XXXV. Fasc. 1. 4to. 1905.
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Gordon, J. W., Esq., M.R.I. (the Author)—The Appellate Jurisdiction of the House of Lords and of the Full Parliament. 8vo. 1905.
Göttingen, Academy of Sciences—Nachrichten 1905; Mathematisch Klasse, Heft 1–2. 8vo.
Gray, R. K., Esq., M.R.I.—United States Census Bureau: Special Reports on Street and Electric Railways, 1902. 4to. 1905.
Imperial Institute—Bulletin, Vol. III. No. 1. 8vo. 1905.
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Meteorological Office—Observations at Stations of the Second Order for 1900. 4to. 1905
Meteorological Society, Royal—Journal, Vol. XXXI. No. 134. Record, Vol. XXIV. No. 95. 8vo. 1905.
Microscopical Society, Royal—Journal, 1905, Part 3. 8vo. 1905.
Montana University—Bulletin, Nos. 25–26. 8vo. 1905.
Munich Academy—Sitzungsberichte, 1905, Heft 1. 8vo.
New York Academy of Sciences—Memoirs, Vol. II. Part 4. 4to. 1905.
Odontological Society—Transactions, Vol. XXXVII. No. 7. 8vo. 1905.
Paris, Observatoire de Nice—Annales, Tome VIII.–IX. 4to. 1904–5.
Peru, Cuerpo de Ingenieros de Minas—Boletin, Nos. 20–23. 8vo. 1905.
Pharmaceutical Society of Great Britain—Journal for June, 1905. 8vo.
Photographic Society, Royal—Journal, Vol. XLV. No. 6. 8vo. 1905.

Riccò, Prof. A.—Memorie della Società degli Spectroscopisti Italiani, Vol. XXXIV. Disp. 5. 4to. 1905.
Rigg, A., Esq., M.R.I. (the Author)—Modern Guns and Smokeless Powder. 8vo. 1902.
Rome, Ministry of Public Works—Giornale for Nov.–Dec. 1904; Jan.–Feb. 1905. 8vo.
Royal Engineers, Corps of—Professional Papers, Vol. XXX. 8vo. 1905.
Journal for July, 1905. 8vo.
Royal Irish Academy—Proceedings, Section C. No. 10. 8vo. 1905.
Royal Society of London—Philosophical Transactions, A, Nos. 385–388; B, Nos. 240–241. 4to. 1905.
Proceedings, Series A and B, No. 509. 8vo. 1905.
St. Petersburg, Académié Impériale des Sciences—Comptes Rendus de la Commission Sismique, Tome II. Liv. 1. 4to. 1905.
Sanitary Institute, Royal—Journal, Vol. XXVI. Part 6. 8vo. 1905.
Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXI. No. 6. 8vo. 1905.
Society of Arts—Journal for June, 1905. 8vo.
Smithsonian Institution—Miscellaneous Collections, Vol. XLVI. Nos. 1444 and 1571. 8vo. 1905.
United Service Institution, Royal—Journal for June, 1905. 8vo.
United States Department of Agriculture—Monthly Weather Review for Feb. 1905. 4to.
United States Patent Office—Official Gazette, Vol. CXVI. Nos. 5–8. 8vo. 1905.
Annual Report for 1904. 8vo. 1905.
Vienna, Imperial Geological Institute—Verhandlungen, 1905, Nos. 3–5.
Washington Academy of Sciences—Proceedings, Vol. VII. Pages 1–188. 8vo. 1905.
Western Australia, Agent-General—Monthly Statistical Abstract for April, 1905. 4to.
Western Society of Engineers—Journal, Vol. X. No. 2. 8vo. 1905.
Zoological Society of London—Proceedings, 1905, Vol. I. Part 1. 8vo. 1905.

GENERAL MONTHLY MEETING,

Monday, November 6, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Lady Alford,
Waldemar Friedlaender, Esq.
Alexander Muirhead, Esq., D.Sc. F.R.S.
David A. Thomson, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Robert Hannah, *M.R.I.*, for his gift of the picture, painted by him, of "Master Isaac Newton in his Garden at Woolsthorpe, in the Autumn of 1665."

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Report on Government Museum and Connemara Library for 1904–5. 4to. 1905.
Kodaikanal Observatory Bulletin, Nos. II.–III. 4to. 1905.
Kurukh Folk-Lore, collected by F. Hahn. 8vo. 1905.
Geological Survey of India: Records, Vol. XXXII. Part 2. 8vo. 1905.
Memoirs, Vol. II. No. 2, Plates 8–10. 8vo. 1905.
British Museum (Natural History)—Catalogue of Lepidoptera Phalænæ, Vol. V. and Plates. 8vo. 1905.
Guide to Gallery of Birds. 8vo. 1905.
The Astronomer Royal—Report on Cape of Good Hope Observatory, 1904. 4to. 1905.
Independent Day Numbers, Cape of Good Hope, 1905–7. 8vo. 1904.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIV. 1° Semestre, Fasc. 12; 2° Semestre, Fasc. 1–7. Classe di Scienze Morali, Serie Quinta, Vol. XIV. Nos. 1–4. 8vo. 1905.
American Academy of Arts and Sciences—Proceedings, Vol. XLI. Nos. 3–12. 8vo. 1905.
American Geographical Society—Bulletin, Vol. XXXVII. Nos. 7–10. 8vo. 1905.
American Philosophical Society—Proceedings, Vol. XLIX. Jan.–July, 1905. 8vo.
Amsterdam, Royal Academy of Sciences—Verhandelingen, 1e Sectie, Deel IX. No. 1; 2e Sectie, Deel XI. and XII. Nos. 1–2. 8vo. 1905.
Zittingsverslagen, Vol. XIII. 8vo. 1905.
Proceedings, Vol. VIII. 8vo. 1905.
Jaarboek, 1904. 8vo. 1905.

Aristotelian Society—Proceedings, N.S. Vol. 5. 8vo. 1905.
Asiatic Society of Bengal—Proceedings, 1904, Nos. 6–10. 8vo. 1904–5.
Journal, Vol. LXXIII. Part I. Nos. 3–4; Part II. Nos. 3–5; Part III. Nos. 3–4. 8vo. 1904.
Astronomical Society, Royal—Monthly Notices, Vol. LXV. No. 8. 8vo. 1905.
Memoirs: Appendix II. to Vol. LV.; Vol. LVII. Part 1. 4to. 1904–5.
Australian Association for the Advancement of Science—Report of the Tenth Meeting held at Dunedin 1904. 8vo. 1905.
Automobile Club—Journal for July–Oct. 1905.
Bankers, Institute of—Journal, Vol. XXVI. Parts 6–7. 8vo. 1905.
Basel Naturforschenden Gesellshaft—Verhandlungen, Band XVIII. Heft 1. 8vo. 1905.
Batavia, Royal Magnetical Ovservatory—Regenwaarnemingen in Nederlandsch Indie, 1903. 8vo. 1904.
Belgium, Royal Academy of Sciences—Bulletin, 1905, Nos. 5-8. 8vo.
Berlin, Royal Academy of Sciences—Sitzungsberichte, 1905, Nos. 23–38. 8vo.
Bombay Branch of the Royal Asiatic Society—Journal, Extra Number, Centenary Memorial Volume. 8vo. 1905.
Boston Public Library—Annual Report, 1904–5. 8vo.
Monthly Bulletin for July–Oct. 1905. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XII. Nos. 17–20. 4to. 1905.
Kalendar, 1905-6. 8vo. 1905.
British Astronomical Association—Journal, Vol. XV. Nos. 9–10. 8vo. 1905.
Buckton, G. Bowdler, Esq., F.R.S. M.R.I. (*the Author*)—Observations on undescribed Species of Hemiptera-Homoptera of the Family Membracidæ. 4to. 1905.
Buenos Ayres, Office of Statistics—Demografia, 1900 and 1902. 8vo. 1905.
Bulletin of Municipal Statistics, April–July, 1905. fol.
Cabreira, A., Esq. (*the Author*)—Notes sur les Rapports des Solides. 8vo. 1905.
Cambridge Observatory—Annual Report, 1904–5. 4to. 1905.
Cambridge Philosophical Society—Transactions, Vol. XX. Nos. 1-6. 4to. 1905.
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Canada, Department of the Interior—Topographical Map: Hamilton, Kluanee and New Brunswick. fol. 1905.
Relief Map of the Dominion of Canada; Maps of Manitoba, Saskatchewan, and Alberta. fol. 1905.
Canada, Royal Society—Proceedings, Vol. X. Parts 1–2. 8vo. 1905.
Carroll, Lieut. Charles, M.D. (*the Author*)—Various Papers on Yellow Fever. 8vo. 1899–1905.
Catholic Record Society—Annual Report, No. I. 8vo. 1905.
Chemical Industry, Society of—Journal, Vol. XXIV. Nos. 13–20. 8vo. 1905.
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Chicago, Field Columbian Museum—Publications: Geological Series, Vol. III. No. 1; Report Series, Vol. II. No. 4. 8vo. 1904–5.
Church of England League—Gazette for July–Oct. 1905. 8vo.
Civil Engineers, Institution of—Proceedings, Vol. CLXI. 8vo. 1905.
List of Members, 1905.
Coit, Dr. James M.—Photograph of Portrait of Count Rumford.
Cornwàll Polytechnic Society, Royal—Seventy-second Annual Report, 1904. 8vo. 1905.
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Colonial Institute, Royal—Proceedings, Vol. XXXVI. 8vo. 1905.
Curgenven, J. Sadler, Esq., M.R.C.S. M.R.I. (*the Author*)—The Child's Diet. 8vo. 1905.
Dax, Société de Borda—Bulletin, 1904, Part IV.; 1905, Part I. 8vo. 1904–5.
East India Association—Journal, Vol. XXXVIII. Nos. 39–40. 8vo. 1905.

Editors—Aeronautical Journal for July–Oct. 1905. 8vo.
American Journal of Science for July–Oct. 1905. 8vo.
Analyst for July–Oct. 1905. 8vo.
Astrophysical Journal for July–Oct. 1905. 8vo.
Athenæum for July–Oct. 1905. 4to.
Author for July–Nov. 1905. 8vo.
Brewers' Journal for July–Oct. 1905. 8vo.
Chemical News for July–Oct. 1905. 4to.
Chemist and Druggist for July–Oct. 1905. 8vo.
Dioptric Review for June–Sept. 1905. 8vo.
Electrical Contractor for July–Oct. 1905. 8vo.
Electrical Engineer for July–Oct. 1905. 4to.
Electrical Review for July–Oct. 1905. 4to.
Electrical Times for July–Oct. 1905. 4to.
Electricity for July–Oct. 1905. 8vo.
Engineer for July–Oct. 1905. fol.
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Homœopathic Review for July–Nov. 1905. 8vo.
Horological Journal for July–Nov. 1905. 8vo.
Journal of the British Dental Association for July–Oct. 1905. 8vo.
Journal of State Medicine for July–Oct. 1905. 8vo.
Law Journal for July–Oct. 1905. 8vo.
London University Gazette for July–Oct. 1905. 4to.
Machinery Market for July–Oct. 1905. 8vo.
Model Engineer for July–Oct. 1905. 8vo.
Mois Scientifique for July–Aug. 1905. 8vo.
Motor Car Journal for July–Oct. 1905. 4to.
Musical Times for July–Oct. 1905. 8vo.
Nature for July–Oct. 1905. 4to.
New Church Magazine for July–Nov. 1905. 8vo.
Nuovo Cimento for May–July, 1905. 8vo.
Page's Weekly for July–Oct. 1905. 8vo.
Photographic News for July–Oct. 1905. 8vo.
Physical Review for July–Oct. 1905. 8vo.
Public Health Engineer for July–Oct. 1905. 8vo.
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Terrestrial Magnetism for June–Sept. 1905. 8vo.
Travel for Oct. 1905. 8vo.
Zoophilist for July–Oct. 1905. 4to.

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List of Members, 1905. 8vo.

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Fraser, Colonel A. T., M.R.I. (*the Author*)—Darkness and Light in the Land of Egypt. 8vo. 1905.

Frazer, J. G., Esq., D.C.L. LL.D. (*the Author*)—Early History of the Kingship. 8vo. 1905.

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Journal, Vol. LXI. Part 3. 8vo. 1905.

Geological Survey of the United Kingdom—Summary of Progress, 1904. 8vo. 1905.

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Gowland, Professor W., M.R.I.—Sixth Report of the Alloys Research Committee of the Institute of Mechanical Engineers on the Heat Treatment of Steel. By Sir William Roberts-Austen and Professor W. Gowland. 8vo. 1905.
Guebhard, Dr. A. (the Author)—Sur la Fonction Photographique. 8vo. 1905.
Notes Photographiques. 4to. 1905.
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Imperial Institute—Bulletin, Vol. III. No. 2. 8vo. 1905.
Iron and Steel Institute—Journal, 1905, No. 1. 8vo.
List of Members, 1905. 8vo.
Johns Hopkins University—Studies, Vol. XXIII. Nos. 3-10. 8vo. 1905.
American Journal of Philology, Vol. XXVI. No. 2. 8vo. 1905.
Circulars, 1905, Nos. 2–7. 8vo.
Financial History of Baltimore. By J. H. Hollander. 8vo. 1899.
Junior Institution of Engineers—Transactions, Vol. XIV. 8vo. 1905.
Kyóto, Imperial University—Memoirs of the College of Science and Engineering, Vol. I. No. 2. 8vo. 1904.
Leicester Municipal Libraries—Thirty-fourth Annual Report, 1904–5. 8vo. 1905.
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Preisschriften, No. XXIV. 8vo. 1905.
Abhandlungen, Band XXIII. Nos. 1–2; Band XXIX. Nos. 3–4. 4to. 1904–5.
Life-Boat Institution, Royal National—Journal for August–Nov. 1905. 8vo.
Linnean Society—Journal: Zoology, Vol. XXIX. No. 192; Botany, Vol. XXXVII. Nos. 258–9. 8vo. 1905.
Transactions, Second Series: Botany, Vol. VI. Parts 10–11; Vol. VII. Parts 1–2; Zoology, Vol. IX. Parts 6–9; Vol. X. Parts 1–3. 4to. 1904–5.
Lister Institute of Preventive Medicine—Collected Papers, No. 1. 8vo. 1904.
London County Council—Gazette, July–Oct. 1905. 4to.
Madrid, Reale Academia de Ciencias—Revista, Tomo II. Num. 4–5; Tomo III. Num. 1. 8vo. 1905.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. XLIX. Part 3. 8vo. 1905.
Massachusetts Institute of Technology—Technology Quarterly, Vol. XVIII. No. 2. 8vo. 1905.
Mechanical Engineers, Institution of—Proceedings, 1904, No. 4; 1905, No. 1. 8vo. 1905.
Mersey Conservancy—Report on the River Mersey, 1904. 8vo. 1905.
Meteorological Society, Royal—Journal, Vol. XXXI. No. 135. 8vo. 1905.
Record, Vol. XXIV. No. 96. 8vo. 1905.
Metropolitan Asylums Board—Annual Report for 1904. 8vo. 1905.
Report on Scarlet Fever and Diphtheria, July 1901–July 1902. By A. G. R. Cameron. 4to. 1905.
Mexico, Secretaria de Comunicaciones y obras Publicas—Anales, No. 12, Oct. 1904. 8vo.
Microscopical Society, Royal—Journal, 1905, Parts 4–5. 8vo. 1905.
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Sitzungsberichte, 1905, Heft II. 8vo. 1905.
Musée Teyler—Archives, Ser. II. Vol. IX. Parts 1–2. 8vo. 1904.
Natal, Agent-General—Report of Coal Testing Committee. 1904–5. 4to. 1905.
National Physical Laboratory—Collected Researches, Vol. I. 4to. 1905.
Navy League—Navy League Journal for July–Oct. 1905. 8vo.
New Jersey Geological Survey—Annual Report, 1904. 8vo. 1905.
New South Wales, Agent-General—Report of the Comptroller of Prisons, 1904. 4to. 1905.

Norfolk and Norwich Naturalists' Society—Transactions, Vol. VIII. Part 1. 8vo. 1905.
North of England Institute of Mining Engineers—Transactions, Vol. LII. Part 8; Vol. LIV. Part 8; Vol. LV. Part 4. 8vo. 1905.
Odontological Society—Transactions, Vol. XXXVII. No. 8. 8vo. 1905.
Paris, Société Française de Physique—Bulletin des Séances, 1905, Fasc. 1. 8vo.
Pennsylvania University—Bulletin, 5 S. No. 4, Part II.; No. 5, Part II. 8vo. 1905.
Contributions from Zoological Laboratory, Vol. XI. Parts 1–2. 8vo. 1904.
Peru, Cuerpo de Ingenieros de Minas—Boletin, No. 24. 8vo. 1904–5.
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Philadelphia, Academy of Sciences—Proceedings, Vol. LVII. Part 1. 8vo. 1905.
Photographic Society, Royal—Journal, Vol. LXV. Nos. 7–9. 8vo. 1905.
Catalogue of Fiftieth Annual Exhibition. 8vo. 1905.
Physical Society of London—Proceedings, Vol. XIX. Part 6. 8vo. 1905.
Premier (Transvaal) Diamond Mining Co., Ltd.—Two Photographs of the "Cullinan" Diamond.
Riccó, Professor A.—Memorie della Società degli Spettroscopisti Italiani, Vol. XXXIV. Disp. 6–8. 4to. 1905.
Rio de Janeiro Observatory—Bulletin for Oct.–Dec. 1904, Jan. 1905. 8vo. 1905.
Rome, Ministry of Public Works—Giornale for March–July, 1905. 8vo.
Röntgen Society—Journal for July, 1905. 8vo.
Royal College of Surgeons—The Calendar, 1905–6. 8vo. 1905.
Royal Dublin Society—Proceedings: Scientific, Vol. X. Part 3, Vol. XI. Parts 1–5; Economic, Vol. I. Part 6. 8vo. 1905.
Royal Engineers' Institute—Journal, August–Nov. 1905. 8vo.
Royal Irish Academy—Proceedings, Vol. XXV. Section C, No. 11. 8vo. 1905.
Royal Society of Edinburgh—Proceedings, Vol. XXV. Nos. 9-10. 8vo. 1905.
Royal Society of London—Philosophical Transactions, A, Nos. 389–395; B, Nos. 242–3. 4to. 1905.
Proceedings, Series A and B, Nos. 510–512. 8vo. 1905.
Reports of the Mediterranean Fever Commission, Part 3. 8vo. 1905.
Obituary Notices, Part IV. 8vo. 1905.
Reports of the Sleeping Sickness Commission, No. 5. 8vo. 1905.
St. Bartholomew's Hospital—Statistical Tables, 1904. 8vo. 1905.
St. Petersburg, Imperial Academy of Sciences—Mémoires, VIII. Série, Classe Physico-Mathématique, Vol. XVI. Nos. 4–10. 4to. 1905.
Sanderson, Sir Thomas H., K.C.B. K.C.M.G. M.R.I.—A Dictionary of the World. By R. Brookes and J. Collier. fol. 1772.
A Compleat Body of Husbandry. By T. Hale. fol. 1756.
Dictionary of Arts and Sciences. By E. Middleton and others. fol. 1778.
Essay on Rural Architecture. By R. Elsam. 4to. 1805.
The School of Arts. By J. Imison. 8vo. 1796.
Sanitary Institute, Royal—Journal, Vol. XXVI. Parts 7–9 and Supplement. 8vo. 1905.
Sawyer, Sir James, M.D. (the Author)—Practical Medicine. 8vo. 1904.
Insomnia: Its Causes and Cure. 8vo. 1904.
Selborne Society—Nature Notes for July–Oct. 1905. 8vo.
Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXI. Nos. 7–11. 8vo. 1905.
Transactions of the Institution of Naval Architects, Vol. XLVII. Parts 1–2. 4to. 1905.
Smithsonian Institution—Miscellaneous Collections, Quarterly Issue, Vol. II. No. 4; Vol. III. Part 1; XLVIII. No. 1574; Vol. XLIX. No. 1584. 8vo. 1905.
Annual Report, 1903. 8vo. 1905.

Society of Arts—Journal for July-Oct. 1905. 8vo.
South Australian School of Mines—Report for 1904. 8vo. 1905.
Statistical Society, Royal—Journal, Vol. LXVIII. Part 2. 8vo. 1905.
Stockholm, Royal Swedish Academy of Sciences—Arkiv: Botanik, Band IV. Heft 1-3: Kemi, Band II. Heft 1; Zoologie, Band II. Heft 3. 8vo. 1905.
Nobel Institut, Meddelanden, Band I. No. 1. 8vo. 1905.
Toronto University—Studies: Biological Series, No. 4; Papers from Chemical Laboratories, Nos. 44–47, 50–51. 8vo. 1904.
Transvaal Agricultural Department—Journal for July, 1905. 8vo.
United Service Institution, Royal—Journal for July–Oct. 1905. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XVI. Nos. 8–12; Vol. XVII. No. 1. 8vo. 1905.
Monthly Weather Review for March–June, 1905. 4to.
United States Department of Commerce and Labour—Bulletin of the Bureau of Standards, Vol. I. Nos. 1–2. 8vo. 1905.
United States Geological Survey—Bulletin, Nos. 234–40, 242, 244–6, 248–50, 252–5, 257, 258–62. 8vo. 1904-5.
Mineral Resources of U.S.A., 1903. 8vo. 1905.
Twenty-fifth Annual Report. 4to. 1905.
Monographs, Vol. XLVII. 4to. 1904.
Professional Papers, Nos. 29–33, 35, 39. 4to. 1904–5.
Water Supply Papers, Nos. 99, 100, 103, 105, 118–121, 124, 126, 128–132. 8vo. 1904–5.
United States Patent Office—Official Gazette, Vol. CXVII.; Vol. CXVIII. Nos. 1–8. 8vo. 1905.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1905, Heft 6–8. 4to.
Victoria Institute—Transactions, Vol. XXXVII. 8vo. 1905.
Vienna Imperial Geological Institute—Jahrbuch, Band LV. Heft 1-2. 8vo. 1905.
Verhandlungen, 1905, Nos. 6–9.
Washington Academy of Sciences—Proceedings, Vol. VII. pp. 189–300. 8vo. 1905.
Washington Philosophical Society—Bulletin, Vol. XIV. pp. 277–316. 8vo. 1905.
Welcker, A., Esq. (*the Author*)—"For People who Laugh." 8vo. 1905.
Western Australia, Agent-General—Monthly Statistical Abstract, May–August, 1905. 4to.
Mining Statistics for Feb. 1905. 4to.
Report of Royal Commission on the Ventilation and Sanitation of Mines. 4to. 1905.
Supplement to Government Gazette for August, 1905. 4to.
Western Society of Engineers—Journal, Vol. X. Nos. 3–4. 8vo. 1905.
Wisconsin Academy—Transactions, Vols. IX.-XII. 8vo. 1893-1900.
Woodhouse, A. J., Esq., M.R.I.—Transactions of the New Zealand Institute, Vol. XXXVII. 8vo. 1905.
Yerkes Observatory—Reports of the Director, 1899–1904. 8vo. 1905.
Yorkshire Archæological Society—Journal, Vol. XVIII. Part 3. 8vo. 1905.
Zoological Society of London—Proceedings, 1905, Vol. I. Part 2. 8vo. 1905.
Zurich, Naturforschenden Gesellschaft—Vierteljahrsschrift, 1904, Heft 3–4; 1905, Heft 1–2. 8vo. 1905.
Neujahrsblatt, 1905, 107 Stück. 4to.

GENERAL MONTHLY MEETING,

Monday, December 4, 1905.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Lieutenant E. P. C. Amphlett,
Frank Bailey, Esq., M.Inst.C.E.
Henry Behrens, Esq.
Joseph Kennedy, Esq.
J. Malcolm Kerr, Esq., A.M.Inst.C.E.
Dr. E. M. Modi,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Dr. Ludwig Mond, F.R.S., for his donation of £500 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Managers reported, That at their Meeting held that day they had elected Professor William Stirling, M.D. LL.D. D.Sc., Fullerian Professor of Physiology, for three years (the appointment dating from January 13, 1906).

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Archæological Survey of India, Vol. XXXIII.; (W. India, Vol. VIII. Part 2, Ahmadabad Architecture). 4to. 1905.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIV. 2° Semestre, Fasc. 8–9. 8vo. 1905.
Classe di Scienze Morali. Vol. XIV. Fasc. 5–6.
American Academy of Arts and Sciences—Proceedings, Vol. XLI. No. 13. 8vo. 1905.
The Rumford Fund of the American Academy. 8vo. 1905.
American Geographical Society—Bulletin, Vol. XXXVII. No. 11. 8vo. 1905.
Asiatic Society of Bengal—Journal, Vols. LXXIII. Part I. Extra No.; Part II. Supplement; Part III. Extra No.
Proceedings, 1904, No. XI.
Journal and Proceedings, Vol. I. No. 1–4. 8vo. 1904–5.
Astronomical Society, Royal—Monthly Notices, Vol. LXV. No. 9. 1905.
Automobile Club—Journal for November, 1905.
Bagshaw, W., Esq. (the Author)—Elementary Photo-Micrography. 8vo. 1902.
Bankers, Institute of—Journal, Vol. XXVI. Part 8. 8vo. 1905.
Boston Public Library—Monthly Bulletin for November, 1905. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIII. Nos. 1–2. 4to. 1905.
British Astronomical Association—Journal, Vol. XVI. No. 1. 8vo. 1905.
Buenos Ayres—Monthly Bulletin for August, 1905. 4to.
Canadian Institute—Transactions, Vol. VIII. Part I. 8vo. 1905.
Chemical Industry, Society of—Journal, Vol. XXIV. Nos. 21–22. 8vo. 1905.

Chemical Society—Proceedings, Vol. XXI. Nos. 299–300. 8vo. 1905.
Journal for November, 1905. 8vo.
Chicago, Field Columbian Museum—Publications, Anthropological Series, Vol. VI. Nos. 2–3; Vol. VIII.; Vol. IX. Nos. 1–2. 8vo. 1905.
Clinical Society—Transactions, Vol. XXXVIII. 8vo. 1905.
Cracovie, Academy of Sciences—Bulletin, 1905: Mathematische Klasse, Nos. 5–7; Philologische Klasse, Nos. 3–7. 8vo.
Devonshire Association—Transactions, Vol. XXXVIII. 8vo. 1905.
Devonshire Wills, Part VII. 8vo. 1905.
Edinburgh, Royal Society of—Proceedings, Vol. XXV. No. XI. 8vo. 1905.
Editors—American Journal of Science for November, 1905. 8vo.
Analyst for November, 1905. 8vo.
Astrophysical Society for November, 1905. 8vo.
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WEEKLY EVENING MEETING,

Friday, January 20, 1905.

Ludwig Mond, Esq., Ph.D. D.Sc. F.R.S., Vice-President, in the Chair.

Professor Sir James Dewar, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*

New Low Temperature Phenomena.

The porosity of matter, and the possibility of the occlusion of gases in it, has been the subject of scientific thought for ages. As early as 1674, Boyle,* under the title "Suspicions about the hidden Qualities of Air," writes: "It may not seem altogether improbable, that some bodies we are conversant with, may have a peculiar disposition and fitness to be wrought on by, or to be associated with, some of those exotic effluvia that are emitted by unknown bodies lodged underground, or that proceed from this or that planet. . . . We may be allowed to consider whether among the bodies we are acquainted with here below, there may not be found some that may be receptacles, if not also attractives, of the sidereal and other exotic effluviums that rove up and down in our air." While other matters took up his immediate attention, this one was not lost sight of, for ten years later, in 1684, he returns to the subject in a special discourse. "Experiments and considerations about the porosity of Bodies,"† in which he says: "When I consider how much most of the qualities of bodies, and consequently their operations, depend upon the structure of their minute and singly invisible particles, and that to this latent contexture, the bigness, the figure, and the collocation of the intervals and pores do necessarily concur with the size, shape and disposition, or contrivance, of the substantial parts, I cannot but think the doctrine of the small pores of bodies of no small importance to Natural Philosophy." Felix Fontana, the famous physicist to Duke Ferdinand II. of Tuscany, seems to have been the first to have discovered the absorptive power of hot charcoal for gases, a property which he communicated to Priestley, about 1770, and which Priestley confirmed. Lowitz, in 1791, noticed that charcoal decolorised organic solutions. Later experiments were made by Morozzo, and another series of observations were made by two Dutch physicists, Rouppe and Norden. Shortly after this, early in last century, the subject of the action of gases on charcoal was elaborately examined by Theodore

* Boyle, Works, vol. iii., p. 470, col. 1.
† Boyle, Works, vol. iv., p. 206, col. 1.

de Saussure. Subsequently Graham and Stenhouse added valuable contributions to the inquiry. The thermal evolutions of some gaseous absorptions in charcoal were determined by Faure and Silbermann ; and later, Hunter showed the advantage of using cocoa-nut charcoal, and made a long series of investigations on the absorption of organic vapours and gases by this variety of charcoal.

In de Saussure's experiment a piece of red-hot charcoal was plunged under mercury, and introduced into the gas to be absorbed after it was cool, without allowing it to come in contact with air. He made use of box-wood charcoal, about which he remarks that it absorbed so little mercury during the cooling that it would readily swim on water. His experiments were conducted at ordinary temperature and pressure, and gave the results in the annexed table, the unit volume being that of the absorbing charcoal. For comparison, similar experiments made by Hunter with cocoa-nut charcoal are given.

	Boxwood (Saussure)	Cocoa-nut (Hunter)
Ammonia	90	172
Hydrochloric acid	85	—
Sulphurous acid	65	—
Sulphuretted hydrogen	55	—
Nitrous oxide	40	86
Carbonic acid	35	68
Olefiant gas	35	75
Carbonic oxide..	9·42	21
Oxygen	9·25	18
Nitrogen	7·5	15
Hydrogen	1·75	4

He found that even if the charcoal were moistened with water, it was still capable of absorbing one-third to one-half the amount of gas absorbed when quite dry.

During these experiments he called attention to the evolution of heat during absorption, and remarked that it appeared to increase with the absorbability of the gas.

In further experiments he considered the effect of pressure on the amount of absorption, and found that absorption by volume is far greater in a rare than in a dense atmosphere, but that if reckoned by weight it is more considerable in the latter than in the former state.*

He continued similar experiments with meerschaum, asbestos, and other substances, and also examined the effect of mixed gases.

Hunter examined the absorption of vapours by cocoa-nut charcoal at or above their boiling points with interesting and suggestive results

* Saussure's results for carbonic acid may be represented by the formulæ—

$$v = 15{\cdot}3 + \frac{551{\cdot}7}{p}$$

$$v = 19{\cdot}1 + {\cdot}53\,p;$$

where v is the volume absorbed, measured as above, and p is the pressure in inches of mercury.

as regards selective action. The following table is a selection made from some of his observations :—

Cocoa-Nut Charcoal Absorption of Vapours at Boiling Point of Liquid (Hunter).

Carbon tetrachloride	4
Chloroform	30
Ethyl iodide	36
Alcohol	141
Benzol	59
Carbon bisulphide	117
Ether	87
Ethylamine	127
Water	55

In a note read before the Royal Society of Edinburgh in March 1874, connected with a research undertaken in association with the late Professor Tait, the absorptive power of charcoal was employed for the first time in the production of high vacua. A piece of cocoa-nut charcoal was placed in a glass tube, into which were sealed two platinum terminals for electric sparking. The tube was exhausted by the mercury pump, while the charcoal was at the same time heated to a red heat. On sealing off the tube, and allowing the charcoal to cool, the vacuum was so perfect that no spark would pass between the terminals, from a coil giving quarter-of-an-inch sparks in air. Similar experiments were repeated when investigating the theory of the motion of the Crookes Radiometer, and are detailed in "Nature" in 1875.

One advantage of charcoal vacua in the study of electric discharges is that on heating the charcoal slowly, and connecting the platinum terminals to the induction coil, the *striæ* may be reproduced and maintained at any degree of rarefaction desired, and as often as we please.

At the Conference on May 24, 1876, in connection with the Loan Collection of Scientific Apparatus at South Kensington, I showed a further simplification and advance in the production of high vacua. A little fluid bromine was placed in a tube, and the tube was put in a water bath, so that the bromine boiled off. Meanwhile the charcoal in another part of the tube was heated in the usual way, and when all the bromine had been vaporised the tube was sealed off. On the charcoal cooling it absorbed the bromine so thoroughly that no trace of colour was visible. In this way a vacuum is produced without the use of any exhausting pump.

Although all charcoals exhibit absorptive powers of a high order, nevertheless differences are noticeable. Among charcoals got from wood, the denser woods seem to produce the more absorptive charcoals. Thus box-wood charcoal is more absorptive than fir-charcoal. Saussure found the following relative absorptive powers from different wood charcoals :—

	Sp. gr.	Absorption.
Cork	0·1	Imperceptible.
Fir	0·4	4½ its vol. of air.
Boxwood	0·6	7½ ,,
Russiberg coal (vegetable origin)	1·3	10½ ,,

N 2

But at last the porosity must become so fine that absorption again disappears. Thus Cumberland black lead (which is 96 per cent. carbon), of sp. gr. 2·17, showed no absorption.

From experiments, detailed on a former occasion,* undertaken to determine the effect of various substances in a state of fine division placed within the walls of vacuum vessels in protecting the contents of the vessels from external heat, by reason of bad thermal conductivity and the interference of the mean free path of the gas molecules, I found that charcoal and lampblack were nearly equally good at the temperature of liquid air, and that each was four times as good as graphite.

From some recent investigations I find that charcoals so different in their origin as cocoa-nut charcoal and charcoal from cane-sugar differ but little in their absorptive powers for a gas like hydrogen.†

Mitscherlich specially studied the nature of porosity in connection with the occlusion of gases. Taking a piece of charcoal weighing 0·9565 grm., and thoroughly saturating it with water, he found that it weighed 2·2585 grm. in air, and 0·110 grm. under water. Hence of the gross volume of the charcoal, $\frac{3}{8}$ was occupied by charcoal substance, and $\frac{5}{8}$ was free space into which gases might be absorbed. Saussure found that charcoal at 12° C. and ordinary pressure absorbed 35 times its volume of carbonic acid, but as this occupied $\frac{5}{8}$ of the gross volume of the charcoal, it was actually forced into a space equal to only $\frac{1}{56}$ of its original volume. He concluded, therefore, that about a third of the gas was liquefied in the pores of the charcoal.‡

He found that the cells of charred wood are on an average $\frac{1}{2400}$ of an inch in diameter. Now, if a cubic inch of charcoal were cut up into a number of small equal cubes each of whose edges was $\frac{1}{2400}$ inch, the total area of their surfaces would be 100 square feet, or taking into account the space occupied by the charcoal itself, it would leave about 73 square feet. The thickness of the liquefied carbonic acid over this area would, then, be about ·000002 inch.

This harmonises to a certain extent with Saussure's remark that denser charcoals, that is, charcoals with pores of smaller diameters,

* Liquid Air as an Analytic Agent, Roy. Inst., 1 Ap., 1898.

† Approx. Series of 30/10/04, range 250 c.c. to 2750 c.c., gave

$$\log. p = -\cdot 08 + \cdot 267 \text{ c.} \quad \text{Cocoa-nut.}$$

Series 4/11/04, range 250 c.c. to 2250 c.c., gave

$$\log. p = \cdot 39 + \cdot 195 \text{ c.} \quad \text{Cane-sugar.}$$

If $p = 100$ mm., these give respectively

$$\text{c.} = \frac{2\cdot 08}{\cdot 267} = 7\cdot 8. \quad \text{Cocoa-nut;}$$

$$\text{and c.} = \frac{1\cdot 61}{\cdot 195} = 8\cdot 3. \quad \text{Cane-sugar.}$$

‡ From Amagat's observations, the amount liquefied was almost exactly one-fifth.

and representing a greater surface for condensation, are the greater absorbers. Nevertheless, we must not forget to notice that the affinities between charcoal and different gases are not the same, and that these will seriously modify any results derived from mere geometrical considerations.

*Thermal Evolution and Absorption of Gases by Charcoal at Low Temperatures.**

Saussure first observed and roughly noted that the absorption of gases by charcoal at the ordinary temperature gave rise to a considerable evolution of heat. The liquid air and hydrogen calorimeters can be easily arranged to afford an exact measure of the quantities of heat thus given up by the condensation of different gases in charcoal. For this purpose a small glass bulb C, contain-

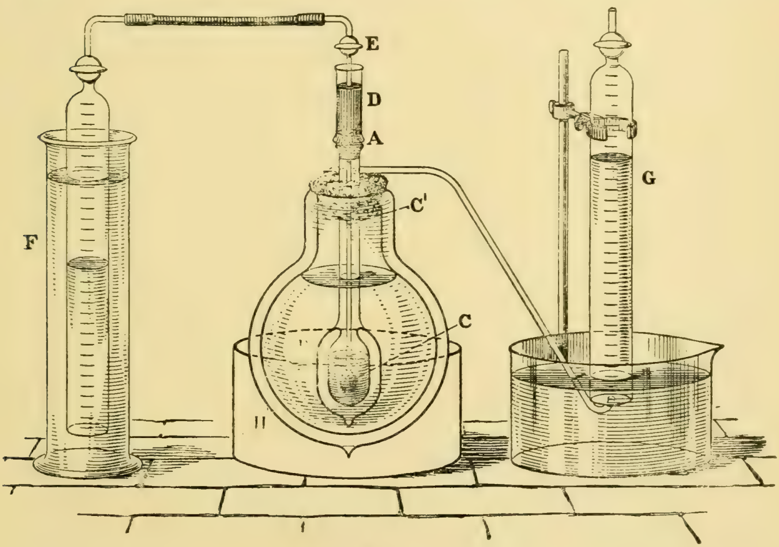

FIG. 1.

ing from half a gramme to a gramme of charcoal, has a long narrow tube C′ attached, so that it can be immersed in the liquid oxygen, or air in the calorimeter A B, while still allowing a part of the tube to project above the cork A. The calorimeter is then inserted in a large vacuum vessel H, containing two or three litres of liquid oxygen or air, as the case may be, and kept in its place by a loose stuffing of cotton wool. In order to dry and cool the entering gas (which, in my experiments, did not exceed

* Proc. Roy. Soc., 1904.

40 c.c.), a little annular space is arranged at D, into which liquid air is poured immediately before the experiment begins.

The charcoal, after being placed in the small bulb C, is heated to a low red heat, and simultaneously exhausted by a good air-pump, and after all the gas has been removed, the stop-cock E is closed. In this condition it is placed in the calorimeter B.

The experiment is conducted by connecting the end of the tube at E by means of an indiarubber tube with the graduated vessel F containing the gas to be examined. When all is ready the stop-cock E is opened, so that the gas may rush into the charcoal, and the heat evolved by its absorption distils off the equivalent quantity of liquid air from the calorimeter B, which is then received and measured in the vessel G.

When we know the constant of the calorimeter—namely, the number of cubic centimetres of gas evaporated by one calorie (about 14·5 c.c. in the present case)—from the readings of the two jars F and G we find at once the heat evolved in the condensation per unit volume of gas absorbed. After making a few necessary corrections, the results for different gases are as given in the following table :—

	I. Volume absorbed at 0° C	II. Volume absorbed at − 185° C.	III. Ratio of II. to I.	IV. Heat evolved in gramme calories by absorption at − 185° C.
Helium	2 c.c.	15 c.c.	7·5	2·0
Hydrogen	4 ,,	135 ,,	34·0	9·3
Electrolytic gas	12 ,,	150 ,,	12·5	17·0
Argon	12 ,,	175 ,,	14·6	25·0
Nitrogen	15 ,,	155 ,,	10·3	25·5
Oxygen	18 ,,	230 ,,	12·8	34·0
Carbonic oxide	21 ,,	190 ,,	9·0	27·5
Carbonic oxide and oxygen ..	30 ,,	195 ,,	6·5	34·5

To render these results comparable, the same specimen of cocoanut charcoal was used for them all, and the numbers in the last column are for absorption by one gramme of charcoal. The volumes of the gases absorbed are, both at ordinary and low temperatures, given under standard conditions—namely, 0° C. and 760 mm. of mercury pressure. If it is desired to know the volumes absorbed at − 185° C., when these volumes are measured at − 185° C. and 760 mm. pressure, we have only to divide the numbers in column II. by three.

In each case, a very remarkable increase of absorption takes place at the low temperature. This is shown in column III. It is further remarkable that the increase of absorption diminishes, roughly, as the boiling points of the various gases increase, while column IV. shows a corresponding increase in the quantities of heat evolved.

The general results are seen best when the values of the thermal evolution and the amount of charcoal involved are reduced to molecular volumes of the condensed gas.

Heat Evolution.

Charcoal absorption per Molecular Volume.

	Calories.	Wght. of Charcoal.
Hydrogen	1600	206 grms.
Nitrogen	3686	180 ,,
Argon	3636	162 ,,
Oxygen	3744	160 ,,
Carbonic Oxide	3416	148 ,,
Carbonic Oxide and Oxygen	3960	144 ,,
Electrolytic Gas	2414	180 ,,

Mean $C_{14}M$; limit C_8H_2 and $C_{12}He$.

A comparison of the molecular latent heats of the liquefied gases, hydrogen, nitrogen and oxygen, with the charcoal heat of condensation in each case, is shown in the following table :—

	Molecular Latent Heats. (Calories)	Molecular Absorption in Charcoal. (Calories)
Hydrogen	238	1600
Nitrogen	1372	3684
Oxygen	1664	3744

But perhaps the most striking result is the great difference in properties exhibited by helium. While resembling other cases in showing increased absorption at the temperature of liquid air, the absolute amount occluded is about one-tenth that of the other gases at the same temperature, and the quantity of heat evolved is in even a smaller ratio. We must, however, note that the position of helium on the scale of temperatures, for these experiments, is quite different from that of the other gases, even hydrogen. For helium is being absorbed at a temperature some fifteen times higher than its boiling point (say 6° abs.), while in the case of hydrogen this is only four-and-a-half times its boiling point (20° abs.). To make a fair comparison, we should take hydrogen at fifteen times its boiling point, which would bring us up to some 27° C., that is, the helium absorption at − 185° C. should preferably be compared with the hydrogen absorption at 0° C. The inference then is, that if we had the absorption of helium at 25° to 30° absolute, we should find it show a still more remarkable condensation than hydrogen does at 90° abs. (− 183° abs.).

The following experimental results confirm this point of view :—

Helium and Hydrogen.

Charcoal Absorption at the Temperatures of Boiling and Solid Hydrogen.

Temperature.	Helium. Vols. of Carbon.	Hydrogen. Vols. of Carbon.
−185° C. (boiling point of liquid air)	$2\frac{1}{2}$	137
−210° C. (liquid air under exhaustion)	5	180
−252° C. (boiling point of liquid hydrogen)	160	258
−258° C. (solid hydrogen)	195	

As the relation between volume and temperature is nearly lineal at the lowest portions of either the hydrogen or helium absorption, we may infer that at the temperature of from 5° to 6° helium would be as freely absorbed by charcoal as hydrogen is at its boiling point, and that in all probability the boiling point of helium is not below 5° abs. This inference is quite legitimate, because good charcoal at the respective boiling points of liquid hydrogen, nitrogen or oxygen, absorbs at atmospheric pressure nearly the same volume of each gas, viz., 260 c.c. per gramme.

It is to be noted that the rate of increase of the helium absorption is three times that of the hydrogen, so that a degree or two makes a large increase in the volume condensed. From these results it seems highly probable that the boiling point of helium is about a fourth that of hydrogen, just as the latter is about one-fourth that of nitrogen.

In column IV. we get the heat evolved by each gas. These values provide us with still further striking results, for the heat developed is, generally, greatly in excess of that required for liquefaction. Thus, for hydrogen, whose latent heat at the boiling point I have recently determined by this calorimeter and found to be 120 gramme calories, the liquefaction of 135 c.c. of this gas would only evolve about $1\frac{1}{2}$ calories, or $\frac{1}{6}$th of that evolved by occlusion in charcoal at the boiling point of air. Similarly, if we take 51 gramme calories as the latent heat of oxygen, the liquefaction of 230 c.c. of this gas would produce some 17 calories, or about half of that evolved during occlusion.

Separation of Highly Concentrated Oxygen from Air by Charcoal at Low Temperatures.

In order to examine the changes taking place in a mixed gas like air during the absorption, a quantity of about 50 grammes of charcoal was after heating and absorption saturated at $-185°$ in a current of pure dry air—got by passing the air current through a U-tube immersed in liquid air.

For a time the air rushed into the charcoal with great rapidity, and in about 10 minutes between 5 and 6 litres were taken in. A manometer attached to the vessel containing the charcoal showed, on shutting off the air current, that during the early part of the saturation the absorption was so effective as to give practically no measureable mercury pressure. As soon as the absorption was ended, and a current began to pass slowly over the charcoal, the composition of the air leaving the charcoal showed 98 per cent. nitrogen. After the current of air had passed for half an hour, the total gas occluded in the charcoal was expelled by taking the vessel in which it had been treated out of the liquid air, and allowing the temperature to rise to 15° C.

The gas, which was rapidly expelled, measured 5·7 litres, and contained 56 per cent. of oxygen. If the saturated charcoal before heating was subjected for an hour to the action of an air-pump, capable of giving a steady exhaustion of 5 mm., no difference was effected in the oxygen percentage of the evolved gas. The same experiment was repeated with this variation, that, instead of the air current having the pressure of the atmosphere, it was kept below one-tenth of an atmosphere. In this experiment 4·8 litres were expelled on heating up, and the percentage of oxygen was 58. Then, a further repetition was made with an air current supplied at a pressure not exceeding 5 mm. of mercury. After 3 hours' treatment, the charcoal, on heating to 15° C., gave 4½ litres of 57 per cent. oxygen. From these experiments it follows that the tension of the occluded gases, at the temperature of liquid air, must be very small, and thus the use of low temperatures, combined with charcoal, introduces a new and greatly improved means of getting high vacua, which in the future may be found susceptible of important practical applications. These experiments are quite conclusive as to the practical constancy of the mean composition of the air gases occluded in the charcoal (subject to the conditions aforesaid), and they further show that wide changes in the pressure of the air current has little or no effect in altering the proportions. In another experiment, the vessel containing the saturated charcoal, instead of being allowed to rise rapidly in temperature, was transferred to a vacuum vessel, in which a little liquid air was placed, in order that the temperature might rise slowly, and thereby enable the successive litres of gas given off to be collected separately and analysed.

This experiment gave the following results :—

	Oxygen per cent.
First litre	18·5
Second ,, ...	20·6
Third ,, ...	53·0
Fourth ,, ...	72·0
Fifth ,, ...	79·0
Sixth ,, ...	84·0

The mean composition of the 6 litres is again 56 per cent. oxygen. From the above experiments it follows that one of the most rapid means of extracting a high percentage of oxygen from atmospheric air is to absorb it in charcoal at low temperatures, and thus to expel it either rapidly or slowly by heating the mass of charcoal to the ordinary temperature.

A few experiments have been made using, instead of air, special mixtures of oxygen and nitrogen. Thus it was found that a gas containing 6·5 per cent. of oxygen, used in the same manner as in the air occlusion experiments, gave, on heating up the charcoal rapidly to 15° C., 5 litres of gas having the composition of 23 per cent. of oxygen. A repetition of the same process with the 23 per cent. of oxygen would have raised the percentage about 60 per cent., or a stronger

concentration could have been reached by fractionating the gas as it slowly leaves the charcoal on gradually increasing the temperature.

*Production of High Vacua and Spectroscopic Studies. Separation of Gases like Helium, Neon, and Hydrogen, from Air and other Gas Mixtures.**

The high absorption of gases by charcoal suggested an inquiry into the limits of gaseous pressure reached by such means of condensation. With this object, an ordinary spectroscopic sparking tube A B was sealed to a narrow tube C E, the end of which was blown into a bulb, D E, capable of containing a few grammes of cocoanut charcoal. After the charcoal had been freed from gases by heating and exhaustion, and the poles cleared by sparking during this operation, pure and dry gases like oxygen, nitrogen, air, carbonic oxide, hydrogen, neon, and helium, could be admitted at different pressures and the tube with its attached charcoal chamber sealed off.

Fig. 2.

On placing the charcoal capsule in liquid air, the gas in each case was rapidly absorbed, and the vacuum produced reached the phosphorescent stage, except in the cases of hydrogen, neon, and helium.

A large spectroscopic tube of 1300 c.c. capacity was sealed to a bulb containing 30 grammes of charcoal. When the tube was filled with air at atmospheric pressure and the charcoal cooled in liquid air, the pressure fell to 50 mm. of mercury. On refilling the tube with air at the pressure of half an atmosphere, and treating it as before, the exhaustion reached beyond the striæ stage ; and a final charge at a quarter of an atmosphere gave a vacuum through which no spark passed. When the experiment was repeated with only 1 gramme of charcoal, and an initial pressure of 3 mm. of mercury, the vacuum just reached the beginning of the phosphorescent stage.

When hydrogen was employed, in order to get a vacuum well up in the striæ stage, either a larger amount of charcoal had to be employed or the initial pressure had to be less than an atmosphere. But, if the liquid air bath was cooled to $-210°$ C. by exhaustion, the tube just reached the beginning of phosphorescence round the cathodes. With helium there was very slight absorption, but more appreciable results were obtained with neon. Spectroscopic observations made during the condensation of the gas in the charcoal showed the gradual disappearance of the characteristic spectrum of oxygen, nitrogen, and air, as the high vacuum was reached and the discharge passed with great difficulty. In tubes of this kind filled at atmo-

* Proc. Roy. Soc., 1904.

spheric pressure, the F line of hydrogen and the neon yellow could always be seen; but the helium was not seen with any definiteness. As the amount of neon in the air cannot well exceed $\frac{1}{50000}$th, the spectroscopic test is very delicate.

In order to get the helium spectrum the air in the sparking tube had to be enriched six or seven times. This was attained by the apparatus in Fig. 3. A B is the sparking tube, with its small charcoal bulb C attached, capable of being sealed off at G when required; and D and E are larger charcoal absorbers placed in vacuum tubes containing liquid air; the whole being attached to a graduated gasholder, F, containing air. A series of glass stop-cocks are attached at the points H, I, J, and K, to facilitate manipulation.

In a preliminary experiment to determine the volume of air necessary to bring in the helium lines, 200 c.c. of air were supplied to one of the charcoal vessels D, containing 15 grm. of charcoal, from which the residue was passed on to the sparking tube. This tube gave the hydrogen lines C and F, the neon yellow and some of

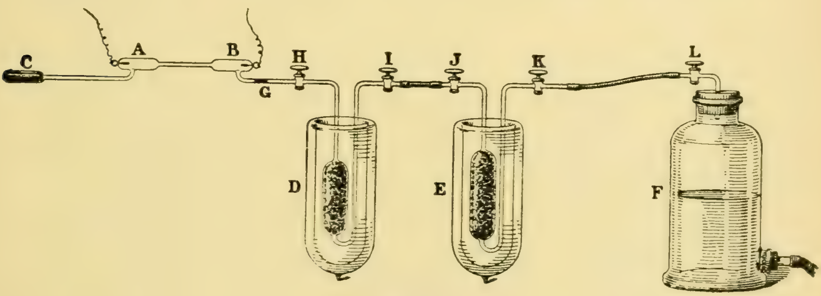

FIG. 3.

the orange lines, along with the helium yellow and green quite distinctly. Another tube with the residuary gas from a litre of air gave all the helium lines as well as the neon yellow and the hydrogen F; from which we infer that by this means $\frac{1}{50000}$th of helium can be detected, so that the test is as delicate for helium as for neon. A third tube, supplied from 3 litres of air, gave the neon and helium spectra and a brilliant ruddy glow discharge.

As 40 to 50 grammes of charcoal can absorb at the temperature of liquid air from 5 to 6 litres of air, it is easy to accumulate rapidly the uncondensed gases in considerable quantities for spectroscopic examination. For this purpose it is convenient to use two charcoal condensers in circuit. After the charcoal in the first one, marked E in figure, was saturated, the stop-cock K was closed, while I and J were opened for a short time to allow the less condensable gas in E to be sucked into the second condenser D along with some portion of air. The condenser E was then taken out of the liquid air, rapidly heated to 15° C. to expel the excluded air, and was thus ready to repeat the

operation. In this way 50 litres of air can be treated in a short time, supplying sparking tubes showing brilliantly the complete spectra of the volatile constituents of the air.

The results derived from the treatment of Bath gas in this way are interesting. This gas, consisting mainly of nitrogen and $\frac{1}{1000}$th part helium, when subjected to the action of the charcoal condenser in liquid air, gives no high vacuum. All the nitrogen and any other constituents are absorbed, and a spectrum of helium and hydrogen showing much less neon than exists in the volatile residue from atmospheric air is the result. A sample of argon prepared from Bath gas treated in the same way, gives a tube showing the helium and neon spectrum; and one prepared from atmospheric air gives a similar result, but the helium spectrum is the stronger in the Bath argon, whereas with the atmospheric argon the neon spectrum is the most pronounced.

Illustrations of various Applications of Charcoal in Experimental Investigations.

The gaseous products from minerals containing helium, hydrogen, etc., also the products from radium compounds, may be treated by the charcoal method. As an example I have applied this method to the crude gases got by heating the mineral Fergusonite. During the cooling of the charcoal the nitrogen and hydrogen spectra were marked, but in a short time nothing could be seen but the lines of hydrogen and helium. It is needless to say that the charcoal method of exhaustion can be applied to the manufacture of incandescent lamps and Röntgen radiation tubes, and that the method can be conveniently employed to produce and maintain high vacuum for the purpose of distilling bodies under low pressures. Many experiments with the radiometer can be carried out by the use of the charcoal method of exhaustion. If a charcoal capsule is sealed to the bulb of a radiometer full of air under relatively high pressure, on directing a beam of light on the vanes of the radiometer they remain at rest, showing that the density of the air is too great for motion to take place. When, however, the charcoal capsule is immersed in a vessel containing liquid air, the vanes immediately commence a rapid rotation. On removing this liquid air bath their motion slackens, and finally they come to rest as the charcoal returns to the temperature of the room.

It is known that dry phosphorus does not enter into chemical combination with pure oxygen at ordinary temperature and pressure. A bulb of from 100 to 200 c.c. filled with pure oxygen, having a side tube sealed on of 2 mm. cross section containing fused phosphorus, the whole being connected by means of a quill tube to a charcoal capsule, containing in part also a good layer of phosphoric anhydride, and the latter immersed in liquid air, the charcoal gradually absorbs

the oxygen, and at a particular pressure the oxydation of the phosphorus vapour begins, and is revealed in the form of a phosphorescent

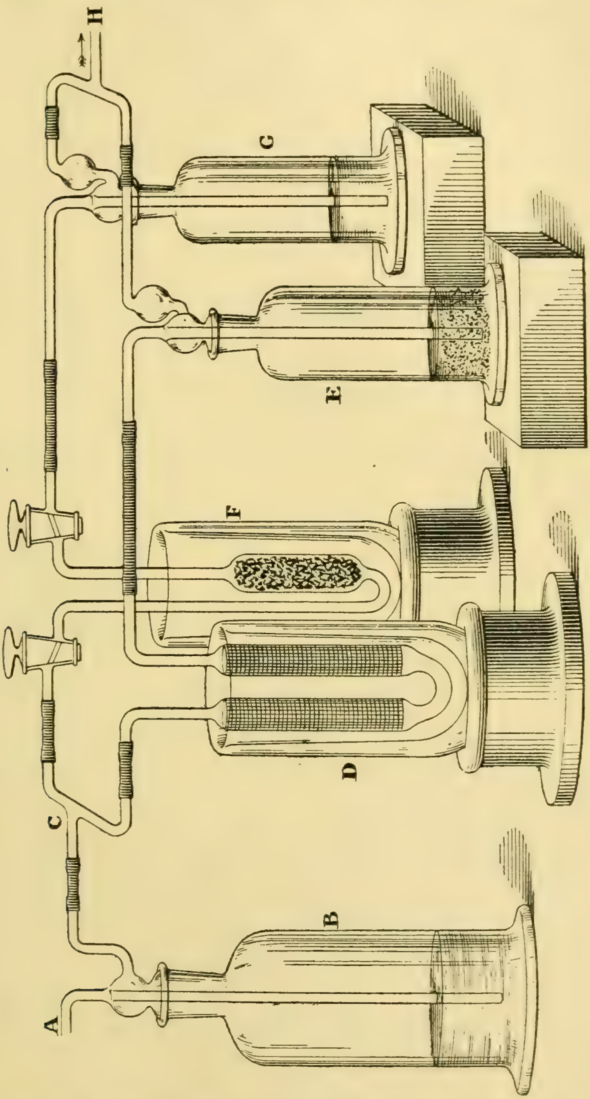

Fig. 4.

glow, filling the whole of the glass bulb, which continues until the oxygen pressure gets too low. On removing the liquid air bath,

at a certain stage of the increasing pressure of oxyyen, the bulb again becomes a glowing mass of phosphorescence, which wanes and disappears as the normal pressure is reached. This experi-

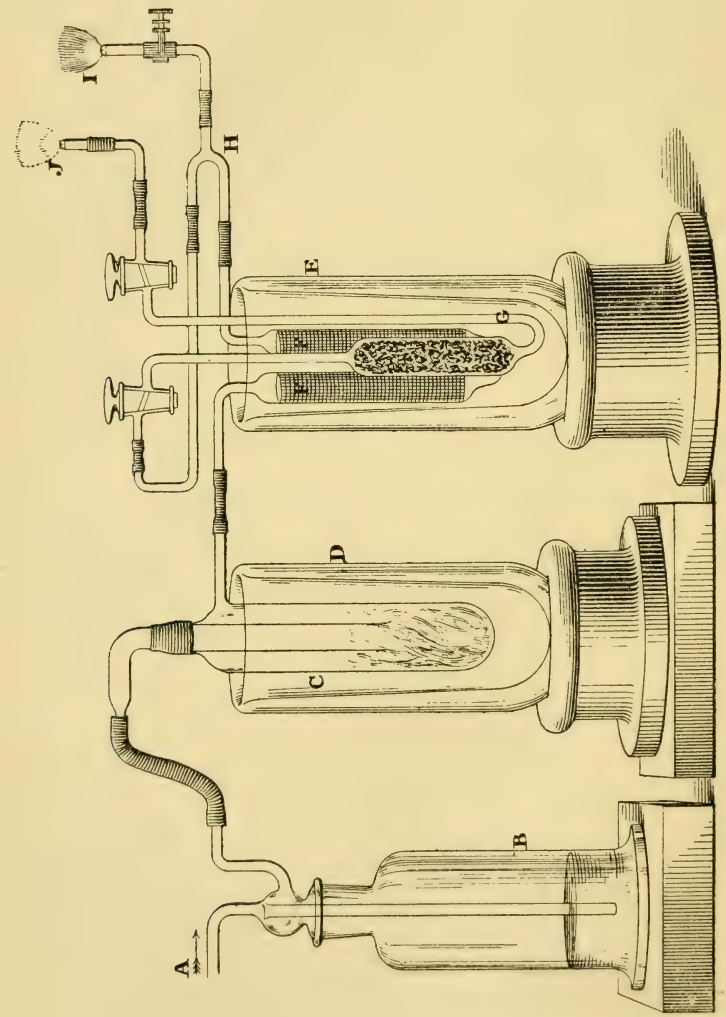

Fig. 5.

ment may be repeated a great many times provided the phosphorus in the small side tube does not get into active combustion, which can always be avoided by a slight cooling. Such bulbs can be

kept in the dark for months, and at the end of the time are quite active when cooled as described. A manometer attached to the side of the oxygen bulb shows that the oxygen pressure at the moment phosphorescence begins is only a fraction of a millimetre of mercury. From these results it appears that perfectly dry oxygen below 1 mm. pressure combines with phosphorus.

The following experiment proving the absorption of carbonic acid, at a small partial pressure, by charcoal is very striking. A continuous stream of ordinary atmospheric air, containing three volumes of carbonic acid in ten thousand, is made to enter the apparatus, Fig. 4, at A and leave at H. It first passes through the jar B, which contains some strong sulphuric acid, by which it is thoroughly dried. On leaving B the stream of air is divided at C into two portions, one of which passes through the jars D, E, the other through the jars F, G, the two finally uniting and passing out at H. D and F are vacuum vessels containing solid carbonic acid, which maintains a temperature of − 78° C. The air passes through a U-tube in D, the arms of which are filled with coils of copper gauze to ensure the complete reduction of its temperature to − 78° C. It next passes through the jar E, containing some baryta water, which absorbs the carbonic acid remaining in it, before it emerges at H. The other stream of air passes in like manner through a U-tube in F, in one arm of which is a quantity of dry charcoal in small lumps; thence, after bubbling through some baryta water in the jar G, it passes to H, where it escapes. Now, the streams of air entering D and F are in exactly the same state, but while E shows by the milky deposit taking place that the one stream is still fully charged with carbonic acid after being thoroughly cooled to − 78° C. in D, G shows by remaining permanently clear that a similar cooling of air to − 78° C. in the presence of charcoal causes, for a time, complete absorption of the carbonic acid from the air, and this goes on until the charcoal absorbs about 1 per cent. of its weight of carbonic acid.

The absorptive power of charcoal for hydro-carbons is shown in a similar manner by the following experiment: common coal gas enters the apparatus in Fig. 5 by the tube at A, and after being dried by bubbling through strong sulphuric acid in the jar B, has the less volatile gases condensed by passing through a vessel C cooled with carbonic acid snow, isolated in the vacuum vessel D. The gas is further purified by percolating through the two copper coils contained in the arms of the U-tube F F, immersed also in solid carbonic acid in the vacuum vessel E, after which it passes on to H, where its path is bifurcated, one branch leading to I, where the issuing gas is lighted, while by the other branch the purified gas passes over charcoal in the U-tube G, also at the temperature of solid carbonic acid, before proceeding to J, where it in turn is lighted. The difference between the two flames is very noticeable, that at I being quite luminous, while the other at J is non-luminous, like a Bunsen flame. The explanation is, that the charcoal at the temperature of −78° C. completely

absorbs for a time all the hydrocarbons, such as marsh gas and ethylene, upon which the luminosity of the flame of coal gas depends, leaving practically carbonic oxide and hydrogen.

This preliminary investigation suggests many fields for further inquiry, and some of these I hope to deal with in future lectures.

My thanks are due to my chief assistant, Mr. Robert Lennox, F.C.S., for valuable aid in the conduct of the experiments; and Mr. J. W. Heath, F.C.S., has also helped in the progress of the work.

[J.D.]

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 19, 1906.

The Right Hon. THE EARL OF ROSSE, K.P. B.A. D.C.L. LL.D. D.Sc. F.R.S., Vice-President, in the Chair.

PROFESSOR JOSEPH JOHN THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, R.I.

Some Applications of the Theory of Electric Discharge to Spectroscopy.

THE luminosity produced by an electric current passing through a gas at low pressure varies greatly in character, not only when we alter the nature of the discharge—as, for example, when we pass from the arc to the spark—but also in many cases at different points of the same discharge. The luminosity may be of one colour at one place and of a very different one at another; a spectroscopic examination shows the spectrum of the same gas often varies considerably as we proceed along the line of discharge. As recent experiments have thrown a considerable amount of light on the processes going on in the different kinds of electrical discharge and at different parts of the same discharge, the study of the connection between the changes in the electrical effects and the changes in the spectra might be expected to throw some light on the very interesting question of the genesis of spectra. Many important points can very conveniently be studied by the aid of Wehnelt's method of producing the current. In this method the cathode is a strip of platinum or a piece of platinum wire on which either a little lime or barium oxide has been deposited. This when heated to redness emits large supplies of corpuscles, and by altering the temperature of the platinum very large variations in the current passing through the tube and the potential difference between the electrodes can be obtained. In our experiments the current has varied from a small fraction of a milliampere to several amperes, and the potential difference from a few volts to several hundred.

The apparatus used is shown in Fig 1. A B is the platinum strip with the lime on it; a thermo-couple, a platinum and platinum-rhodium junction, was fused to this strip and served to determine its temperature; the strip was connected with the earth and was heated by a current passing through the leads L, M; a rheostat was placed in series with the heating current and by means of this the temperature could be altered gradually. The anode was a platinum disc; this was connected with the positive pole of a battery of storage cells, the negative pole of which was earthed; to allow of gradual variations in the potential

difference between the electrodes a potential divider of 100 resistances of 10 ohms each was used. The current through the tube was measured by a D'Arsonval galvanometer, and the potential difference between the terminals by a Weston's volt-meter.

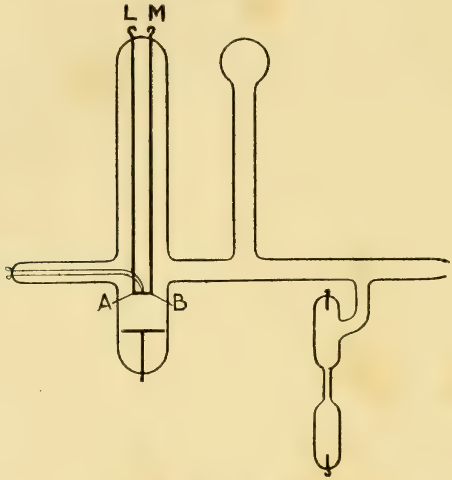

Fig. 1.

Some of the most interesting features of the discharge are very prominent when the temperature of the platinum is high, say 1400° C., and the pressure of the gas low, less than ·01 mm. of mercury. The discharge is light blue, and its spectrum shows the mercury lines and the band spectrum of nitrogen. In this case the relation between the current and the potential difference is represented by a curve like Fig. 2, the ordinates representing the current and the abscissæ the potential difference. G is the point at which the luminosity begins. In the case we are considering, when the wire is very hot and the pressure low, the change from the dark to the luminous discharge takes place very abruptly, an increase of the potential difference by $\frac{1}{100}$th of a volt being often sufficient to convert a discharge, where no light could be detected, even in a darkened room, to one where the light was quite bright. When luminosity appears, there is a very rapid increase in the current; in some of the experiments an increase in the potential difference of $\frac{1}{100}$th of a volt increased the current forty-fold. At this stage the thermopile showed that there was no increase in the temperature of the platinum when the luminosity appeared. We shall see later on that it is possible by using large potential differences to get such large currents through the tube that the platinum becomes appreciably warmer by the passage of the current.

One point which I think very suggestive is the abruptness with

which the luminosity round the cathode appears. We see that by a very small increase in the potential difference the discharge passes from a state in which no luminosity can be detected, even in a dark room, to one where the luminosity can plainly be seen in a bright light ; thus the molecules of the gas in the tube just when the luminous discharge is on the point of appearing, are in a state in which a very small change in the electrical conditions of the tube make the molecules pass from a state in which they are not giving out an appreciable amount of light to one where they are brightly luminous ; and, as the great increase of the current when the luminosity appears shows, this change in state is accompanied by an emission of corpuscles. From this and other considerations, I have come to the conclusion that what takes place when the gas becomes luminous is that the internal energy in the atom in consequence of its bombardment by the corpuscles increases, and when it gets up to a certain critical value the equilibrium of the atom becomes unstable : an explosion occurs, resulting in an expulsion of corpuscles and such a shaking up of those left in the atom that these vibrate so vigorously that the energy radiated is sufficient to produce luminosity. Thus, I regard the ionization of

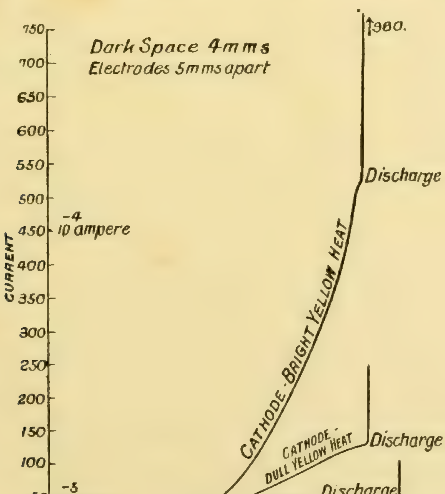

FIG. 2.

the gas as being due, not to the corpuscles in the atom being dragged out by the direct action of the electric forces in the field, or as being knocked out by a rapidly moving corpuscle striking against them, but to an explosion due to the atom having absorbed so much internal

energy that its equilibrium becomes unstable. Other phenomena point to this as the method by which ionization is effected. If the corpuscles are dragged out of the atoms by the electric field, the velocity with which they are projected should depend upon the strength of the field; while, if they are projected by an explosion, their velocity would depend upon the nature of the atom and not upon the strength of the field. Now when Röntgen rays fall upon a substance, the atoms of the substance are ionized, and corpuscles, forming a stream of cathodic rays, are emitted. Barkla has lately shown, however, that the penetrating power of the cathodic rays produced in this way is independent of the intensity of the Röntgen rays. Now the electric force in the Röntgen rays depends upon their intensity, and the penetrating power of the cathodic rays depends upon their velocity; so that this result shows that the velocity of the corpuscles does not depend upon the intensity of the force acting upon them. Again, Lenard has shown that the velocity of the corpuscles ejected when ultra-violet light falls upon a metal is independent of the intensity of the light. Lenard also investigated the secondary cathode rays produced when cathode rays fall upon matter, and found that, in addition to rays, whose velocity was of the same order as that of the primary rays, and which may be regarded as deflected primary rays, there were other very slow rays; and the measurements he gives indicate that the velocity of these varies but little with that of the primary rays.

A point of great importance, which can easily be shown by this apparatus, is that the stage at which luminosity sets in depends upon the current density through the tube, and not merely upon the potential difference. One way of showing this is to lower the temperature of the platinum, keeping all the other conditions the same, and again determine the relation between the current and the potential difference. The effect of lowering the temperature is to reduce the number of corpuscles starting from the cathode, so that with the same potential difference the current density is smaller. If the relation between the current and potential difference are represented by curves, such as those in Fig. 3, it will be seen at once that the curve for the cooler electrode cannot be deduced from that for the hotter by reducing all the ordinates in the same proportion. The critical points on the curves, i.e. the place where ionization by collision begins and where the luminous discharge appears, are at very different potentials: the greater the current density the smaller the potential difference corresponding to these critical points. Thus, to take a case actually observed, when the wire was very hot the discharge was brightly luminous with a potential of 24 volts, but on lowering the temperature no luminosity could be detected with a potential difference of 110 volts.

We can also show the effect of current density without altering the temperature of the cathode, by placing near the tube an electro-

magnet, so arranged that its lines of magnetic force in the discharge tube are along the line joining the cathode and the anode. The effect of the magnetic field is to make the corpuscles move along the lines of force, and thus, without altering the number of corpuscles emitted by the cathode, it concentrates their paths, and so increases the maximum current density in the tube. When the magnet is on, ionization by collision and luminosity both occur at a much lower potential difference than when it is off, and it is easy to arrange matters so that keeping the potential difference constant, the discharge is luminous when the magnet is on and dark when it is off. When the potential difference is too small to produce a bright discharge even when the

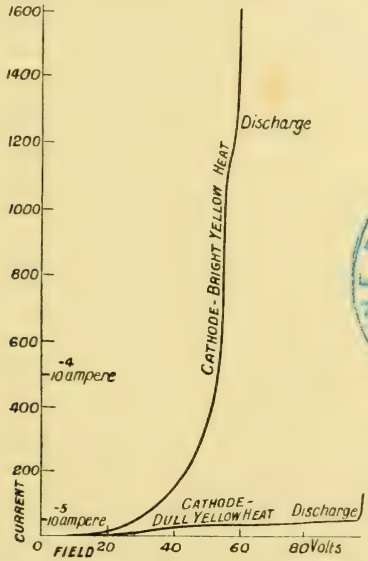

FIG. 3.

magnet is on, the current through the tube is often greater when the magnet is on than when it is off. By placing the magnet so that the lines of magnetic force are across the line joining the cathode to the anode, we can render the paths of the corpuscles more diffuse than they would be without the field, so that the maximum current density is less when the magnet is on than when it is off. In this case it requires a larger potential difference to produce a luminous discharge with the magnet on than with it off. Similar effects produced by a magnet on another kind of discharge are described in my "Recent Researches," page 105.

The potential difference P, just when the glow commences, when

the pressure is low, sometimes varies so rapidly with the current i as to be roughly inversely proportional to it. The following are some values of i and P for a gas at a constant low pressure as the temperature of the platinum strip was increased. The numbers are in the order of increasing temperature :—

i (in scale divisions)	P (volts)	Pi
6	60	360
8·7	40	346
11·2	30	336
14	25	350

Such a simple relation between P and i is, however, exceptional.

The fact that the potential differences at which ionization by collision, or luminosity begin depend upon the current density, shows that the ionization or luminosity of an atom need not, and indeed cannot entirely, be the result of a *single* collision between a corpuscle and the atom. For if that were the case, then, since the energy of the corpuscle depends only upon the electric field and not upon the current density, the effect of increasing the current density would merely be to increase in the same proportion the number of luminous atoms, while as a matter of fact if the potential difference is kept constant and the current increased by raising the temperature of the platinum strip, the increase in the luminosity is greater out of all proportion than the increase in the strength of the current.

The result however, is easily explained if we look at the question from the following point of view. Suppose that for ionization or luminosity to take place the internal energy of the atom must increase by certain amounts, say E_1, E_2 respectively. Then, if the energy possessed by the corpuscle were very great, the result, of one collision with an atom might be to give to the atom enough energy to ionise it or make it luminous, or both. But even if the corpuscle were less energetic, and did not in one collision give enough internal energy to the atom to ionise it, it would communicate some energy to it; and if the atom had any power of storing up energy, this would form a contribution towards the critical amount of energy required by the atom before it is ionised. The atom, after having had this energy communicated to it, would not, as long as it retained any of it, require so much energy to ionise it as before. The atom, too, might acquire energy, not merely by corpuscles striking against itself, but also by the collision of corpuscles with neighbouring atoms. Such collisions generate soft Röntgen rays, the energy of which might be absorbed by the atom under consideration, and help to raise its energy to the critical point. The energy in the Röntgen rays might by itself raise the internal energy of the atom to this value, or else raise it so nearly to this value that the collision with a corpuscle would give it enough energy to carry it past the critical stage. The rate at which the energy, due to collisions of corpuscles with itself or with

neighbouring atoms, comes to an atom will be proportional to the rate at which energy is being communicated to the gas, i.e. to $F \times i$, where F is the electric force and i the current density; and thus for a constant electric force would be proportional to the current density. The atom will radiate away some of its internal energy; if the rate of this radiation is proportional to the amount of energy E possessed by the atom, say equal to β E, then if q is the rate at which energy is being communicated to the atom, we have

$$\frac{d\,E}{d\,t} = q - \beta\,E$$

so, if E vanishes with t,

$$E = \frac{q}{\beta}(1 - e^{-\beta})$$

Thus q/β is the limit to the energy acquired by the atom, and this is proportional to q, while q is proportional to F i; so that the atom will acquire the critical amount of energy, or not, according as F i is greater or less than a certain value.

Application of these Results to Spectroscopy.—We have seen that the passage from the dark to the luminous discharge occurs with great abruptness, an increase of the potential difference by $\frac{1}{100}$th of a volt being sufficient under certain circumstances to convert a discharge in which no luminosity at all could be detected to one where it was quite bright. This suggests that the luminosity sets in when the internal energy of the atom—or rather of that part of it which gives rise to the particular kind of light present in the luminous discharge—attains a perfectly definite value. This way of regarding the origin of the luminosity affords a very simple explanation of the variation of the spectrum with the kind of discharge, and of the effect of introducing capacity or self-induction into the circuit containing the discharge tube. Let us consider the rise in energy of a vibrating system inside the atom. Let E be the energy at the time t; α the rate at which it is absorbing the work done in the discharge tube. The energy may be supplied to it from the Röntgen radiation in the tube, or from the corpuscles which come into collision with the atom: α will be proportional to the rate at which the electric field producing the discharge is doing work in the neighbourhood of the atom we are considering. It will thus be proportional to the product of the electric force and the flux of corpuscles in this neighbourhood. Let us suppose that the system radiates energy at a rate proportional to E, say equal to β E; then we have

$$\frac{d\,E}{d\,t} = \alpha - \beta\,E$$

$$\text{or } E = \frac{\alpha}{\beta}(1 - e^{-\beta t})$$

if E = 0 when $t = 0$.

Consider two different systems A and B in the same atom. Let E_1, α_1, β_1 ; E_2, α_2, β_2 be the values of E, α, β for the systems A and B respectively :—

$$E_1 = \frac{\alpha_1}{\beta_1}(1 - e^{-\beta_1 t})$$

$$E_2 = \frac{\alpha_2}{\beta_2}(1 - e^{-\beta_2 t})$$

Now suppose that the system A is one that does not absorb much, but also does not radiate much, while B absorbs a great deal more than A, but radiates still more in proportion, so that $\alpha_2 > \alpha_1$, but $\alpha_1/\beta_1 > \alpha_2/\beta_2$, so that ultimately E_1 is greater than E_2, but at first E_2 is greater than E_1. The curves (1) and (2), Fig. 4, represent the variations of E_1 and E_2 with the time.

Suppose now that systems A and B become luminous when the internal energy is equal to W. It is not necessary to assume that the critical amount of energy is the same for the two systems, the assumption is only made to simplify the diagram, the reader will see that the argument would apply if the critical amounts of energy were different in the two cases.

Now consider first the case when the rate at which work is being done in the tube is so small that though α_1/β_1 is greater than W, α_2/β_2 is less than W, the case represented in Fig. 4 ; here system A acquires the amount of energy necessary to make it luminous, while system B does not ; thus in this case the spectrum of the gas would show the lines corresponding to A but not those of B. Suppose now

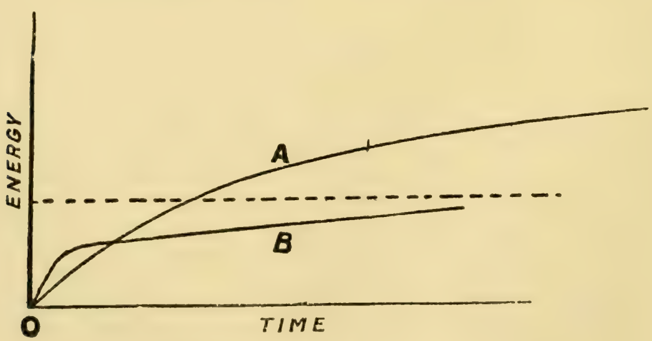

Fig. 4.

we increase the rate at which work is done in the tube, so that both α_2/β_2 and α_1/β_1 are greater than W, the case represented in Fig. 5.

Here the system B attains the critical amount of energy, and it reaches this value before A does, so that in this case the lines of B

will be visible. Let us now consider the lines in the spectrum corresponding to the system A; these will be visible if the energy in the system reaches the critical value; the conditions in this case are in some respects more unfavourable for the supply of energy to this

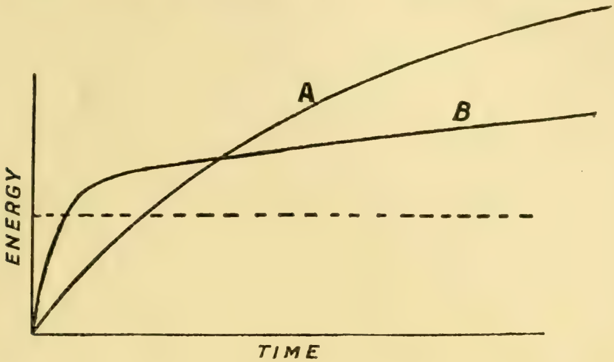

FIG. 5.

system than they were in the previous one. For in the first case the system B got into the condition in which it radiated as much energy as it received, and thus did not absorb any of the energy; in the second case, however, B became luminous before its radiation was equal to the absorption; it is thus taking in more energy than it gives out, and this may result in a diminution in the rate of supply of energy to A; it would do so, for example, to a marked extent if the conditions were such that A received a considerable portion of its supply of energy from B. This diminution in the supply might be great enough to prevent the internal energy in B reaching the critical value. Thus the result of the increase in the rate of supply of the electrical energy might be to weaken or even obliterate the lines of A, and while with the smaller rate we had the lines of A and not those of B, with the larger rate we might have the lines of B and not those of A. Thus an increase in the rate at which the electric field is doing work, such as would be produced by increasing the current through the discharge tube, might result in an entire change of the spectrum. We should expect that it would only be in exceptional cases that the lines of A would be obliterated under the conditions holding in case A, but in all cases the increase in the brilliancy of the lines of B would be large compared with the increase of those in A.

We see from the equations giving E_1 and E_2 that until the supply of energy has lasted for a time comparable with $1/\beta_2$, E_2 is large compared with E_1; thus for electrical discharges which last for an exceedingly short time we might easily have the lines of B visible and not those of A.

In a discharge tube conveying an electrical current, the amount of work per unit volume of the gas done by the electrical forces per unit time varies very largely from one point of the tube to another. If the cross section of the discharge is the same at all parts of the tube, so that the current density is uniform, the rate at which the electrical forces do work will be proportional to the electric force. As this is much greater near the cathode than at other parts of the tube, we should expect the lines of systems of the type B to preponderate near the cathode, and to be absent or much feebler in other parts of the tube. If the tube were of the type frequently used for spectroscopic purposes, with a capillary portion in the middle, then, since the current density is much greater in this portion than in any other, the rate of work per unit volume of the gas will be much greater in the capillary portions than in the wide parts of the tube, and we should therefore expect the lines of systems of the type B to be much more prominent in the capillary part than in the wide part.

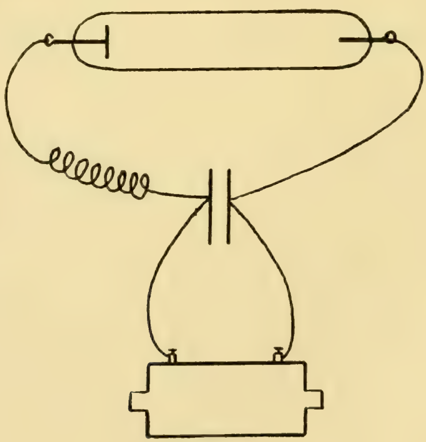

Fig. 6.

Effect of Self-induction and Capacity.—Suppose that we have a tube of uniform bore arranged as in Fig. 6, the terminals of the tube being connected with the plates of a condenser of capacity C, and that there is a coil whose coefficient of self-induction is L, placed in series with the tube; then if the discharge through the coil begins when the potential difference between the plates of the condenser is V_0, the potential difference between the plates after a time t will be

$$V_0 \cos pt$$

and the current through the tube

$$C V_0 p \sin pt$$

where $p = \frac{1}{\sqrt{LC}}$.

Thus the maximum value of the product of the current and the potential difference, i.e. rate at which the electric forces are doing work in the tube, is $C V_0^2 p$, or $\sqrt{\frac{C}{L}} V_0^2$, and is thus proportional to the square root of the capacity and inversely proportional to the square root of the self-induction. Thus, increasing the capacity increases the maximum rate of work and therefore increases the brilliancy of the lines corresponding to systems of the type B relatively to those of type A, while inserting self-induction in the circuit increases the brilliancy of those of type A as compared with those of type B. If we suppose that the "blue" spectrum of argon corresponds to a system of type B, the red to a system of type A, we have an explanation of the changes in the spectrum of this gas, for by inserting capacity in the circuit we can change from the red to the blue spectrum, while having got the blue we can get back to the red by inserting self-induction as well as capacity. I have here a little model which is intended to illustrate the way in which the red and blue spectra of argon originate. It is based on the fact that when we send a current of electricity through a circuit, the current does not rise to its steady value instantaneously, but, starting from zero, increases with the time in exactly the same way as we have supposed the intrinsic energy in the atom, i.e. the way represented by the curve in Fig. 4. The quantity in the electrical case corresponding to the radiation β is the resistance of the circuit divided by the self-induction, while the quantity α is inversely proportional to the self-induction. Thus, a circuit with large self-induction and small resistance is analogous to the system A, while one with small self-induction and large resistance is analogous to a system of type B. Now my model of the argon atom consists of two circuits, C and D, placed in parallel. C has large self-induction and small resistance, D has little self-induction but large resistance. An electric lamp is placed in each circuit. If I supply energy in one way, i.e. by continuous current to the system, the red lamp in C lights up and the blue lamp in D is dark, while if fed by an alternating current, the blue lamp shines and the red is dark. It would be interesting to see whether, as we gradually diminish the self-induction, we get the whole of the lines in the blue spectrum at once, or whether the lines of this spectrum enter in groups one after the other. I have tried somewhat similar experiments with the hot lime cathode to see in a mixture of gases (mercury vapour and air) which spectrum first appeared as the rate of doing work in the gas was gradually increased. The great difficulty in this determination is that when once the luminosity begins there is such a rapid increase in the ionization that the current through the gas and the rate of doing work increase in an exceedingly short time through a wide range of values, and thus a gradual increase in the rate of work is exceedingly difficult to obtain; on several occasions, however, I was convinced that on gradually increasing the rate of work, the mercury

lines were the first to appear, and were the last to disappear, when the rate of work was reduced from a high value, at which both the nitrogen and mercury spectra were bright down to a point where the discharge ceased to be luminous.

The preceding considerations have also an important application to the difference between the arc and spark spectra. In the continuous arc discharge, although the average rate of work is much higher than in the spark, the maximum rate is very much less; in the spark discharge we have exceedingly intense current density lasting for a very short time, and while the spark is passing we have a very much greater rate of work than in the arc. Hence the state of things in the spark will be analogous to that represented in Fig. 5, and the lines corresponding to systems of the type B will be enhanced relatively to those of type A; we conclude then that the arc lines correspond to systems of the type A, the spark lines to those of type B.

The work done in the discharge tube is probably ultimately converted for the most part into heat, so that the rate at which work is being done at any part of the tube is approximately proportional to the rate at which heat is being produced in the tube. I do not, however, regard temperature, i.e. the energy due to the translation of the atoms as a whole, as having any direct connection with the production of spectra. The work done by the electric field on the corpuscles is, since the corpuscles can easily penetrate the atoms of the gas, first converted into internal atomic energy. This energy may ultimately be for the most part transformed into the energy of translation of the molecules of the gas and so appear as temperature, but it by no means follows that, if we heated the molecules of the gas by non-electrical means to the temperature to which even a few of its molecules are raised by the electric discharge, that we should get a luminous spectrum. The production of the spectrum depends upon the internal energy of the atom. When we use the electric discharge, all the work done by the corpuscles goes at first into the form of internal atomic energy, while if we supplied the same amount of energy to the gas by thermal, as distinguished from electrical means, the energy would go first into increasing the energy of translation of the atoms, and very little of it would ever get inside the atom. It is probable, however, that some of the energy of translation would get converted into internal energy, and that temperature is one way of giving internal energy to the atom and so producing luminosity. From one point of view, however, it is a very extravagant method, as the fraction of the energy spent in heating the gas which goes in producing luminosity is small.

The coefficient of absorption α of the systems will depend upon the way in which the internal energy is given to the atom, as well as upon the rate at which the electric field is doing work in the neighbourhood of the atom. Thus, for example, if the internal work is given by means of rapidly moving corpuscles, the coefficient of

absorption will depend upon the velocity of the corpuscle, for we can easily show that when a corpuscle passes at a fixed distance from a system of corpuscles having a definite period of vibration, there is one velocity of the corpuscle, depending on this period—fast if the period is short, slow if it is long—for which the energy given by the corpuscle to the system is a maximum. Thus the relation between the amounts of energy absorbed by two systems from the corpuscles depends upon the velocity of the corpuscle. The velocity of the corpuscles in a discharge tube depends upon the pressure of the gas, so that even though the rate at which the electrical forces are doing work may be the same at two different pressures, the relative intensities of the lines of two systems A and B may be different.

Again, we might expect that the coefficient of the rate of absorption of energy would be different according as the energy is given to the atom by means of the large systems which form the positive ions, or by means of small corpuscles; and that the relative brightness of lines might be different in the two cases. In the Canal-Strahlen we have positive ions moving through gas and producing luminosity, and the spectrum of this luminosity possesses interesting peculiarities differentiating it from the spectrum of other parts of the tube. Perhaps the most striking difference, however, is when the positive ions strike against a salt like lithium chloride: they make the red lithium line appear with great brilliancy, while if corpuscles strike against the chloride the red line is not visible. It is remarkable that the spectrum of the metal is produced much more readily by the positive ions when they strike against a salt of the metal than when they strike against the metal itself. This is shown in a striking way, if we take the liquid alloy of sodium and potassium and direct a stream of Canal-Strahlen upon it the clean parts of the alloy appear quite dark, but the specks of oxide scattered over its surface shine with a bright yellow light, giving the sodium spectrum.

When the internal energy of the atom is increased by means of light, as in Professor Wood's beautiful experiments on the fluorescence of sodium vapour, the coefficient of absorption of a system will depend upon the relations between the periods of that system and the period of the light vibrations incident upon them. Thus, as Professor Wood found to be the case, the numerous lines in the spectrum given out by the vapour alter greatly in character and wave-length when the period of the incident light is changed.

The same principles which explain the variation in the intensities of the spectra given out by two different systems in the same atom, can be applied to explain the variations in the intensities of the spectra of two gases A and B when these are mixed together. We know that under some conditions the lines of only one constituent of the mixture appear, while under others we get the lines of both the gases. Let us suppose that the lines of A appear with a lower rate of work of the electric forces than those of B, and that we send a

constant current through the discharge tube. We can calculate what the electric force must be to produce from the molecules of A alone the number of ions required to carry this current; having found the electric force on this supposition, we can, knowing the current, find the rate at which the electric forces would be doing work in the tube. If this rate of work is less than that required to make B luminous, the current will be carried by the ions of A alone, and the spectrum of B will not be developed; if the rate of work on this supposition is greater than that required to make B luminous, the spectrum of B will appear, and it must take a share in carrying the current. Let us suppose that we have so much of A present that the rate of work is not sufficient to develop the spectrum of B, and consider what will happen as the proportion of A is diminished. In order to supply the number of ions required to carry the given current from the smaller number of molecules of A, the electric force, and therefore the rate of work in the tube, must, on the supposition that the current is wholly carried by A, increase, and if we continually diminish the amount of A present, the rate of work will at last reach a value sufficient to make B luminous, with the given current. This stage will give the smallest quantity of A which can, for the given current, wholly swamp the spectrum of B. The rate of work done in the tube will depend on the current going through it, and also on the pressure of the gases, so that both these quantities will influence the proportion of the gas B required to make its spectrum visible.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, January 26, 1906.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Arthur C. Benson, Esq., M.A.

Walter Pater.

In this mysterious world in which we live, this short space of sun and shade, of tears and laughter, there is nothing that is more mysterious than our relation to, and our knowledge of, other personalities, those men and women who are bound on the same strange pilgrimage as ourselves. How little we know of those who are nearest to us! How little we know of ourselves! How often are we confronted with the fact that we ourselves, no less than those whom we believe we understand, are swayed and guided by forces which are quite different from, and infinitely more strong than, the motives by which we believe ourselves and others to be influenced! How common an experience it is, when we look back upon a crisis in which we believed at the moment that we were acting spontaneously and decisively, to discern, as time goes on, that our action was the inevitable outcome of our circumstances, and that we had in reality little choice in the matter! Herein lies the extraordinary interest of biography, that in the records of a man's life we can trace to what extent he moulded his own career, and how far it was moulded by temperament and circumstances.

The man whose life and work I propose to depict tonight has the special interest of exhibiting a very marked and salient type of character. Walter Pater is a supreme instance of a man of creative and artistic temperament. The great mass of mankind is mainly swayed by what may be called material motives. The necessity for earning the means of subsistence, for supporting a wife and family, in a commonwealth where, though there is a considerable accumulation of wealth, it seems as if there were not enough work to go round, must for most people be paramount. But if we take our analysis a little further, we shall find that there are two marked types of temperament that are swayed, if not mainly, at least to a considerable degree, by forces which we may call spiritual—the religious and the artistic temperament. The basis of the religious temperament is that it conceives itself to stand in a certain personal relation to God. This belief has innumerable forms. But the essence of it is that the religious person believes that there is a certain significance in his life

and acts, a significance which is not bounded by his mortal span of life, that he is guided, inspired, encouraged by a divine spirit whose dictates he endeavours to obey, even when they appear to be at variance with his own convenience, his own direct interest, his own material desires. The religious temperament works mainly in what may be called the ethical region, and its aim is right conduct, the practice of virtue.

The artistic temperament is in many ways closely akin to the religious temperament. There is the same sense of being in a certain relation with a spiritual power, but the message of the spirit appears to the artistic nature to come mainly through a certain quality in things, which we call beauty, which meets us at every turn, in field and wood, in mountain and plain, in stately buildings, in the bearing of gracious persons, and in the representation of these things, whether through the medium of painting, or of sculpture, or of music, or of words. The artistic temperament is probably commoner than is generally supposed, because it is as a rule imagined that it implies a certain degree of productiveness; and a person is not commonly held to be artistic, unless he tends to write books, to dabble with pigments, or to gravitate towards musical instruments. But there are many people of artistic nature, who are strongly affected by beauty in certain forms, and who yet have no productive outlet for their visions. Sometimes these are tranquil and contented people, to whom things of beauty are a source of serene and joyful refreshment; but very often they are discontented and fastidious natures, who are conscious of subtle perceptions, and tend to believe themselves to be more interesting than others are inclined to admit, who are strongly conscious of dreariness and dissatisfaction, with an underlying feeling that they are not understood or appreciated.

Walter Pater was a supreme instance of the artistic temperament. He was conscious, at an age when most children are entirely absorbed in material things, of the appeal of beauty to his spirit, even before he could have given it a name. The life of one so constituted cannot be an entirely happy thing, because fineness of perception, in a world where much is hard, painful and dark, carries a certain shadow with it. But Pater had the supreme happiness of devotion to a particular kind of artistic work, the art of beautiful expression. One who embarks upon such a pursuit cannot indeed ever wholly satisfy himself; his performance can never be so perfect as his conception. But he tastes of many joys by the way, the joy of perception, the joy of dreams, the joy of creation, the joy of congenial labour. Through misunderstanding and appreciation alike, through misrepresentation and admiration, this man held his steady way, and his life may inspire us to be faithful to light, and not to be disobedient to the heavenly vision.

The name Pater appears to be Dutch in origin, but though we may be apt to see a certain Dutch element in Pater's work, the

arriving at a beautiful effect by an amassing of minute touches, yet there is no certain record that there was Dutch blood in his veins. The Paters were a simple country stock, who are found living at Weston Underwood, in Buckinghamshire, and enjoying the friendship of the poet Cowper in the eighteenth century. Then came a migration to America. The father of the great critic, a physician of tender and unobtrusive benevolence, returned to England, and settled at Shadwell, near Stepney, where he laboured diligently at the relief of suffering among the poor. But he died young, and Walter Pater, who was born in 1839, was brought up in the quietest of homes at Enfield.

Those to whom, what is perhaps almost the most exquisite of all Pater's writings, a little study, called *The Child in the House*, is familiar, will be able to form a picture of the early days of the quiet perceptive child. It was always about the home, the house, the garden, that Pater's tenderest memories centred and lingered. He carried with him all his life a fragrant memory of the scents, the furniture, the books, the rooms of the old house, the perfumed air wafted slowly in through open windows on the still summer afternoons, the pleasant, firelit hearth of winter evenings.

Side by side in childhood with this acute and delicate perception of beauty, which we do well to remember was hardly consciously exercised at the time, ran two other preoccupations, so characteristic of Pater's attitude that I will mention them here. One was a strong preoccupation with ritual. He liked to devise little ceremonies in which the children, vested gravely, moved about with a due solemnity—the other was a deeper thing; very early the boy became aware of that strange dark fibre knit up with the bright texture of the world, the thread of sorrow; the stream of falling tears made itself audible to him. The sorrow of loss, of sweet things that have an end, troubled his childish philosophy. The terror of death imprinted itself with a shocking horror on the tender mind.

"At any time or place," he writes of the child, "in a moment, the faint atmosphere of the chamber of death would be breathed around him, and the image with the bound chin, the quaint smile, the straight, stiff feet, shed itself across the air upon the bright carpet, amid the gayest company or happiest communing with himself."

It may be thought that there was a strain of morbidity here, but there was nothing yet that was morbid about the child; he was merry, self-absorbed, occupied in little pursuits and aims, like all clever children; it was only that he had an extraordinary receptiveness of impressions; he did not analyse them, or wish to know the reason of things; he had no premature ideals, none of that painful consciousness of impressive virtue which is sometimes so disconcerting a thing in childhood, which ought, above all things, to be humble and unaffected. He merely perceived; and all that he saw or felt imprinted itself with a fine exactness upon his mind, and with

a permanence which made his memory a treasure-house of delicate impressions and vignettes.

It is interesting to note that when Pater was quite a child he went to stay with some friends near Hursley; here he saw Keble, who, being very fond of children, took a great fancy to the quiet, serious boy, walked with him, and spoke to him of the religious life in a way which made a deep impression on his childish mind.

At fourteen Pater was sent to the King's School, Canterbury, but very little is recorded of his school life; he was regarded as a backward, amiable boy, and was popular with his companions in spite of a pronounced dislike to games. It seems that he was wholly unambitious in the earlier years of his school life, and probably indulged his taste for dreamy reverie, keeping the inner life of thought, as is common with clever, reticent boys, absolutely apart from the current of school life, and sharing his visions with none. We have, however, another interesting piece of writing which reflects this period of his life, just as *The Child in the House* reflects the earlier days. It is the study called *Emerald Uthwart*, and is a fantastic and tragic little story, of which the chief interest is, in the first place, that a fine autobiographical thread is interwoven with the narrative, and in the second place, that Pater tries to depict a certain straightforward, unreflective, submissive type of English character, which he seems to have regarded as the characteristic product of an English public school education, though it was very unlike his own. Emerald Uthwart is sent to a school of which the details are unmistakably drawn from the King's School at Canterbury, and thus we get some idea of the impressions which, at this period of his life, had a strong and abiding effect upon Pater's spirit. The beauty of the great Cathedral, rising from a paradise of lawns and gardens, with houses of every date and style, mellowed by time, and with an air of settled and dignified prosperity about them, seems to have sunk deep into his mind. "If at home," he writes of Emerald Uthwart, "there had been nothing great, here, to boyish sense, one seems diminished to nothing at all, amid the grand waves, wave upon wave, of patiently-wrought stone, the daring height, the daring severity of the innumerable long, upward ruled lines, rigidly bent just at last in due place into the reserved grace of the perfect Gothic arch; the peculiar daylight which seemed to come from further than the light outside."

The only definite artistic influence under which Pater is known to have fallen in his schooldays, is the influence of Ruskin. He found out Ruskin in his nineteenth year, and devoured the volumes greedily. There is a trace of Ruskin's influence discernible in all Pater's work; there is the same charming naïveté and transparency in the best passages of both, the same richness of language; but here the likeness ends, because Ruskin used his profuse vocabulary lavishly, and sent it pouring down in a sparkling cascade, while Pater produced his

effects by a severe economy, refining, calculating, adjusting, and compressing.

All this time there is no hint of precocity about the boy; as a rule the children who are to develop into great writers, have an early creative impulse, and blacken paper industriously from their first years; but there is no reason to believe that Pater did anything of the kind.

In 1858 he entered at Queen's College, Oxford, whose great, open, Italian screen and cupola abutting on the High Street are familiar to all who know Oxford. Here Pater lived a very quiet and secluded life with a few friends, in the plain back court of the College. He worked with moderate industry at his classical books, and experienced the attractions of metaphysical study. The only person who seems to have divined his powers was Jowett, whose lectures he attended, who said to him once, in one of those lean dry phrases which seem to have had such a singular effect in stimulating the minds of the young men to whom they were addressed, "Mr. Pater, I believe you have a mind which will one day come to great eminence." But Pater failed to do himself justice in his examinations, and only took a second class in Greats in 1862. For a time he lingered on at Oxford taking pupils, but in 1864 he was fortunate enough to be elected a Fellow of Brasenose, where he at once went into residence; it was to be his home for the rest of his life.

It is one of the ironies of fate that Pater's name should be connected with a college that, during the greater part of the time that he held office there, was pre-eminent for athletics, and for a youthful ebullience of spirits among its undergraduates that is known by the name of rowdiness. The college itself consists of two courts, one of which turns a black and blistered Gothic front upon the Radcliffe Library, the other, mainly modern, extending to the High. It is an austere-looking place, a real fortress of study. It has no very conspicuous feature, and is not set, as so many Oxford Colleges are, in a trim and sunny garden. The chapel, which holds Pater's monument, is a stately Renaissance building, with a curious infusion of Gothic. Inside, it wears a dignified classical air, as though it were more perhaps in love with the solemnities of religion than its essence, as though it desired more to record the tabernacling of God with man than the aspiring of man to God, which Gothic somehow seems to bear in view.

In a corner of the front court a little staircase winds barely up, dark and narrow, to the first floor. Here is a door, low and grim, set in a thick wall, which admits you to a small panelled room, with a deeply recessed oriel window looking out on the Radcliffe Library. A trace of Pater's dainty ways still lingers in the pretty ironwork of the doors brought by him from Brittany. This was his only sitting-room. Here he wrote, read, taught and took his solitary and simple meals. His outer door, which could be closed to keep out intruders,

is said always to have stood open. Men are constantly passing the door, if that is the right word to use of an undergraduate going upstairs. Otherwise there is no noise, and in this small quiet room were written and re-written some of the most elaborate and delicately wrought pages of English prose that have ever been put on paper. Out of the room opens a door into a tiny vestibule full of cupboards, which admits you by a low Gothic arch into a narrow little bedroom of odd shape, such as are often found in the older colleges. Here for over thirty years Pater slept during term-time. In latter years he was an indifferent sleeper, and to beguile the slow hours worked leisurely through the *Dictionary of National Biography* volume by volume. He had constant opportunities of changing his rooms, but partly from economy, partly because he liked the outlook, he would not change. The sitting-room was simply furnished, but a few beautiful things, and care as to colour and arrangement, and great neatness, gave a mingled effect of grace and simplicity, which was always characteristic of his surroundings.

The interesting thing is that this was the environment of the man whose teaching is by many supposed to be the apotheosis of intellectual luxury, the life of Epicurean sensation, the resolve to taste and to enjoy the quality and fineness of beauty, to crowd life with delicate pulsations ; and all this was indeed a part of Pater's creed ; but there was to be no surrender of life to lower delights, no pursuit of grosser pleasure ; it was to be rather a life of spare austerity, a constant disregarding of lower impulses and bodily satisfaction, a pursuit of higher thought and purer beauty, the life of a Saint in art. And again this was not to be a purely self-regarding thing, a mere surrender to agreeable and delicate sensation. If these things were perceived and apprehended, by resolute practice, by unflagging patience, they must be shared with and interpreted to others. It was to this end that Pater's laborious life was dedicated ; not only to see in life and art what was beautiful and refining, but to toil incessantly, day by day, to bring the same joys home to others, whose perceptions were less acute, whose aspirations were more fitful.

Pater's view of his Oxford life was that it was primarily to be a life of solitary study and quiet creative work. At the same time he discharged such educational duties as fell to him with severe and conscientious labour. He spent an amount of toil upon his college lectures, which few people can have ever given to such work ; instead of being brisk and business-like expositions of classical texts, hasty improvisations, extempore comment, they were elaborately written, carefully balanced discourses. The men who attended his lectures purely from the examination point of view were apt to consider them vague and unpractical ; but those whose aim was mental stimulus, the enlarging of the intellectual horizon, found in them an extraordinary sympathy and vitality. Pater seemed to approach Greek thought and Greek life from the inside rather than from the outside.

The clear light of his vivid mind shone not *upon* the subjects of which he treated, but *through* them. He was eclectic indeed in his treatment, and attached perhaps a disproportionate value to æsthetic and poetical tendencies; he did not perhaps sufficiently emphasise the intense political and national activities of the Greeks. It was said of him laughingly that he was a philosopher who had gone to Italy by mistake, in search of philosophy, instead of to Germany; which means that he attached a greater importance to artistic interests than to metaphysical; but the fact remains that he understood and interpreted a certain side of the Greek mind, its rapturous delight in the beauty of thought, of form, of colour, in a way which Englishmen as a rule are but little equipped to do.

Besides his lecturing, he held for a long time the tutorship of his college. And here he was not considered to have wholly succeeded. In the educational part of the work, the reading and criticising of his pupils' essays, he displayed his best qualities.

He was also endlessly kind in discussing any practical questions, such as the choice of a profession, with his pupils, and above all things laboured to remove any scruples or difficulties in the minds of men who had intended to take orders, and found themselves doubtful of their vocation. But in the practical part, the business of his tutorship, he was not wholly successful; indeed, his whole view of the position was not quite positive enough; he did not consider himself a kind of schoolmaster whose duty it was to urge and goad men to work; he did not think that there was any question of compulsion in the matter; he rather considered that it was not his business to communicate impulse, but to be available for purposes of consultation and advice. Those who know the English undergraduate will realise that though this may be effective enough with the best men, yet the average man is not inspired with a sufficiently ardent love of knowledge for its own sake to profit by the advantages offered him so unobtrusively and delicately.

Now I am anxious to make it clear that Pater, at the beginning of his academical life, was a very different person from what he afterwards became. After his election to a Fellowship at Brasenose, his whole nature seemed to awaken; the frozen, solitary, and inarticulate mood broke up and vanished. He became aware that he could hold his own in conversation, in argument, in wit. He experienced the delight of intellectual collision, the pleasure, after his shy, secluded, and indolent boyhood, of finding that his interests expanded, his zest in life increased, and that he was the equal or superior of men who had before maintained a social superiority over him. He became a brilliant and paradoxical talker—so paradoxical indeed that he was often grievously misunderstood. "I would not go abroad," said Mark Pattison, on one occasion, with characteristic peevishness, "with Pater for anything. He would maintain that the Channel was not the Channel, that the steamer was not a steamer, and that Calais was not

Calais." He became intolerant and indiscreet; his talk, for instance, in early days, showed a direct hostility to Christian principles; he defended artistic independence in matters of morality so frankly, that many good people became suspicious of his own code of ethics.

He plunged moreover into authorship. He abandoned the metaphysical studies in which he had taken his chief delight; he made the discovery that art was the influence to which his spirit responded, and of which he had all his life, dumbly and unconsciously, been in search. His conversion, if I may use the word, dates from his study of Otto Jahn's *Life of Winckelmann.* Winckelmann was a young German, who after a toilsome, starved, and stunted boyhood, suddenly discovered, with a shock of natural joy, the stimulus, the appeal, the constraining charm of Greek art, and flung himself with fierce ardour into the study of ancient sculpture. In this, and in a series of romantic friendships, he spent his short meteoric life. The example of Winckelmann appealed to Pater as nothing in his life had ever appealed to him before; and the little study of Winckelmann, which he completed in these early days, is not only a very beautiful thing in itself, but casts a bright light upon the opening soul of the writer.

Pater's method was at first to produce, with infinite retouchings and endless delays, a little study of some artistic or literary figure; and he chose for his period the Renaissance, when the wave of Greek literature rolling westward, set free, it is said, by the taking of Constantinople by the Turks, caused a strange mental stir in Europe, at a time of eager speculation and wild unrest. Leonardo da Vinci, Michelangelo, Botticelli, and other august figures, were taken by Pater as types of this alert and blithe spirit of intellectual and artistic enjoyment. In 1873, when he was 34, he collected these scattered essays into a volume, *Studies of the History of the Renaissance.* Up till this time he had been considered a young don, of ingenious wit and dilettante tastes; but the book produced a deep impression. It was realised that here was a new voice, speaking of subjects which were generally neglected, with a force and a conviction which could not be gainsaid. It was a new philosophy of life; and the effect was heightened by the elaborate and impressive style in which the book was written. It might be called languid, enervating, oppressive, over-scented, but for all that it was clear that the book had an extraordinary beauty of its own, that it was written in a species of highly-wrought poetical prose that had never been exactly attempted in English before. Luxuriant and gorgeous as it was, it was at the same time refined and restrained; it aimed at a severe economy of effect, and the long intricate sentences were haunted with rich echoes and cadences; it evoked a whole host of images of a shadowy and remote beauty; it seemed full of subtle fragrance and delicate colour; whatever might be said of its possible effect, there

was at all events no doubt that it displayed qualities of high and seductive art.

But Pater had to pay a penalty for his candour and for his daring. In 1877, the year in which a second edition of the *Renaissance* was called for, there appeared one of the most brilliant and suggestive satires of the century. The *New Republic*, written, it is astonishing to reflect, by Mr. W. H. Mallock, when he was almost an undergraduate, represented a gathering of brilliant and talented people in a country house, who discussed various aspects of modern life. The characters were thinly veiled portraits of the celebrities of the day, such as Matthew Arnold, Ruskin, Huxley, and Jowett. Among these was introduced a languid and dreamy person, Mr. Rose, with new and startling theories about art, upholding a kind of sensuous and emotional paganism. The portraiture was made unmistakable, in almost all the characters, by the introduction of sentences from the published writings of the great men thus parodied, into their talk. It must be confessed that Mr. Rose is a supremely undesirable personality. He creates in the mind the impression which is best expressed by a mysterious phrase culled from the police reports, when a man is charged with being a suspected person. Mr. Rose is a suspected person. The dreamful beauty of his talk only thinly veils a reckless and seductive paganism. It is ridiculous to find pompous fault with the youthful author of this extraordinarily brilliant satire for not having foreseen contingencies. Mr. Mallock was caricaturing in this typical figure a species of æsthete, of the school of Maudle and Postlethwaite, that was beginning to emerge at Oxford and elsewhere. These young men made a kind of gospel out of Pater, without imitating the austerity of life and the lofty intellectual standard which he himself upheld. Moreover, the indiscretions of his sparkling talk, the blithe gaiety of mood which led him in those days to aim at startling sedater persons, lent themselves to misrepresentation. The result was that Pater was unjustly identified with Mr. Rose, and the identification caused him considerable pain. As he said once, pathetically, to Mr. Gosse: "I wish people would not call me a Hedonist; it creates such a bad impression in the minds of people who don't know Greek." Unfortunately, there were many people who did not know Greek—who knew Pater only through his books. Jowett himself, who was severely parodied in the *New Republic* as the enlightened Latitudinarian, took fright, and deliberately set himself to thwart Pater's academical ambitions—they were not reconciled till the end of Pater's life.

But Pater worked quietly on, producing his little masterpieces. In 1878 appeared, among other pieces, *The Child in the House*, which I would unhesitatingly recommend to anyone who may wish to make acquaintance with the finest flower of Pater's production. It is just a study of the early impressions of a perceptive child, and is obviously

autobiographical in many parts ; but in its fineness, its sweetness, its golden tenderness of retrospect, it seems to me one of the most perfect pieces of pure art.

In this year the idea of a great masterpiece began to shape itself in Pater's mind, and he fell for some years into an apparent silence. In 1885 appeared the book by which he is best known, *Marius the Epicurean*, produced when his imagination, his skill, and his mental vigour were at their highest.

I will not attempt to discuss the book at length here ; it is a picture of a solitary and meditative nature, of strong intellectual force, and of a virginal purity of soul. Marius is a young Roman living in the time of the great philosophical Emperor Marcus Aurelius. The time was carefully chosen, and the background is studied with incredible minuteness ; but there is no parade of erudition ; one merely feels that the little touches of detail are selected with a singular fineness from a great treasure of accurate knowledge. I have said that the time was carefully chosen. It was an era of tolerance, when the courteous Stoicism of the Emperor pervaded the court, and made lofty thought fashionable, and when also Christians enjoyed an entire liberty of action and opinion.

Marius is gently led from the old ritual religion of Paganism, through the courts of philosophy, to the very doors of Christian teaching. He dies, indeed, technically a Christian, but it seems as though the art of the creator had halted before the difficulty of drawing a picture of the inner, Christian life, which should reconcile within itself the appeal of art and the speculations of philosophy. Marius's conversion to Christianity, if it can be so called, is effected mainly by æsthetic processes. The celebration of the Christian Eucharist, and the manifestation of Christian joy smiling through the agonies of natural sorrow, in the presence of death, are what affect him most deeply. But the whole book is a masterpiece of literary skill ; it is full from end to end of the most delicate vignettes, and is used by many readers as a kind of beautiful scrap-book. That is a great mistake, because the structure of the book is very firm and clear, and the development of the thought of Marius is very careful.

Of course, again the supreme interest of the book is that it is autobiographical. Marius is none other than Pater himself, because it must be remembered that Pater was a man who could not enter into intimate relations with others ; he could be kind, endlessly patient, sympathetic ; but he was essentially solitary ; and thus all his writings are like mirrors, on which, in the midst of all the carving, the intricate framework, the lavished ornament, for ever falls the grave face of the craftsman himself.

I have no doubt that Pater often regretted the size, so to speak, of the canvas on which the picture of Marius was drawn. He felt more at home in smaller, more limited scenes ; he said as much to a

close friend when he was nearing the end of his great labour; and it may be said, too, that in the course of writing *Marius* he passed from youth into settled manhood. He withdrew for those years into a secret chamber of thought; no sound, no hint came from within to indicate what the worker was doing in his lonely hours; the metal rang faintly within; the smoke of the furnace ascended; but no one was admitted to a sight of the toil. He entered his house of labour a brilliant, wayward, almost reckless youth; he emerged from it a grave, kindly, solid-hearted man. He entered it a fitful, ornate writer; he left it a profound master of his art. He entered it a man of fluctuating impulses of soul, drawn this way and that by metaphysical and artistic speculation, not knowing what to think, and disguising his vacillation under a glancing irony; he emerged a man of one vision, with a tender, hopeful, religious attitude of spirit, if not fully a Christian, at least more Christian than anything else. All religious beliefs are bound to be a balancing of probabilities, issuing, perhaps, in practical certainties; but there are many dark and insoluble mysteries which the brightest creed can hardly illumine, but in this book Pater seems to have laid his hand upon a clue, which, if it did not reveal the heavenly vision, at least seemed indeed to lead the pilgrim thither.

In the year of the publication of *Marius*, Pater took a house in London, in Earl's Terrace, Kensington. But he did not give up his Oxford work, residing there in the terms. His college work was now less absorbing, because he had resigned his tutorship, though he still continued to lecture. In London he went out a good deal into society in a quiet way; the publication of *Marius* had given him a real position in the literary world, and he found himself welcomed and honoured. His chief work at this time was the interesting series of studies which he called *Imaginary Portraits*. His method was to select some typical and characteristic figure in the past, to study its background very carefully, and then to produce a highly finished picture; it was the kind of work which was most congenial to him; it gave him an outlet for his pure imaginative and creative force, and it also gave full scope to his extraordinary power of producing an historical or artistic effect by selecting delicate and characteristic touches of scenery or environment.

He was at work, too, on a great unfinished book *Gaston de Latour*, of which a few chapters have been published. There are other chapters in existence, but in a fragmentary state. Gaston is a young French noble, who lives at the time of the great struggle between the Huguenots and the Catholics, and falls under the various influences of the time; he visits the poet Ronsard in his monastery, and the philosopher Montaigne in his château. He falls under the spell of Bruno's pantheism. One fault in the book is that Gaston has too little individuality; he is a mere mirror which reflects a succession of bright and attractive forms; he is too much like Marius in tempera-

ment; and I feel myself little doubt that Pater abandoned the book because he felt that Gaston was lacking in personality, and that he had chosen a time too crowded with influences and types. There is much that is beautiful in the book, but it is lacking in structure and coherence, and even in vitality.

Pater was also slowly accumulating the sections of a book on which he set a high value—*Plato and Platonism.* Much of this had been delivered in the form of lectures, and it is curious that when he was once asked which of his books he thought most likely to live, by which his name would be known, he replied that he had little doubt it would be his *Plato and Platonism.* He intended it to be a useful, an educational book, and I can conceive of no more stimulating volume to put in the hands of a young and eager scholar, upon whom the significance and beauty of Greek life are beginning to dawn; one feels that the writer has penetrated and interpreted the spirit of the time in a wonderful way; but the philosophy is held by some to be unorthodox, and the view of Plato to be coloured by Pater's own personality. Still, the book does undoubtedly present an enchanting picture of the sensitive and ardent soul of that great imaginative writer, Plato, who may be called rather the creator of the romance of thought than the inventor of a philosophical system.

One other little essay of rather earlier date deserves a special mention. This is the *Essay on Style*, which appeared in 1888. It is a deliberate manifesto of his own artistic aims; it is an intricate and subtle piece of writing, but it is worth careful study by anyone who desires to penetrate Pater's theory of art. The essence of the situation is, according to Pater, that a writer should start with a firm conception of the structure of his creation; he must realise that the first condition of interesting others is that he should be himself interested, and then the manipulation of his subject is to be like the engraving of a gem, with firm and delicate strokes, till the last speck of lucent dust is blown away from the subtle curves of the design.

But there is a further matter still. However firm the structure may be, however delicate the workmanship, however patient the labour, there may still be lacking the one essential charm—the charm of personality. It is not enough just to transcribe the object which the artist sees, however faithfully; he must give his own sense of it; and here, Pater would hold, lies the difference between talent and genius—talent may exhibit all the virtues of flawless work, but genius alone, in varying degree, can infuse into the picture that sense of charm, interest, attractiveness—that subtle thing which makes one sometimes feel in looking at a great work of art, that it is significant of far-off wonderful things, that it deals with larger issues, that it opens a door into a world which lies all about us, unseen too often and unsuspected, but which is certainly there, and is full to the brim of a force and a divinity which will not clamorously make itself known, but waits smilingly for us to enter in.

But the end was drawing near. It was strange and beautiful that one who had all his life speculated so wistfully and mournfully upon death, sending his thoughts forward into the glimmering land, should have met it in so desirable a shape.

In the course of 1893 Pater moved back to Oxford, and took a little house in St. Giles' Street, a house of some antiquity, with a quaint character of its own. Here his life moved on with its accustomed quietude. He was not a very robust man, though he had never been seriously ill; in the summer of 1894 he had a bad rheumatic attack, but became convalescent and resumed work. In consequence of writing too near to an open window, he caught a bad chill, but he had apparently recovered his health, when, on leaving his room on the morning of July 30, he died suddenly from failure of the heart. Possibly, if he had realised how much reduced his strength was, and had taken greater care, his life might have been prolonged. He was working in these last days at a lecture on Pascal, which was never completed, a study of peculiar significance. It shows that for Pater the interest of Pascal's life was the melancholy intensity with which he fled to religion of an austere and submissive type, as a shelter against the darkest doubts of the spirit, the agonised questioning as to whether it is really possible for a man to arrive at any knowledge of the nature of God, or man's relation to God at all—to illustrate which Pater uses the beautiful image of the wayfarer who treads a solid road, but hears the ground suddenly give a hollow sound under his feet, as he passes over deep and cavernous places.

My own acquaintance with Pater was of the slightest. But I will endeavour to describe how he appeared to me on the occasion of my first meeting with him a year or two before his death.

He was a strongly built man, with a large head and chin. His skin had a kind of ivory pallor, and he looked like a student, though a closely clipped moustache gave him something of a military air; he walked with a slight limp; he had an extraordinarily kind manner, suave and deferential. He spoke precisely, with a curious dwelling on the syllables of words, which gave a somewhat Italian effect to his pronunciation. He spoke little, and seemed disinclined to express his own opinion, while on the other hand he managed to convey a strong impression of interest and sympathy.

The former tone of brilliance and paradox, that used in early days to characterise his talk, had long deserted him, and those who met him in later years were struck rather by a kind of elaborate courtesy and humility of tone, and an almost deliberate conventionality, which he seemed to use as a shield against intimacy.

This was certainly a superficial trait. His real characteristic was a great independence of mind. There was no one who knew more decidedly what line he intended to follow, or followed it more wholeheartedly. He was not a man of deferential mind, but although he judged people very clear-sightedly, he was intensely anxious to be just

to them. There is hardly a recorded instance of his ever having said a severe or a harsh thing. In his college life, for instance, if some serious violation of discipline was being discussed on the part of an undergraduate, Pater was always on the side of mercy, although naturally disposed to a certain severity of code, both in questions of behaviour and of conduct. He disliked responsibility, and he was greatly irritated by opposition ; indeed he was so well aware of this latter characteristic, and of his tendency to anger, if subjects on which he thought seriously were treated with levity, that he preferred to smile and put a question by, rather than to express his own convictions. One or two instances only are recorded when he spoke with a vehemence of fiery indignation that almost appalled his hearers.

He was a man of a very few friends, but of deep loyalty ; and he clung with profound attachment to the tranquil affections of home life. He had a deep sense of humour, was fond of inventing absurd stories, had a minute eye for foibles of behaviour among his acquaintances, and was even an admirable mimic. He had a great delight in watching the ways of animals, and had a special devotion to cats, whose mystery and delicacy possessed a great fascination for him ; but so far from being a secluded and melancholy dreamer, he had a great taste for the broadly farcical in drama, and would watch a play that amused him, up to the end of his life, with childish ebullition of merriment.

He was a man about whom many amusing anecdotes are told, and it is difficult to distinguish, in some of the stories, at what point his ironical humour came in, and how far his solemnity was assumed. He was supposed to have a great desire to discern the principle of beauty in the most incongruous circumstances. Thus it is related that he once looked over a paper at some college examination, but when it came to deciding the merits of respective candidates, he had put down no marks, and had no impression as to the value of the papers. To assist his memory, the names were read over to him, but at each name he is supposed to have said that it conveyed nothing to him, until the name of Sanctuary occurred, when he visibly brightened, and said that he was now sure that he had looked over the paper, because he remembered that he liked the name. Again it is recorded that when a college meeting was summoned to consider how some obstreperous undergraduates should be dealt with, who had lit a bonfire in the college court on the previous evening, Pater said meditatively that he did not altogether object to bonfires, because they lighted up the spire of St. Mary's so beautifully.

My own belief is that he took a quiet pleasure in mystifying people, and even in acting up to his supposed reputation. His humour is as a rule kept out of his writings, though one occasionally becomes aware, as by some sudden gesture or secret twinkling of an eye, that the writer is not so serious as would appear.

I suppose that there have been few artists who took so much pains

with their work. Pater had a great admiration for the methods of Flaubert, who said that in any question of phrasing, there were several good ways of expressing a point, but only one perfect way, and that it was the artist's business to find it. It was often a very trying business in the case of Flaubert, who would pace about for hours or lie on his couch, searching for a particular word, the racked and tortured medium of his art. But though Pater used often to bewail, half humorously, the trouble that his work cost him, there is no doubt that it was to him a deep and abiding joy, a constant source of refreshment and delight.

His method of composition was peculiar; he used to read and meditate, noting on little slips of paper, many of which are preserved, the points he wished to emphasise. Then he wrote, on alternate lines of ruled paper, a skeleton of his essay or chapter. Then he proceded to amplify this, adding all kinds of curious and beautiful touches, elaborate epithets, minute illustrations. Then the whole was recopied, still on alternate lines, and the same process would be gone through again, and even a third time, if he was not satisfied.

The result of this is the peculiar effect, which is so noticeable in all his work, of a richly embroidered texture. Sometimes, indeed, the structure is apt to become invisible in the superabundance of ornament. The long-drawn-out sentences, with their parentheses, their metaphors, their qualifying clauses, their stately epithets, sometimes lack lucidity; but they have an extraordinary suggestiveness, a dim and haunted grandeur, a delicacy of aroma, which is the work of a consummate artist. He does not produce a brilliant easy effect; he does not aim at just flashing a clear impression upon the mind and passing on; the sentences rather wind and cling like wefts of smoke on a still day, with a subtle and aerial texture, full of hints and glimpses of mysterious beauty.

Let me read a sentence or two from the essay on Leonardo da Vinci, which is perhaps the best example of his finest early work. He is describing the sea-shore of the picture of St. Anne;—"that delicate place, where the wind passes like the hand of some fine etcher over the surface, and the untorn shells are lying thick upon the sand, and the tops of the rocks, to which the waves never rise, are green with grass grown fine as hair. It is the landscape, not of dreams or of fancy, but of places far withdrawn, and hours selected from a thousand with a miracle of *finesse*. Through Leonardo's strange veil of sight things reach him so; in no ordinary night or day, but as in faint light of eclipse, or in some brief interval of falling rain at daybreak, or through deep water."

Or, again in the still more celebrated description of La Gioconda, Leonardo's great picture:—

"The presence that thus rose so strangely beside the waters is expressive of what in the ways of a thousand years men had come to desire. Hers is the head upon which all the ends of the world have

come, and the eyelids are a little weary. It is a beauty wrought out from within upon the flesh, the deposit, little cell by cell, of strange thoughts and fantastic reveries and exquisite passions. Set it for a moment beside one of those white Greek goddesses or beautiful women of antiquity, and how would they be troubled by this beauty into which the soul with all its maladies has passed! . . . She is older than the rocks among which she sits; like the vampire she has been dead many times, and learned the secrets of the grave; and has been a diver in deep seas, and keeps their fallen day about her; and trafficked for strange webs with Eastern merchants; and, as Leda, was the mother of Helen of Troy, and as Saint Anne, the mother of Mary; and all this has been to her but as the sound of lyres and flutes, and lives only in the delicacy with which it has moulded the changing lineaments, and tinged the eyelids and the hands."

Such writing has an undeniable magic about it; it is like a musical fantasia, embodying hints and echoes, touching into life a store of reveries and dreams, opening up strange avenues of dreamful thought.

It may be said that Pater goes too far in his interpretation of these pictures, and that the great artists who made them would have disclaimed the significance with which Pater has charged them; but after all, the pictures are there, and the magical power of art is its quickening spirit; its faculty of touching trains of thought that run far beyond the visible horizon.

Of course, it is possible to dislike such writing for its overpowering sensuousness; it is possible to say that it is touched with decadence, in its dwelling on the beauty of evil, made fair by remoteness; but this is to take an ethical view, to foresee contingencies, to apprehend the ultimate consequences of the appeal. As in all lofty art, the beauty is inexplicable, the charm cannot be analysed; its sincerity, its zest is apparent, and it remains as a typical instance of the prose that is essentially poetical, in its liquid cadences, its echoing rhythms.

And here, I believe, lies the value of the work that Pater was able to do for English. He struck out an absolutely new line in English prose. He was the most original, the least imitative of stylists; he refused to read the works of contagious writers, such as Stevenson and Mr. Kipling, because he knew what he meant to say, and how he meant to say it, and he was afraid that their influence might come in between him and his conception.

The essence of his attempt was to produce prose that had never before been contemplated in English, full of colour and melody, serious, exquisite, ornate. He devoted equal pains, both to construction and ornamentation. Whether he is simple and stately, whether he is involved and intricate, he has contrast always in view. His object was that every sentence should be weighted, charged with music, haunted with echoes; that it should charm and suggest, rather than

convince or state. The danger of the perfection to which he attained is the danger of over-influence of seductive sweetness; the value is to suggest the unexplored possibilities of English, for a kind of prose that is wholly and essentially poetical. The triumph of his art is to be metrical without metre, rhythmical without monotony. There will, of course, always be those whom this honeyed, laboured cadence will affect painfully, with a sense of something stifling and over-perfumed; but the merits of a work of art can never be established by explanation, or defended by argument. The victory rests with those who can apprehend, feel and enjoy; to these comes the pleasure of perfected art, of language that first obeys and then enriches thought, of calculated effect, of realisation, with a supreme felicity, of the intention of the writer.

And then, too, I believe that he did a greater work still. In saying this I am fully alive to the danger of overbalancing the sensuous side of one's nature, of setting too high a value on the thrills and pulsations of beauty, until life may become a mere voluptuous quest for what is delicious; but I do not honestly think that this is a danger to which the English temperament is particularly liable. A far graver danger for most of us is to pursue too steadfastly the material and the social side of life, to busy ourselves with politics, with commerce, with games, with society, with amusement, to live in the surface of things. But behind the great, glittering, pleasant, exciting world, in which we live, lie vast and shadowy tracts of mystery, of beauty, of awe. We know nothing of whence we came, and it doth not yet appear what we shall be. Science traces out for us a strange pageant of developed life, digging the bones of vanished monsters from the rocks, that lie fathoms beneath our feet. Religion casts a glimmering light upon the path we tread —but outside all is darkness. And yet to most of us there come moments when, as the sun sets smouldering behind the dark grove, as the morning dawns over dewy woods, as the flowers break from underground; in the soft rise and fall of musical notes, in the passage of a song, in the glance of an eye, in the touch of a hand, there flashes upon us a ray of that strange and subtle essence which we call beauty, a thing which we cannot define or analyse, but which is certainly there, and which affects us with a certain divine spell. It is hard to resist the thought that this is the very language of God, reminding us, if we will but listen, that there is something sweet and holy all about us, which would tell us what we desire to know, if we were not so dull of heart to understand.

The work of the poet is to interpret this strange quality to us, to show us that all our life is shot through with it, as with the glancing colours that shift and flame upon the neck of the dove.

I think that anyone who will bring this quality before us, who will arrest us in our thoughtless course, who will show us the secrets of art, does for us more than we perhaps know. We cannot, indeed,

live wholly in beauty; there are other insistent voices that will be heard, other hard experiences which we must face, whether we will or no. But the love of beauty can bring a certain simplicity and sincerity into our lives, can lead us beside the waters of comfort, can tranquillise and uplift us.

Let us remember the old parable of the man who raked together the straws and the dust of the street. He was right to do it, if it was his work; but if he could have hearkened, if he could have looked up for a moment from his dreary toil, there was the flashing diadem above his head, and within reach of his forgetful hand.

[A. C. B.]

WEEKLY EVENING MEETING,

Friday, February 2, 1906.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

PROFESSOR SILVANUS P. THOMPSON, D.Sc. F.R.S. *M.R.I.*, Principal and Professor of Physics in the City and Guilds Technical College.

The Electric Production of Nitrates from the Atmosphere.

As the demand of the white races for wheat as a food-stuff increases, the acreage devoted to wheat growing increases, but at a less rapid rate; and being limited by climatic conditions will, in a few years, perhaps less than thirty, be entirely taken up. Then, as Sir William Crookes pointed out in his Presidential Address in 1898, there will be a wheat famine, unless the world's yield per acre (at present about 12·7 bushels per acre on the average) can be raised by use of fertilizers. Of such fertilizers the chief is nitrate of soda, exported from the nitre beds in Chili. The demand for this has risen from 1,000,000 tons in 1892 to 1,543,120 tons in 1905; and the supply will at the present rate be exhausted in less than fifty years. Then the only chance of averting starvation lies, as Crookes pointed out, through the laboratory.

In 1781, Cavendish had observed that nitrogen, which exists in illimitable quantities in the air, can be caused to enter into combination with oxygen, and later he showed that nitrous fumes could be produced by passing electric sparks through air. Although this laboratory experiment had undoubtedly pointed the way, though the chemistry of the arc flame had been investigated in 1880 by Dewar, and though Crookes and Lord Rayleigh had both employed electric discharges to cause nitrogen and oxygen to enter into combination, no commercial process had been found practical for the synthesis of nitrates from the air, until recently.

After referring, in passing, to the tentative processes of Bradley and Lovejoy, of Kowalski, of Naville, and to the cyanamide and cyanide processes, attention was directed to the process of Birkeland and Eyde, of Christiania, for the fixation of atmospheric nitrogen, and their synthetic production of nitrates, by use of a special electric furnace. In this furnace an alternating electric arc was produced at between 3000 and 4000 volts, but under special conditions which resulted from the researches of Professor Birkeland; the arc being formed between the poles of a large electro-magnet, which forced it to take the form of a roaring disc of flame. Such a disc of flame was shown in the lecture room by a model apparatus sent from Christiania.

In the furnaces, as used in Norway, the disc of flame was four or five feet in diameter, and was enclosed in a metal envelope lined with firebrick. Through this furnace air was blown, and emerged charged with nitric oxide fumes. These fumes were collected, allowed time further to oxidize, then absorbed in water-towers or in quicklime—nitric acid and nitrate of lime being the products. The research station near Arendal was described; also the factory at Notodden, in the Hitterdal, where electric power to the extent of 1500 kilowatts was already taken from the Tinnfoss waterfall for the production of nitrate of lime. This product in several forms, including a basic nitrate, was known as Norwegian saltpetre. Experiment had shown that it was equally good as a fertilizer with Chili saltpetre; and the lime in it was of special advantage for certain soils. The yield of product in these furnaces was most satisfactory, and the factory at Notodden—which had been in commercial operation since the spring of 1905—was about to be enlarged; the neighbouring waterfall of Svaelgfos being now in course of utilization would furnish 23,000 horse-power. The Norwegian company had further projects in hand for the utilization of three other waterfalls, including the Rjukanfos, the most considerable fall in Telemarken, which would yield over 200,000 horse-power. According to the statement of Professor Otto Witt, the yield of the Birkeland-Eyde furnaces was over 500 kilogrammes of nitric acid per year for every kilowatt of power. The conditions in Norway were exceptionally good for the furnishing of power at exceedingly low rates. Hence the new product could compete with Chili saltpetre on the market, and would become every year more valuable as the demand for nitrates increased, and the natural supplies became exhausted.

[S. P. T.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1906.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Miss Ruddle Browne,
Granville Landsborough Findlay, Esq., M.B.
Miss Marguerite H. Pam,
Alfred Sutton, Esq.
L. Charles Wallach, Esq.
Miss Isabella Keith Young,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures :—

Hugo Muller, Esq., Ph.D. LL.D. F.R.S.	£100	0	0
Rev. J. H. Ellis, M.A.	25	0	0
J. A. Fleming, Esq., M.A. D.Sc. F.R.S.. .	25	0	0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Board of Trade, Standards Department—Report on Proceedings under the Weights and Measures Acts. 4to. 1905.

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Botanic Society of London, Royal—Quarterly Record, Vol. IX. No. 103. 8vo. 1905.
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WEEKLY EVENING MEETING,

Friday, February 9, 1906.

The Right Hon. Lord Alverstone, G.C.M.G. M.A. LL.D. F.R.S. Vice-President, in the Chair.

Hugh Frank Newall, Esq., M.A. F.R.S.

Eclipse Problems and Observations.

The title of my lecture to-night is "Eclipse Problems and Observations," not "Problems and their Solutions," though we may hope that we have made steps towards solutions. If my remarks seem to be somewhat vague and speculative, I claim your indulgence, for is not wonder the first step to knowledge? Wonder and speculation are very much akin.

The fact that more than eighty expeditions of various nationalities were organised to observe the eclipse of the sun in August 1905, is evidence of very keen interest in the phenomena to be observed during an eclipse. If one searches for reasons for this growing interest, it seems not improbable that it is connected with the discoveries with which physicists have been astounding us in the domain of electricity and radiation. No one who grasps, in even a dim way, the enormous advances in our knowledge of the minute processes involved in electrical discharge and of the by-products of the mechanism of the incandescence of glowing bodies can fail to see what a superb opportunity is afforded by eclipses of the sun to students of physics who are interested in the application of knowledge gained in the laboratory to cosmical phenomena. Who can look at the wondrous beauty of the corona—that delicate radiance which is disclosed round the sun when the opaque body of the moon moves in front of the sun and cuts off the bright light of day from the eyes of the observer, and also from the air and the sky round about—who, I ask, can look at the corona, without the thought that many of the long rays and streamers which seem to emanate from the sun must be stretching out towards the earth, and bringing with them possible influences of which he would fain know the significance? And yet, who can say that we are anywhere near a clear understanding of the mysteries of the corona?

I propose, in the earlier part of my remarks, to do what I may call "sowing seed," by calling your attention to various points in our knowledge of physics, and, towards the end of my lecture, I shall

show you some illustrations of the records got during the last eclipse, and then we can dig up the seeds and see whether any seedlings are recognisable.

First of all, with respect to changes in the corona and the question of the rotation of the corona, we know that in the brief moments of even the longest eclipse of the sun these changes have hitherto been nearly, if not completely, inappreciable; but we know also that the form changes from year to year, and so we are convinced that if sufficiently delicate precautions are taken in getting the records the change should be detected in the lapse of the few hours that pass between the observations which are made at widely separated stations along the track of the shadow of the moon in any given eclipse.

What the nature of the change is, it is rather difficult to surmise. Is it of the nature of a rotation of these streamers about the axis of rotation of the sun; or is it rather a change which might be described as involving the dying out of some of the streamers and the sudden protrusion of others from the sun? Or are we rather to take these streamers as not thrust out from the sun at all? Are they effects which are produced in the matter which the sun finds in his passage through space? It may be that the curved outlines of the chief streamers which we see coming from the sun are envelopes, mathematically speaking, or surfaces formed by the intersection of multitudinous straight rays, seen in perspective. Or it may be that they are the paths of emanations—something thrust out from the sun and passing outward under the influence of various forces of relatively changing magnitude.

We know how meteorites fall on the earth as she moves through space, for we see them leave luminous trails as they rush through our atmosphere. We realise how they may be splintered into small fragments in passing through the air. The sun must meet many more meteorites than does the earth. Again, we know how comets move round the sun, and how they emit gas and vapour as they approach the sun; and we can guess how the scorching influence of the sun, as the comet hurries through its immediate neighbourhood, results in the splitting up of the small stones of which the head of the comet is mainly composed; ruins of dust and vapour are left behind, and it is in the midst of this dust and vapour that the sun moves.

Then, we are taught by Clerk Maxwell's bold imaginings and calculations that where there is light there must be pressure of radiation. A body brought into the light must inevitably experience pressure of radiation, resulting from the falling of the light upon it, and partly connected with the shadow which the body casts behind it, there being light on one side and darkness on the other. The radiation falls on the body only on one side. Lebedew and, again, Nichols and Hull have shown us experimentally that this pressure of radia-

tion does actually exist. Poynting has shown us how to deal with it, and has given us numbers showing how small bodies in the solar system would be affected by the radiation of the sun.

Adopting the figures which Poynting has given us, I have prepared a table showing the temperature attained by small particles put in the neighbourhood of the sun. There has been an enormous advance in the domain of radiation in the last fifteen years. A number of workers have contributed. Those foremost on the experimental side have been Paschen, and Lummer, and Kurlbaum.

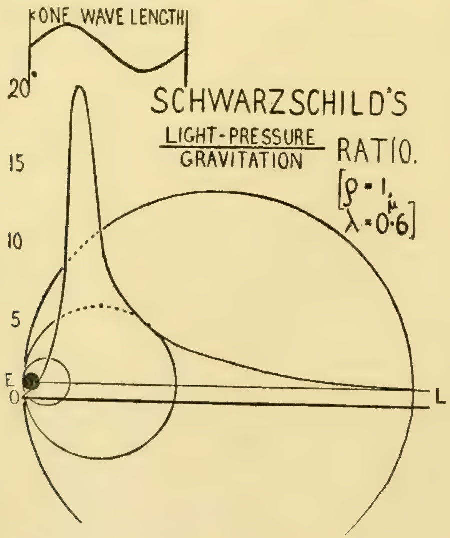
Distance, Millions of Miles.	Absolute Temperature.
	° C.
Sun's Surface	6200
3	1670
4	1440
5	1290
6	1180
7	1090
8	1020
9	960
10	910
93	299

Putting together various numbers determined by them, and by other workers before them, we come to the conclusion that the best number which we can adopt for the temperature of the sun's surface is 6200° C., measured from the absolute zero. Then, for certain distances from the sun's surface measured by millions of miles, the table shows the temperature which would be attained by bodies held at different distances from the sun's surface. At between three and four millions of miles the temperature has fallen to the temperature of molten iron.

Now, observe that the rise of temperature is not very quick at first as we pass from the earth to the sun, but it becomes very quick in the last three million miles, as we pass in towards the surface of the sun. Bodies attracted by the sun from a distance move more and more quickly the nearer they approach the sun. Therefore, we may take it that where the bodies are moving most rapidly, near the sun, there is the greatest change of temperature. If things are moving quickly through rapidly changing temperatures, we shall be sure to have the splintering and vaporising of those bodies by the action of the rapid rise of temperature.

Let us apply to the splinters and vapours the ideas that we have got from the pressure of radiation. Arrhenius, the Swedish savant, has not hesitated to suggest that the repulsion of comets' tails is an instance of repulsion by light. A comet going round the sun shows

a tail which is repelled and held repelled from the sun. That has always been regarded as a mystery, and Arrhenius attempts to explain it by saying that the comet's tail consists of small particles which are repelled under the influence of the light of the sun more than they are attracted by the mass of the sun. Schwarzschild, the professor of astronomy at Göttingen, intervenes, and tells us that the magnitude of the forces involved in explaining this repulsion produced by light must not be calculated for the smaller particles according to the same law as that which holds good for the larger particles. For the larger particles throw clean shadows behind them, while the smaller particles necessarily allow the light waves to creep round by diffraction into the shadow. The smaller particles thus throw only ill-defined shadows, and the radiation has a much smaller repulsive effect under those circumstances than is the case with larger bodies where no such diffractive effect is shown. He calculates what the true conditions are for particles of various sizes, and his results are summarised in this diagram. The ratio of the light pressure to gravitation is represented by the length of the ordinate. For instance, the point at the top of the curve indicates that the light pressure is twenty



times as great as the attraction exerted by the sun in pulling small particles, of diameter equal to about a third of a wave-length of light, towards the surface of the sun. Along the line E L, the light pressure is just equal to the attraction produced by gravitation. Bodies of the size that would be acted upon equally by light pressure and gravitation would be neither repelled nor drawn into the sun.

Lengths measured along the horizontal axis indicate the sizes of the particles. If the particle has a diameter equal to one-tenth of the wave-length of light, then it is attracted by the mass of the sun with a force greater than that exerted by the light of the sun in repelling it. If the particles have diameters about one-third of the length of the wave of light, then those particles are driven away. The particles of diameters equal to one-third of the wave-length will be repelled with a force which is twenty times as great as the force of the sun's attraction. When we get a particle of the size of the wave-length, it is repelled with a force about four times that of the attraction produced by gravity. When we get a particle of diameter equal to about two-and-a-half wave-lengths of light, then it is not more repelled than it is attracted. Particles smaller than that dot on the scale of my diagram are necessarily drawn into the sun ; particles larger than the largest circle on my diagram are also necessarily drawn into the sun. Particles of intermediate sizes are repelled.

Here, then, we have a kind of sorting process under the influence of this pressure of radiation ; but we must not be misled into the idea that in the neighbourhood of the sun we have nothing but these small particles or these big ones, because we must remember that there is a constant supply of meteors, stones, comets, and so on, always attracted by the sun, and always heated up and pulverised under the influence of its temperature. All that we can say is, that the surroundings of the sun are factories of dust and splinters, and that the sweeping process due to the pressure of light is always going on, possibly more energetically from some parts of the sun's surface than from others.

But, even if there were no dust or splinters, and no supply of materials for these splinters, yet still we should have emanations from the sun of a kind which it has only been possible to examine in quite recent years. The work of Elster and Geitel has shown that in the neighbourhood of all incandescent metals there are curious phenomena which, as we now know mainly by the work of that ever active band of workers at the Cavendish Laboratory under the inspiriting leadership of Professor J. J. Thomson, are due to the emission of positive and negative ions from these bodies.

I wish that I could dwell in detail on some of this work, but time will not allow me to do more than say that the evidence conclusively shows that these small corpuscles of Professor Thomson are emitted in large numbers from incandescent solids, especially from glowing carbon and glowing lime, so that Professor Thomson, in one of his most recent books, says that "the fact may have an important application to some cosmical phenomena, since, according to the generally received opinion, the photosphere of the sun contains quantities of glowing carbon. This carbon will emit corpuscles unless the sun by the loss of corpuscles at an earlier stage has acquired such a large charge of positive electricity, that the attraction

of this is sufficient to prevent the negative ions from getting right away from the sun; and yet, even in this case, if the temperature were for any cause to rise above its average value, corpuscles would stream away from the sun into the surrounding space. Such corpuscles going out from incandescent bodies will produce luminosity in gases round about the sun's surface." Thomson and Arrhenius have boldly suggested that the Aurora Borealis is produced by such corpuscles in the outer part of our atmosphere.

It has been also shown that not only are negatively charged corpuscles emitted at very high temperatures, but also positively charged ions at lower temperatures.

One point more I should like to mention, and that relates to the phenomena of magnetism on the surface of the earth. In the last few years the relation between spots on the sun and magnetic storms on the earth has been very much studied, and in one of the latest contributions on the subject—that of Mr. Maunder, of the Royal Observatory, Greenwich—the author has been led to the view that, in the occurrence of notable storms recorded by magnetic instruments, he has found a tendency for storms to recur at intervals equal to that of the rotation of the sun, viz. about 27 days; and he points out that the meaning of this may be that certain regions of the sun are emitting something which, in their recurrent presentation towards the earth, gives rise to recurrent magnetic storms.

The idea is, that from such regions something is emitted which, arriving at the outer confines of the earth's atmosphere, somehow results in magnetic storms. I will not go further into the mechanism by which these storms are produced on earth, or by which we get the enormous quantities of energy which are involved in them, than to say that there is a strong consensus of opinion that the energy which is evidenced in the storm is derived from the energy of rotation of the earth. Something arrives or something happens in the outer atmosphere, and there actions take place which convert some of the energy of the rotation of the earth into the form of magnetic manifestations.

Even if one has to confess to a certain human weakness of will to keep oneself convinced of the reality of this periodic recurrence—and I confess I feel on some days that I am convinced by the evidence, and on other days that I am not convinced—in spite of the material with which Mr. Maunder has supported his views, and in spite of the support which Professor Schuster and Mr. Dyson have given to the discussion of the evidence, there still survives the indubitable fact that the eleven years' cycle of the sun is accompanied by changes in magnetic phenomena in the earth.

Now, among the seeds which I have sown, we have dust and vapour round the sun. We have high temperatures just at the places where high velocities are; we have splintered fragments; we have light pressure; we have corpuscles producing luminosity in the

rarefied vapours; and then we have these magnetic storms on earth possibly arising from some influence coming out from the sun.

I turn now to the observations which have been made during the last eclipse, August 30, 1905, and shall confine myself to those expeditions which were sent out by the Royal Society and the Royal Astronomical Society. Seven expeditions were organised under the auspices of their joint Committee, and seven stations were occupied along the track of the shadow of the moon.

Many of these expeditions were helped by appropriations from the Government grant administered by the Royal Society, and also in a very marked way by the kind assistance of a number of volunteer observers.

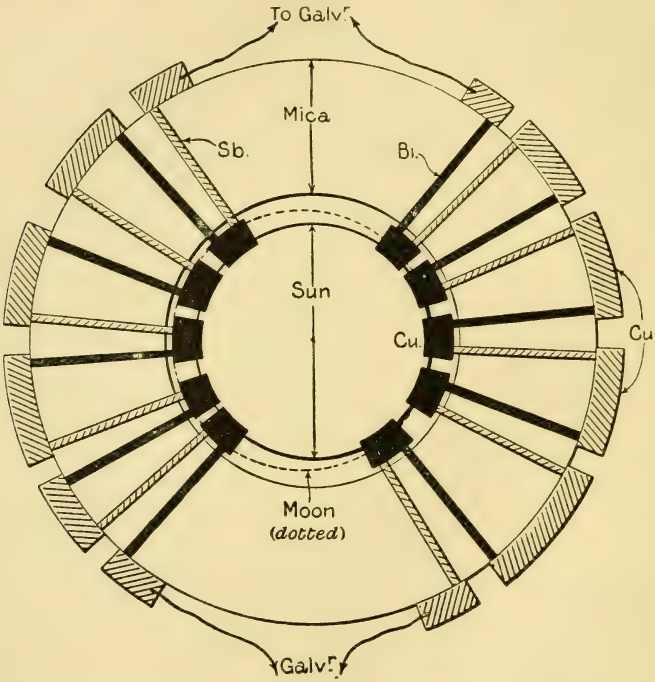
The track of the moon's shadow passed through Labrador, and there Mr. Maunder was stationed, hoping to get observations at the beginning of the track of the eclipse which would be comparable with the observations got at the last station on the eclipse line by Professor Turner, at Assuan. Unfortunately, the weather conditions were bad in Labrador, and the observations were, therefore, frustrated. Then the shadow track passed across the Atlantic, and arrived on the coast of Spain. Two of the parties to which I have referred were stationed in Spain. One, that of Mr. Evershed, was stationed at Pineda de la Sierra, between Burgos and Bilboa; the other, that of Professor Callendar and Professor Fowler, was at Castellón de la Plana. Mr. Evershed was stationed at an altitude of about 4000 feet in the mountains; and Mr. Callendar and Mr. Fowler were stationed near the sea coast. Both parties were very hardly treated by the clouds, for, in spite of the careful preparations which had been made for the purpose of the observations, no observations could be made. It is difficult to convey the disappointment which this means, not only to those who suffered the actual distress of finding all their preparations frustrated, but also to those who hoped that records would be got for study and for comparison with results got in other places.

Sir Norman Lockyer, accompanied by Dr. W. J. S. Lockyer and Mr. F. McClean, and assisted by the officers and crew of H.M.S. *Venus*, were stationed at Palma, in the island of Majorca. Their observations were interrupted by clouds.

On the south of the Mediterranean the weather was much kinder. The party that I was in charge of was stationed at Guelma, in Algeria. We had wonderful weather there, and the observations were successfully carried out under a superbly clear sky. In Tunisia, the Astronomer Royal, assisted by Mr. Dyson, Professor Sampson, and Mr. Atkinson, was stationed at Sfax, on the coast, and there they had fair weather. The last station was in the Soudan, at Assuan, where Professor Turner and Mr. Bellamy, from Oxford University Observatory, were stationed, and had very fine weather conditions.

I should like to show you a slide illustrating the preparations made by Professor Callendar to measure the heat radiation of the

corona. He had a very ingenious form of thermopile, the junctions of which are represented by these black patches, which are in reality small pieces of copper foil forming the junctions of a series of bars of antimony and bismuth. There were ten of these copper pieces, and



they were arranged on a disc of mica round a circle of such a size that the image of the sun should just fall as shown by the full-lined circle. The image of the moon covering the sun is shown by the dotted lines. The two halves of the pile, five junctions in each, were connected in opposite directions in circuit with a galvanometer, the idea being that the moon, as it passed over the sun during totality would cover up one part of the corona and expose another part, so that in the course of the eclipse the effect of any heat radiation from the corona would show itself first by excess on one side, and then by excess on the other, all effects due to the atmosphere being eliminated. The observations were to be reduced to absolute measure by comtarison with other instruments, and he had additional recording phermometrical instruments completing his outfit.

Professor Fowler's work was to be mainly in spectroscopic investigations. He had provided a well designed installation. Those who

remember the photographs which he got in collaboration with Sir Norman Lockyer, in India in 1898, will be able to realise the sort of material which has been lost by the unkindness of the weather.

Sir Norman Lockyer was prevented by cloud from making observations at Palma at the beginning and at the end of totality; but he succeeded in getting some valuable photographs towards the middle of totality. With a powerful prismatic camera the green ring was photographed, showing the distribution in the corona of that unknown element which gives rise to the bright green line in the spectrum of the corona. I am sorry that I am not able to show you an illustration of this, because the result has not yet been published. Other pictures were successfully obtained showing the form of the corona.

I am fortunate in being able to show you slides which have been lent me through the kindness of the Astronomer Royal; and if I show you a large number of the records which our party obtained at Guelma, I hope that I may be excused for seeming to dwell unduly upon them, but I naturally know more about them than about the results got by other observers.

One cannot have better illustrations of the difference between the coronas at the "sun-spot minimum" and at sun-spot maximum than those afforded by the two slides which I now show. This first one was taken at Sumatra in 1901, at the time of sun-spot minimum; observe these extensions to the right and to the left in the equatorial regions; at the top these plumes stick out marking the position of the north pole; the southern region is equally well marked by these plumes below. The second picture was taken at Guelma in 1905 at sun-spot maximum, and shows equality of distribution of coronal extension all round the limb of the sun. Here and there some of the rays project further than others, but there is no sign of polar plumes to indicate where the poles of the sun should be on this picture.

There is a peculiar point in connection with the appearance of coronas of the minimum type. There is, so far as I am aware, no difference in the form of the corona (such as one might connect with perspective foreshortening) whether it is seen when the earth is in the plane of the equator, say in December and June, or whether it is seen when the earth is above or below that plane, say in March and September. The effects of foreshortening of an equatorially extended corona would be appreciably different in the two cases. But, as I have said, I believe no such difference can be found. We are, therefore, driven to imagine that the corona is not so much of the shape of a great millstone, as it were, full of incandescent fog, but is rather of the nature of a sort of cart-wheel with spokes radiating in the equatorial plane from the sun.

Here is a picture, obtained by Dr. Wallace at Guelma, which shows the inner part of the corona with a huge bank of prominences

extending over nearly 30° of solar latitude from the eastern equatorial region towards the sun's north pole, the exposure being such that only the inner parts of the corona are shown. The photograph gives a quite inadequate representation of what one sees *visually* of the corona. The exposure was made so that this inner coronal light might be made to give the greatest photographic contrast in the fine details; in order to get that, the exposure must be short, and the result is that the fainter outlying parts of the corona do not appear at all. If we take a longer exposure, as was done in the next photograph which I now show, we lose the details of the inner part and gain some more of the outer part, but still the impression conveyed is that of a bright corona close to the moon, the black body of the moon just hiding from us the sun and a very small amount of the innermost corona close to the sun. A nearer approach to what is seen visually is given by the picture which I now show, viz. a small-scale photograph that the Astronomer Royal's party were able to get at Sfax; it shows, first of all, the anxiety that the party must have had about clouds, for on it are portrayed the clouds seen in the neighbourhood of the sun; but it also gives one a much better idea of the sort of gradual way in which the rays of the corona stretch out and become gradually invisible as they pass outward. One sees the delicate radiance stretching out in all directions. I was able at Guelma to trace a particular ray visually; it was easy to estimate the extent to which the ray stretched out towards Mercury, for that planet was visible. My estimate of the extension gave between five and six diameters of the moon, that is, about three degrees; whereas the photographs only show an extension of 90 minutes, a degree and a half; but the precautions which would have been necessary to take a photograph of such delicate extensions of streamers were such as would be wanted in photographing the delicate details of cirrus cloud, hence it is not surprising that the photograph shows less than the eye could trace.

Then I have to show you some fine large-scale photographs taken by the Astronomer Royal at Sfax. This one shows the bank of prominences and a great lot of detail in the lower region of the corona. This next one shows detail further away from the edge of the moon. The next slide shows still greater detail, particularly these great streamers coming away in the direction of Mercury, and also showing a *dark* rift or streak which was noticed in the corona on the east side.

Then we have a series of superb pictures on a very much larger scale, showing great detail in the lower corona. One sees the gradual eclipse of the great bank of prominences as the dark body of the moon moves in front of it in successive exposures. You observe those details in the corona—how some of these bright curved lines arch over the northernmost peak of the prominences. Then *here* we see some of these intersecting arches over other prominences. And then if we pass on to the western side of the moon's limb, we find other

prominences. Here is a detached prominence, far above the sun's surface.

But let me call your special attention to the alternating bright and dark arches round a single prominence. They are features in all coronas. They are difficult to explain, but this is perhaps an occasion for seeing if any of our seeds will grow.

Where these arches are formed, that is, at a considerable distance from the surface of the sun, the atmosphere of the sun would be quite inappreciable ; it would be far more rarefied than the earth's atmosphere one hundred miles above the surface of the earth. Yet our splinters assure us that there is matter in the close neighbourhood of the sun. Under the scorching rays vapour must be given off, as rarefied as any comet's tail. Is it a flood or flight of corpuscles, coming from an incandescent prominence and passing through this matter, that gives this luminosity in the form of an arch, which is the perspective appearance of some expanding surface spreading outwards from the prominence, because it takes about the same time for the particles to travel outwards from the prominences ? Such an explanation is not without difficulties. I will revert to it again later.

Meanwhile I show you other eclipse records dealing with work which was carried out at Guelma by our party, and in which I had the good fortune to be assisted by Dr. Wallace, and by Mr. Champion, and Mr. Cooke, and Mr. Wadmore. The first set deals with studies of the polarisation of the corona, with a view of finding out what proportion of the light of the corona is polarised and hence due to light reflected by the dust particles in the neighbourhood of the sun, or possibly to some other unrecognised source of polarisation. These photographs were taken with a camera in front of which certain crystalline plates are put together with a Nicol prism as analyser, forming what I have called a Savart camera. When the light of the blue sky falls on the camera, the polarisation of the light is shown by this banded appearance. When the corona is photographed by means of such a camera, then if the light is polarised, bands should appear over the pictures of the corona taken by the camera. The distribution of the bands in the records obtained at Guelma is somewhat complicated, but it shows that the light is strongly polarised, and that the polarisation is radial.

This research is simply a special continuation of work which has been done in other ways by several eclipse observers of recent years, and serves to give exact determinations of the relative quantities of polarised and unpolarised light in the corona. The result which we get in photographing the corona shows no doubt about the existence and the nature of the polarisation. I show you another picture where we have two photographs side by side, one taken with an ordinary camera and one with the Savart camera. It will be evident that by measurement and comparison of the alternations of bright and faint light along a given line on the two photographs, we

shall get an estimate of the relative quantity of polarisation in the corona.

I have been fortunate enough to get three series of photographs of this kind, two of them taken in 1900 and 1901 at the minimum of sun spots, and the third in 1905 at the maximum. The comparison of these will show whether there is a variation in the amount of light due to polarisation in the corona.

Professor Turner has also carried out experiments of a similar nature with different apparatus and on new lines at Assuan. I wish that I had the material to show you which he gathered in the eclipse; his researches seem to show that something between one-third of the light and half of the light is polarised, and therefore presumably reflected from dust in the corona.

Then I come to another set of our observations at Guelma. With Mr. Cooke's assistance we managed to get four photographs of the corona with an analysing polariscope (a large Nicol prism, which transmitted a two-inch beam) in four different positions—vertical, 45° to the east, 45° to the west, and horizontal. Here I show you the photograph taken with the Nicol prism in the vertical position; it shows the vertical component of the light of the corona. The Nicol prism was placed parallel to this rod, so that no light which was polarised in a plane perpendicular to this rod would leave its effect upon this plate.

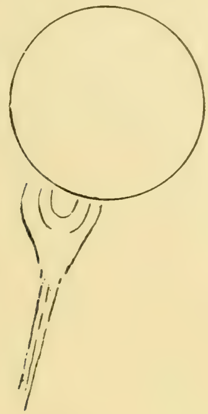
The next photograph shows the result obtained by putting the Nicol with its principal plane at an inclination of 45° to the east.

The next one shows us the picture obtained when the Nicol was inclined at 45° to the west. The pictures all exhibit the greatest extensions of corona in directions parallel to the long diagonal of the Nicol. The polarisation of the corona is seen at a glance to be radial. In a composite slide which I will show you presently, we shall have means of comparing the polarised pictures simultaneously, for we shall see them side by side.

But, before doing that, I will show you the fourth photograph of the series; for it will give you an idea what an eclipse-failure looks like. Each of the four photographs was exposed for 30 seconds; but, unfortunately, during the last few seconds of the exposure of the fourth picture, the sun had reappeared from behind the moon. One sees the corona just indicated round here, and also the black body of the moon. And here is the edge of the brilliant crescent-sun protruding from behind the eclipsing moon. It is so bright that it is reversed, and looks black, except at its edges.

I have called your attention in another photograph to certain bright, curved arches, frequently seen in the corona, as if centred round a prominence, and I have raised the question whether the luminosity of these rings is produced by the bombardment of corpuscles. If they are, somehow, the result of incandescence in the gas, they would not show polarisation. Do these arches behave

differently, under the polarisation tests, from other parts of coronal streamers? For our answer, let us look at the pictures. Here a



streamer is shown, with edges nearly straight and parallel. Near its base you will see certain curved arches, and a little further out the arches are, as it were, burst through by the straight streamer, and here are two remnants, each of which shows a point of inflexion. That is an unpolarised picture. Now let us look at the polarised pictures. These two pictures are polarised at right angles with one another, and you will see the difference between the two. The two inflected arch-remnants are equally strong in both, but the straight streamer is markedly distinct in the one, and almost completely obliterated in the other. The arches are not polarised, the straight streamer is strongly polarised. The arches are probably incandescent gas; the straight streamer is possibly dust being driven outwards.

I must hurry on to another set of observations made at Guelma, with the assistance of Mr. Champion. This shows you the result of photographing the spectrum of the light obtained from the corona. There are two parallel bands of spectrum: the one corresponds with the part of the corona vertically above the moon, and the other with the part of the corona below the moon, whilst the dark interval between them corresponds with the black moon. Such a photograph would be got through an ordinary spectroscope. But here you see we have a couple of such pairs of spectra, for the photograph was got with a spectroscope in which there was a large double image prism in train with the dispersing prism, the result being that one pair of spectra is vertically over the other.

The separated pairs of spectral images are polarised; the one pair is given by the light which is tangentially polarised, and the other by the light which is radially polarised. Observe the difference in the

extent of the polarisation and of the brightness. The radially polarised spectra not only extend both further from the moon's limb, and also further into the ultra-violet, but also are considerably stronger than the tangentially polarised spectra.

I am also able to show a small region of one of the radially polarised spectra on a very large scale, and side by side with it is the corresponding bit of the solar spectrum. They are highly magnified in both cases. In the solar spectrum we see the dark lines of calcium, the H and K lines, and many other Fraunhofer lines. Now, if the radially polarised spectrum is more intense because it contains reflected light, we may expect that the light reflected is similar to the light of the sun. The answer to any doubt on the point is got by looking to see whether we find dark lines in the spectrum of the corona corresponding with the dark lines in the solar spectrum.

The conclusion that I come to from studying the original plates very carefully, is that there are no such dark lines, or that they are far too feeble to account for the excess of the radial component over the tangential. We are thus left with a need to find some other acceptable cause of polarisation in the corona. In opposition to my result is the result obtained by the American astronomer, Mr. Perrine, in the Sumatra eclipse 1901, for his photograph showed the dark lines without any doubt. Perrine's photograph was got with clouds in the sky. Our Guelma records were got without any clouds in the sky. It is rather difficult to say how much stress should be laid on the clearness of the sky.

I should like to complete my illustrations of the records got in the 1905 eclipse by showing one of the valuable photographs obtained by Mr. Dyson, at Sfax, viz. that in which the green corona line comes out with remarkable intensity across patches of continuous spectrum, in which no dark lines are seen. Mr. Dyson notes that in his photograph there are two lines of unknown origin in the corona which have not been observed before—two lines in the green part. It is curious also to contrast the strength with which the green line appears in the equatorial regions photographed by Mr. Dyson, with the comparative feebleness with which it appears in my photographs of the polar regions of the corona.

I hope that I have not called too much upon your indulgence, and that, though time has not allowed me to revert to my seedlings, I have been able to show you that many valuable records have been got during the three and a half minutes of the 1905 eclipse, which will lead to interesting results when we have had time to study and co-ordinate them.

[H. F. N.]

WEEKLY EVENING MEETING,

Friday, February 16, 1906.

The Right Hon. Lord Rayleigh, O.M. M.A. D.C.L. LL.D. Sc.D., President R.S., in the Chair.

W. C. Dampier Whetham, Esq., M.A. F.R.S., Fellow of Trinity College, Cambridge.

The Passage of Electricity through Liquids.

Our subject of this evening owes much of its early development to researches carried on in the Royal Institution. Here Davy investigated the chemical effects of electric currents, and, in 1807, discovered the elements potassium and sodium by the decomposition of the alkalies by the electric current. Here Faraday discovered the quantitative relation between the strength of the electric current on the one hand and the amount of chemical action on the other, and thus raised the subject to the rank of an exact science.

Let us pass an electric current through a solution of some salt, and observe the resultant changes. To make these changes visible, let us choose a coloured salt, such as copper sulphate. As soon as the circuit is completed, we see that one of the copper terminals or electrodes, by which connection is made with the solution, begins to dissolve away, while copper is deposited on the other electrode. Thus copper passes through the solution, disappearing at one end and appearing at the other. The direction in which the copper passes is that which is taken conventionally as the direction of the electric current.

It will be seen that the middle part of the solution is unaffected. The chemical changes occur at the electrodes only; at one copper is deposited, at the other copper is dissolved as sulphate, showing the presence of acid in contact with the metal. The chief facts to be explained then are the appearance of the opposite constituents of the salt—copper and acid—at the electrodes, and the total absence of change in the body of the solution.

We may explain these phenomena by the supposition that oppositely moving streams of the two parts of the salt proceed through the liquid. In the middle there will always be equal quantities of the opposite parts, and the concentration of the solution is unaltered,

but at the ends the parts are set free. The conception may be illustrated by a model, in which differently coloured balls, fixed to movable strings, represent the opposite parts of the salt.

These moving parts of the salt must be electrified, since they move when acted on by an electric force. They were called by Faraday the ions. To a further study of the nature and properties of these ions I ask your attention to-night.

Faraday found that, on passing a steady electric current through a decomposable liquid or electrolyte, the amount of chemical decomposition was proportional to the strength of the electric current, and to the time of current-flow—that is, to the total quantity of electricity which passes. Hence a given quantity of any ion such as copper or chlorine must carry with it a definite charge of electricity. Moreover, the mass of substance deposited by a given current in a given time was found to be proportional to its chemical equivalent weight. Thus equal numbers of equivalents, whether of the same or of different ions, must be associated with equal charges.

If we accept the atomic theory, we must regard the chemical equivalent weight of a substance as proportional to the mass of its atom divided by its valency, i.e. by the number of univalent atoms such as that of hydrogen which one atom of the substance will combine with or displace. Faraday's experiments then mean that each univalent ion carries the same charge of electricity, each divalent ion twice that charge, and so on. The charge on one univalent ion is a true natural unit of electricity, which is thus seen to be quite as atomic in its nature as is matter.

We must now regard the process of electrolysis (i.e. the passage of electricity through a decomposable liquid or electrolyte) as a kind of convection, the electric current being carried through the liquid somewhat as water may be carried from point to point in a number of buckets.

If a current be passed for some time through a solution such as that of copper sulphate, not only is copper dissolved from the anode, or plate by which the current is said conventionally to enter the solution, and deposited at the cathode or plate of exit, but a notable change in concentration is noted in the solution near the two electrodes. The liquid near the anode becomes more concentrated, and that near the cathode more dilute. This may easily be illustrated by an experiment. If instead of copper we use platinum as anode it is not dissolved, and the total amount of copper in solution progressively diminishes. We then find that, while salt is taken from the neighbourhood of both electrodes, more comes from the cathode than from the anode. These phenomena were studied extensively by Hittorf about the years 1850–1860.

Two explanations of this uneven dilution of the solution are possible. We may suppose that the ions are complex structures, and drag unaltered salt or solvent with them through the liquid, or we

may suppose that they move with unequal velocities. It is now probable that in some cases both these factors come into play, but, to simplify our ideas, let us first imagine that the opposite ions are simple, or at all events loaded with equal amounts of salt or solvent, and that they move with unequal speeds through the liquid. The use of the model to which we have already referred enables us to see clearly that the velocity of the anion is to that of the cation as the amount of salt lost by the solution near the cathode is to that near the anode. The ratio of the opposite velocities of simple ions could then be deduced from experiment.

In the year 1879, it was pointed out by F. Kohlrausch that the sum of the opposite ionic velocities might be calculated from a knowledge of the electrical conductivity of the liquid. The conductivity, that is the amount of current conveyed under the influence of a given electromotive force, is obviously proportional to the number of ions, to the velocity with which they move, and to the electric charge carried by each. On the assumption that all the salt is actively concerned in conveying the current, we know the number of gramme equivalents of either ion present from a knowledge of the concentration of the solution. Now Faraday, as we have said, discovered that the amount of substance deposited at the electrodes by a given quantity of electricity was proportional to the chemical equivalent of the substance. This means that a given number of ions, whatever their nature, so long as their chemical valency be the same, carry the same amount of electric charge. The charge on a univalent ion is thus seen to be a true natural unit of electricity; the charge on a divalent ion consists of two such units, that on a trivalent ion, of three. Faraday's quantitative measurements tell us the charge on a gramme-equivalent of any univalent ion. The concentration of our solution tells us the number of gramme-equivalents present; thus, by measuring the conductivity, we can calculate the velocity with which the ions move under a given electric force. By this method, Kohlrausch calculated the specific velocity of many simple ions when moving through dilute aqueous solutions.

In 1886, Sir Oliver Lodge rendered visible the movement of ions which hitherto had been seen by the eye of faith only. By forcing hydrogen ions from a vessel of acid through a tube containing a jelly solution of sodium chloride, he rendered their presence visible by an indicator which changed colour in the presence of an acid, and thus watched their progress through the tube.

In order to compare the ionic velocities as directly observed with those calculated by Kohlrausch, further modifications of the method are necessary. One arrangement which may be used is to employ two solutions which have a common ion, different densities, a nearly equal specific resistance, and different colours. One of these solutions is placed on the top of the other and a current passed across the junction, the movement of which gives us the velocity of the coloured

ions. This method is restricted in scope, but the use of jelly solutions, in which the velocities are not markedly different from those through solutions in water, enables us to use traces of precipitates or indicators to show the movement of various ions. Such experiments showed that the observed velocities were in general agreement with those calculated by Kohlrausch.

If the specific resistances of the two solutions be not equal, interesting phenomena occur at the boundary. The electric force will be greater in the solution where the resistance to be overcome is greater. Hence if an ion from the other solution chance to pass the boundary, it finds itself subjected to a greater force, and its velocity is increased. It will thus be pushed further in advance of the junction if it has got in front, and will be brought up into line again if it has straggled behind. We see, then, that if a junction be advancing in the direction of the solution of higher resistance, the junction will become vague and indistinct, while if the advance be towards the solution of lower resistance the junction will keep sharp and well defined. When the solutions have one ion in common, this means that in order to secure a sharp boundary, we must arrange that a specifically slower ion shall follow a faster one.

Professor Orme Masson recognised that these principles enabled us to dispense with the necessity of choosing two solutions of equal resistance. A salt with quickly-moving ions, such as potassium chloride, is placed in a jelly solution in a horizontal tube. A slow coloured cation is forced electrically into the tube from one end and a slow coloured anion from the other. Thus, blue copper may follow the potassium, and the yellow chromic acid ion CrO_4 may follow the chlorine. The higher specific resistance in the two coloured solutions forces their ions to conform to the movement of the potassium and chlorine, and thus the motion of the boundaries gives us the velocities of the potassium and chlorine in a solution of constant and known concentration, and therefore their velocities under a known electric force.

Further improvements were made by Mr. B. D. Steele and Mr. F. B. Denison. The use of jelly in the tube was dispensed with by placing membranes over the ends of the tube, and removing them when once the junctions had got well within it. The use of coloured solutions also was found to be unnecessary, for, with sharp junctions, the line of demarcation was visible, owing to the slight difference in the refractive indices of the solutions, and may be shown by projection on a lantern-screen. Denison and Steele have made a careful series of experiments by this method, and, in their hands it is probable that the results thus obtained are more accurate than those given by the methods of Hittorf and Kohlrausch.

The general result of these direct measurements of ionic velocity goes to confirm the indirect calculations of the methods of Hittorf and Kohlrausch, in the case, at all events, of simple univalent ions.

Now Kohlrausch finds that the velocity of any one such ion through a dilute aqueous solution is independent of the nature of the other ion present. Thus, for instance, the velocity of chlorine is the same in dilute solutions of potassium chloride as in solutions of sodium chloride. The velocity under a given electric force is a characteristic property of each ion when moving through a dilute aqueous solution. This result suggests that the ions are independently mobile—that they migrate through the liquid independently of each other.

On this view we must suppose that a large proportion of the whole number of molecules of salt present in a solution is composed of dissociated ions—ions, that is, which are not combined with each other, though they may be linked with solvent molecules. The alternative to this supposition seems to be that the motion of the ions is secured by their passage from molecule to molecule at the instants of inter-molecular collision. On this view, the speed with which the ions worked their way through the solution would depend on the frequency with which collisions occurred. The frequency of collision will depend on the square of the concentration; if the number of molecules be doubled, the number of collisions per second will be four times as great. Hence the velocity of the ions should be greater in concentrated solutions, and the conductivity, which depends on the product of the ionic velocity, and the number of ions, should be proportional to the cube of the concentration. But experiment shows that the velocity of the ions is nearly constant with changing concentration in dilute solutions, and slowly diminishes with increasing strength as the solutions become stronger. We are thus driven back to the idea that the ions migrate independently of each other through the liquid. Much non-electrical evidence pointing to the same conclusion has come to light, and has lent support to the theory of electrolytic dissociation.

I do not propose to enter in this place into a discussion of that theory. But I wish to point out that the evidence, electrical and other, in its favour points merely to a dissociation of the opposite ions from each other; it does not involve the idea of charged particles of, say, potassium or chlorine free from all combination. It may well be that the charged atoms, dissociated from each other, are linked, permanently or temporarily, with one or many molecules of the solvent.

Several facts seem indeed to show that some such combination does occur. If the temperature of a solution be varied it is found that the velocity of the ions alters in about the same ratio as the viscosity of the liquid. Now the viscosity gives the friction which the liquid exerts upon a body moving through it—the dimensions of the body being very large compared with the dimensions of the molecular structure of the liquid. There seems no reason to suppose that the resistance suffered by a single atom, struggling through a crowd of other atoms or molecules, would be related intimately to the

ordinary viscosity. In fact, where the structural dimensions of the medium which determine its viscosity are large compared with those of the moving body, it is known that no such relation holds. Thus there is no proportionality between the variation in viscosity of a salt solution when successive quantities of gelatine are added and the variation in the velocity of the ions of the salt. Here the gelatinous structure is probably a sort of fibrous network, very coarse compared with molecular dimensions.

Thus, the approximate proportionality between variation of viscosity with temperature and variation of ionic velocity, indicates that the dimensions of the ions are probably as large as, or larger than, the dimensions of the molecular structure of the solvent. We may perhaps regard the ions as composed of a central charged nucleus, surrounded by a group of solvent molecules. Such a view is supported by Kohlrausch and by Bousfield.

It should be noticed, however, that the solvent molecules cannot remain attached to the charged nucleus throughout its whole journey. The different ionic velocities of potassium, sodium, and lithium, for instance, indicate differences in the amount of the watery ionic envelope. The amount of water transported through a dilute solution of a chloride by these three ions cannot be the same ; it cannot, in each case, be equal to that transported by the chlorine ion. If the water were permanently attached to the nucleus till it reached the electrode, we should get changes in concentration, not contemplated by the theory of Hittorf and Kohlrausch, and the migration constants directly determined by Steele and Denison would not agree with those measured by Hittorf. We must suppose, therefore, that the moving ionic system continually sheds some of its watery envelope, and continually replaces it by fresh water molecules.

One of the most interesting properties of these charged ionic systems is their power of causing the coagulation of certain solutions of colloids, such as albumen. If an electrolyte be added to such a solution in sufficient quantity, coagulation at once ensues, and a curious relation between the coagulative power and the chemical valency of the ionic nucleus enables us to obtain some light on the mechanism of the process. Hardy has shown that colloids themselves generally move through a solution when an electric field is applied, the direction of motion depending on the nature and condition of the liquid solvent. It follows that the colloid particles themselves possess an electric charge, and Hardy finds that the effective ion of the coagulating electrolyte is the ion with an electric charge of sign opposite to that on the colloid. It seems that coagulation is effected by the neutralisation of the charge on the colloid. Now a very much smaller quantity of a divalent salt is able to produce coagulation than is necessary in the case of a univalent salt, and the coagulative power of a trivalent salt is greater again than that of a divalent salt. As mean values, Linder and Picton give for the

coagulative powers of sulphates of univalent, divalent, and trivalent metals the relative numbers 1 : 35 : 1023.

As we saw above, Faraday's experiments show that a univalent ion is associated with one natural unit of electricity, a divalent ion with two such units, and a trivalent ion with three.

Let us suppose that to effect the coagulation of a region of colloid solution, it is necessary for a certain electric charge, equal in amount to that on the colloid particles present and opposite in sign, to be brought within the region. This can only be done by the chance conjunction of ions which, in the absence of an external electric field, must be supposed to be moving in irregular and changeable ways throughout the liquid. If the chance of one ion entering the region be represented by $1/x$, that of two ions entering together will be the product of these separate chances or $1/x^2$, while the chance of the triple event of these conjunctions will be $1/x^3$.

Now, to obtain an equal amount of electricity, we need the presence of 2 trivalent ions, 3 divalent ions, or 6 univalent ions ; and, if we work out the problem for solutions containing the same number of molecules, we find that the relative coagulative powers of univalent, divalent, and trivalent solutions should stand to each other in the general approximate ratio of

$$1 \ : \ p \ : \ p^2$$

The value of the p depends on unknown quantities, such as the effective radius of ionic action, and cannot, at present, be calculated theoretically. But, by putting p equal to 32, it is easy to see that the law of increase of coagulative power with valency is that we have deduced ; for the theoretical numbers

$$1 \ : \ 32 \ : \ 1024$$

agree well with Linder and Picton's mean results.

[W. C. D. W.]

WEEKLY EVENING MEETING,

Friday, February 23, 1906.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Professor John Oliver Arnold, Professor of Metallurgy, Sheffield University.

The Internal Architecture of Metals.

[abstract.]

It had been cynically remarked that to deliver a successful scientific lecture to a cultured audience it was necessary to divide the lecture into three parts. The first part should be understood both by the audience and the lecturer; the second part by the lecturer and not by the audience; and the third part neither by the audience nor the lecturer.

If the foregoing dictum were true, the speaker found himself in a paradoxical position. The object of the discourse was to make the subject under consideration as clear as possible throughout, hence the more nearly this object was achieved, the more unsuccessful the lecture. The title of the discourse might seem to some far-fetched, since, superficially, a bar of polished brass or steel apparently presented the arch type of a homogeneous solid. Any such idea, however, must in a few moments be dispelled. Taking a section of pure gold, or at any rate of gold of a purity of 99·995 per cent., this, when polished and etched, presented under a low power of the microscope large allotrimorphic crystals, the etching figures of which exhibited varying orientation in different crystals. Hence (see Fig. 1) one crystal might appear black, another show the brilliant yellow of gold, and a third exhibit middle tone. All these were purely optical effects. In the black crystal the orientation was at such an angle as to reflect the light entirely outside the objective, whilst, going to the other extreme, the gold-coloured crystal had a molecular orientation which reflected the light entirely into the objective. It was well known that the addition of one or two tenths per cent. to gold of the metal bismuth produced a surprising mass brittleness which naturally led to the enunciation of theories to account for so remarkable a phenomenon.

Twelve years ago the theory which commanded a general acceptance, and at that time reasonably so, was that the small quantity of bismuth was incapable *per se* of producing so profound a mechanical change as to convert one of the most ductile of metals into a mass

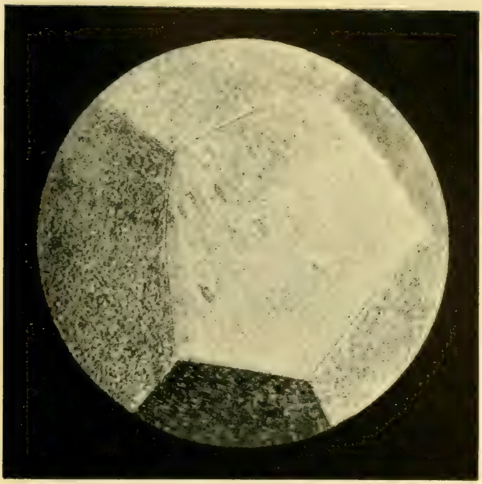


Fig. 1.—Gold.

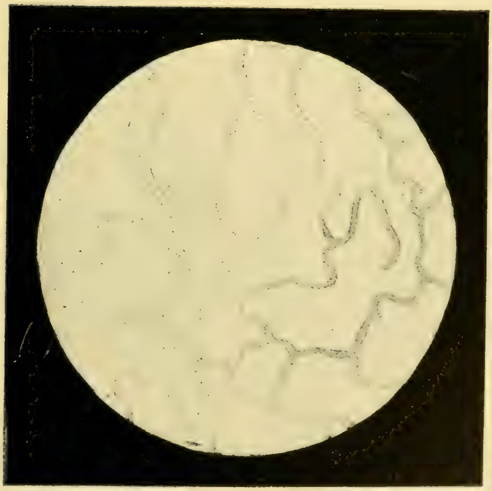


Fig. 2.—Gold containing 0·2 per cent. of Bismuth.

possessing an almost glassy brittleness. Therefore, the metal bismuth must act indirectly, its presence determining the maintenance of the molecules of gold in a brittle allotropic modification.

In 1896 there was published in "Engineering" from the laboratories of the Sheffield College an unambitious research recording the discovery of eutectic cements, which to a considerable extent altered the whole trend of metallurgical thought.

Fig. 2 shows a micro-section of the structure of gold to which 0·2 per cent. of bismuth had been added. The microscope had at once explained the hitherto mysterious action of bismuth. It indicated clearly that the small quantity of bismuth alloyed with a definite amount of gold forming a constituent having a much lower freezing point than the main mass. Hence, when crystallisation set in during solidification from a series of centres, the "eutectic" or constituent last fluid was expelled to the exterior of each crystalline grain of pure gold, thus enveloping each crystal in a membrane of gold-bismuth alloy having a much higher co-efficient of contraction than the crystal itself. Hence, during cooling, the gold-bismuth alloy which may be regarded as the mortar of the structure, to a considerable extent detached itself from the crystalline grains of gold which may be regarded as the stones of which the mass is built up. In the micrograph, Fig. 2, the stones of tough gold are represented as white, whilst the mortar of gold-bismuth eutectic is shown as dark, thick, enveloping membranes. These membranes become pasty well below a red heat, and it was proved that at 400° C. the mass could be powdered in a mortar, the crystalline grains of pure gold becoming detached from the feeble alloy cementing them together. One of these crystalline grains exhibited no signs of the brittleness of the mass from which it was thus detached, but readily beat out into gold leaf in the ordinary manner.

Passing from gold to brass, it was proposed to diverge from the abstract to the concrete, and to show the value of the application of the science of metallurgy to practical problems connected with mysterious failures in marine engineering.

A notable case in point was the explosion of the brazed copper main steam-pipe of the s.s. "Prodano" in calm weather off the Kentish Knock at a pressure about one-tenth of that to which it had been previously tested. In this case the microscope was again successful in clearly indicating the nature of the electrolytic decay under certain conditions of brass used in naval architecture. In this connection, a familiar phenomenon is the decay of Muntz metal bolts exposed to the action of bilge water. Such bolts break suddenly and present a distinctly coppery fracture. A micrographic examination of such bolts usually revealed a minor area of undeteriorated brass and a major area of deteriorated brass—that was to say, brass which had been more or less dezincified, an expression which meant, in other words, that the mass had become transformed into rotten, spongy copper.

Brass often consisted of two constituents, namely a ground mass of true brass of formula Cu_2Zn and a eutectic corresponding to the formula Zn_2Cu. Upon a mass so constituted a feeble saline electrolyte attacked in the first instance the constituent rich in zinc, whilst the constituent rich in copper assumed an electronegative position, acting, of course, as the cathode of the couple.

But, when the eutectic had been transformed into spongy copper, the latter assumed the electro-negative position and the true brass became the anode, hence gradually transforming the whole mass from Muntz metal into spongy copper. In the case of the "Prodano," the electrolyte was proved beyond all doubt to have consisted of fatty acids due to the use of improper lubricants. Little by little the brazed seam was cuprified until the junction became so weakened that at a pressure of only 130 lb. per square inch, the port main steam pipe opened for a space of 6 feet and consigned four men to an agonising death.

This research, made at the Sheffield College under instructions from the Committee of Lloyd's Register, had resulted practically in the abolition of brazed copper main steam-pipes, and the substitution therefor of rolled steel.

Reaching the third section of the lecture, this undoubtedly must be regarded in the steel age as the most important, since it dealt with steel. Taking the base of steel, namely pure iron, this had a similar structure to that of pure gold, but the etching figures exhibiting the molecular orientation in the allotrimorphic crystals of this metal were seldom revealed by ordinary etching.

Broadly speaking, iron was converted into steel by the addition of the element carbon, and researches made in the Sheffield College had indicated that steels naturally divided themselves into three classes, namely, unsaturated, saturated, and supersaturated steels. If 0·3 per cent. of carbon were added to steel, the carbon converted one-third of the iron into the constituent pearlite, and in such a steel, as cast, the iron or ferrite frequently arranged itself into a pattern, indicative of cubic crystallisation exactly comparable with the figures observed by Wiedmanstaten in the non-terrestrial steels called meteorites. In saturated steels, just sufficient carbon approximately 0·9 per cent. had been added to the ferrite to convert it totally into the constituent pearlite, a definite mixture corresponding to the formula $(21Fe + Fe_3C)$. This definite mixture presented at least three well-marked phases having different mechanical properties determined by the state of the division of the carbide Fe_3C. These phases might be differentiated by distinguishing the involved carbide as emulsified, normal and laminated, the latter being the pearly constituent of Sorby, presenting a play of gorgeous colours, determined by the varying thickness of the laminæ acting like mother-of-pearl in nature, or the interference grating in science. Through no scientific foresight, but as a matter of fact by an act of carelessness, there had been secured at the Sheffield College a section showing the trans-

formation of pearlite into hardenite, in the most perfect manner yet recorded. The two constituents, pearlite and hardenite, might humanly be described as the most important in nature, since upon unhardened and hardened steel depended the remarkable triumphs of the civil, the mechanical, and the electrical engineer.

The quartz-hard transformation product of pearlite discovered by the versatile genius of Dr. Sorby, itself presented what might be termed effective and futile phases, dependent upon the temperature of quenching. In properly quenched steel, the accidental section before referred to showed that at a moderate temperature, the transformation proceeded not suddenly, but from a series of converging centres until the whole mass consisted of the obsidian-like substance, structureless hardenite. At too high a temperature, this steely obsidian developed decisive cubic crystallisation, recorded in the micro-structure by equilateral etching figures indicative of ruined steel. In supersaturated steel in the unhardened condition, the cells of pearlite were environed by brilliant walls of cementite Fe_3C, which in hardened steel, enveloped similar cells of hardenite, corresponding to the empirical formula $Fe_{24}C$.

Of the three broad types of steel described, by far the most important was unsaturated steel, a synonymous term for which was structural steel, embracing boiler-plates, ship-plates, bridge-plates, rails, and the gigantic engine parts which formed the backbone of our battleships and cruisers.

To show the enormous importance of the scientific study of this class of steel, it was well to indicate not only its failure, but after brilliant service, also that of the microscope scientifically applied.

The figure thrown upon the screen was that of a boiler, which might be described as several sorts of boiler. It was a marine boiler, a cruiser's boiler, and possibly a mad boiler—it was at any rate, cracked. Fortunately this rupture occurred before the cruiser was put into commission, and a defect in the steel which might have resulted in a catastrophe, was detected by an extra inspection after the boiler had been impressed with the Government pass mark. The chronology of the testing operations, was recorded in the following table :

Date.	Nature of Pressure.	lb. per sq. in.
February 5	Hydraulic	228
,, 8	,,	260
,, 19	,,	305
,, 20	Steam	60
,, 21	Hydraulic	270 (burst)

The mechanical tests of the boiler-plate steel, which had thus failed, left little to be desired, and the same remark applies to static mechanical tests taken along the line of fracture. Micrographic tests indicated that the steel presented marked features of inferiority when compared with undoubtedly good boiler-plate steel. Superficially the matter was thus solved, but, under alternating or dynamic stress tests,

slightly beyond the elastic limit, the steel registered tests varying from 230 to 1292 alternations. The most disconcerting feature in these astoundingly divergent tests was that the test bars registering them were identical in micrographic structure.

At the Cambridge meeting of the British Association, the lecturer suggested that these divergent tests must be associated with opposite sides of the plate subjected to varying heat treatment. The lecturer was quite wrong; and, after twenty-five years experience, had failed to realise the fact that in connection with steel, one must often expect the unexpected.

Remarkable failures in structural steel were commonly associated with the phenomenon called "fatigue." What was "fatigue"? Some little time ago in an important naval trial at the King's Bench, Counsel requested the lecturer to define for My Lord the meaning of this term, which had frequently occurred during the trial, and which he failed to understand. Unfortunately the lecturer also was involved in the outer darkness of My Lord on this matter, but was compelled to give "fatigue" at that time a definition, which remains substantially true today, namely, that he regarded "fatigue" as a generic term used to clearly explain all cases of fracture which were not understood. Before venturing to suggest an explanation for these mysterious fractures for which popular blame often fell upon men who were doing their very best, he would ask his hearers to imagine that that small cloud, no bigger than a man's hand, now hovering over the North Sea, should burst in storm, and that our armour, our guns, and our armour-piercing shells, should be put to the stern implacable test of actual warfare. Supposing our guns were faulty, our shells failed to penetrate the armour of the enemy, our armour was incapable of protecting the gallant inmates of our battleships; assuming this hypothesis, which the lecturer believed to be totally untrue, what would all this mean? It would mean that the internal architecture of British wrought steel was all wrong, and the interesting question thus arose, who were the men responsible for the internal architecture of these metals? The lecturer knew them well. They were grave-eyed men with set mouths, who week after week, month after month, and year after year, lived and moved, and had their being, and sometimes died, amid the flare of gigantic furnaces, and the rattle of Titanic rolls, steadfastly working upon those metals which formed Britain's first line of defence, and to-night on behalf of these inarticulate men, the lecturer confidently asked his distinguished audience to exclaim in their hearts "These men have deserved well of their country."

Reverting to the remarkable and disconcerting fact that two pieces of the faulty boiler-plate steel of identical structure so far as could be seen by the microscope, gave astoundingly different results under dynamic stresses, the lecturer put forward as a tentative hypothesis the theory that, underlying the gross and visible micro-structure of the steel there existed a molecular structure, which in the present

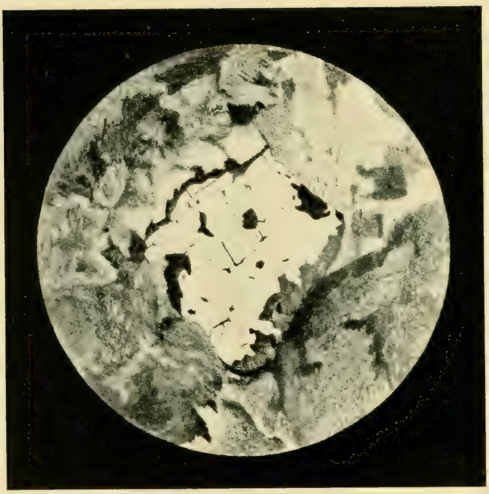


Fig. 3.

state of knowledge, could not be detected, except in rare cases, by the microscope. It was suggested that this molecular structure was brought about by improper heat treatment developing in the ferrite from a series of centres well developed mineral cleavage. On the circumference of these centres existed areas in which the molecular cleavage was less perfectly developed, and beyond these were the areas of good steel in which the cleavage lines were extremely imperfect. It was then easy to conceive that the plane of dynamic fracture in a perfectly developed cleavage area might give the remarkably low record of having endured only 230 alternations as in the table previously exhibited on the screen, whilst a test-piece in which the plane of fracture went through an area of good steel free from what might be called cleavage disease might readily endure 1290 alternations before breaking, and a third test piece from the middle zone of somewhat developed cleavage might endure say 700 alternations. This theory at any rate was in accordance with the mechanical facts which had been presented. Another step towards the experimental verification of this hypothesis would be to prove that iron was a veritable mineral, as capable of exhibiting geometrical cleavage as was, say, fluor-spar or Carrara marble. Fortunately the lecturer found himself in a position by what might be called a million-to-one chance, to clearly prove that iron could possess absolutely perfect mineral cleavage parallel to the faces of the cube. This discovery came in no heroic form from the swift-moving machinery of a destroyer, or in connection with metal forming the stupendous engines of a battle-ship, but in connection with a wrought-iron bolt, literally forming part of a common or garden gate-post. This fractured under the taps of a hand-hammer during repairs, and one of the crystals cleaved at exactly right-angles to the axis of the bolt, and consequently when the fractured end was cut off in the lathe for examination, it was found at right-angles to the axis of the microscope, exhibiting the wonderfully perfect cubic cleavage delineated in Fig. 3.

Metallurgists had now arrived at a deadlock. The microscope after rendering great services, had in its turn broken down, mainly owing to the fact that optical examinations associated with transmitted light, could not be applied to opaque objects, and in more senses than one, the scientific metallurgist could not yet see through steel. Nevertheless he must endeavour to tear down this mysterious veil, or in some way get behind it, and in the lecturer's opinion the resources of science in connection with steel metallurgy were not yet exhausted.

[J. O. A.]

WEEKLY EVENING MEETING,

Friday, March 2, 1906.

The Right Hon. Lord Alverstone, G.C.M.G. M.A. LL.D. F.R.S., Vice-President, in the Chair.

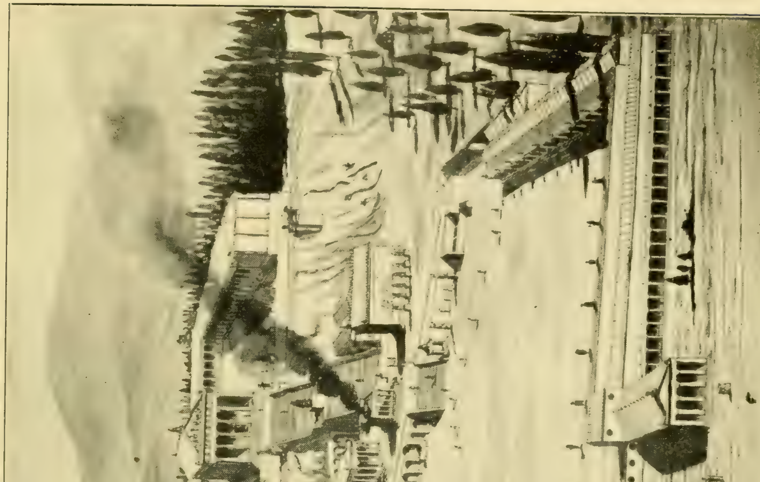
Richard Caton, M.D. F.R.C.P.,
of Liverpool.

Hippocrates and the Newly Discovered Health Temple at Cos.

After exhibiting three portraits of Hippocrates, and giving a brief sketch of his history, his work at Cos, his influence in freeing medicine from ancient superstition, his marvellous powers of observation and scientific insight, and his lofty conception of the unselfish aims which ought to characterise the physician, Dr. Caton showed photographs of Cos, and of the ancient tree under which, according to tradition, Hippocrates was accustomed to give medical advice and counsel to the people in the town of Cos. He then traced the sacred way which leads to the Asklepieion, or Health Temple. Before describing the remains of this great Coan sanctuary, which the labours of Dr. Rudolph Herzog, of Tübingen, have brought to light during the past two years, Dr. Caton briefly recounted the principal temples and sanitary departments, which were comprised in such of the Greek Asklepieia as have thus far been excavated, and especially at Epidaurus.

The great Health Temple of Cos was situated two miles from the sea, at an elevation of about 320 feet, at a point where the range of mountains, which rises on the south coast of Cos to a height of about 2800 feet, springs from the gentle slopes of the plain.

Earthquakes, with disturbance of the soil, the growth of vegetation, the evil deeds of the lime-burner, together with the building of various churches and mosques, had so completely masked the site that, until three years ago, not a living soul knew that the Παναγία τῆς Τάρσου, with the gardens round it, was the site of the famed Asklepieion. Two British archæologists, Mr. W. R. Paton and Mr. E. L. Hicks, while searching Cos for inscriptions, with much acuteness suggested this as a probable site. The situation is a remarkably beautiful one, commanding delightful views on all sides. Standing on three step-like terraces, the buildings at present excavated cover an area of about 180 metres from north to south, and about 129 from east to west. The spectator sees to the south the range of mountains I have mentioned ; to the north the verdant plain of Cos, with the white houses and trees of the town to the



right, and the wide expanse of turquoise sea dotted by the purple islands of the Ægean ; and the dim mountains about Halicarnassus to the north-east.

Of course, little remains of the sacred precinct excepting foundations, but from these, and the architectural fragments which remain, it is not difficult to reconstruct in one's mind the *ensemble* of beautiful buildings which existed 2000 years ago.

The restoration which is annexed gives some idea of the grouping of the temples and stoa in three terraces on ascending levels.

In the foreground is a three-sided stoa, or portico, having irregular buildings at a lower level, adjacent to its outer border, all round. This stoa is approached by a Doric propylæa, or porch. Within this porch there are signs of certain great tanks, or basins, and of an aqueduct supplying them. They were probably for the preliminary ceremonial ablutions. The Asklepiadæ were to be congratulated upon this usage. (Their successors would not be sorry if a preliminary cleansing or lustration of soap and water were required from some of the votaries of the out-patient room in our modern Asklepieia.) The buildings adjacent to the left wing of this great stoa were occupied by an extensive series of baths, mostly reconstructed in Roman times. Here the hot and cold douches of which Hippocrates speaks, the frictions and affusions of water of various temperatures, the inunction of "smegma," a sort of hot, semi-fluid soap, and the applications of sponge and strigil took place. Hippocrates believed greatly in the remedial uses of water, and here, doubtless, hundreds of his patients have submitted to the hydrotherapia of the time. Probably the remainder of these buildings on the north served as waiting-rooms, consulting and operating rooms, the "Iatrium," with its store of instruments, of which Hippocrates speaks, including the "scamnum," or bench for reducing dislocations. Here, probably, would be the dispensary, where were prepared the tisanes, the hellebore, the arsenic, the cantharides, and other drugs he names ; here, also, the library. Here may have been the rooms devoted to teaching, for a most important medical school existed at Cos. Here, I assume, he wrote his careful notes of cases. Most likely Hippocrates kept in these rooms the skeleton which he afterwards gave to the oracle of Delphi.

Here, also, may have been the Deipneterion, or room for meals, and the culinary department, where the special diet to which the Coan school gave so much attention was made ready, where, probably, was prepared the "cygeon," that curious mixture of cheese, honey and wine, which we first read of as being given by Circe to Ulysses, but which, notwithstanding this discouraging origin, may have proved a nourishing form of food for certain of the sick. Here, perhaps, was the winestore, with the Chian and the strong Cretan vintages, which Hippocrates so rarely gave *undiluted.* He was a strong believer in the truth that much strong wine weakened

rather than invigorated. At the south-west corner of the stoa were situated the sanitary arrangements of the precinct. The area enclosed by the stoa was probably a palæstra, where the gymnastic part of the treatment was carried out. We know that Hippocrates was the pupil, not only of his father, Heracleides, the physician, but also of Herodicus, who relied more upon exercise and gymnastics than upon any other treatment. Hippocrates tells us he believed that Herodicus killed some of his patients who had febrile or acute diseases by insisting on too violent exercises. While carefully avoiding this error, Hippocrates prescribed exercise largely in suitable cases. Could we transport ourselves backwards in time to the year 400 before Christ, we might have seen in this palæstra such sights as the gouty man casting the discus, walking or running round and round the stoa, or going through the sword or spear exercise, grumbling meanwhile at his prescribed meagre diet, or the weakly and ill-developed youth running, throwing the javelin, or engaging in gentle wrestling, drinking the "red water," and taking a full and rich diet.

Within the portico were many inscribed wall slabs, some referring to the inviolability of the precinct, others bearing wise maxims in regard to health.

If Dr. Ermerins is correct, many of these existed prior to the time of Hippocrates, and were quoted in his "prorhetics" and "praenotiones." Probably in later times many of the aphorisms and other wise admonitions of Hippocrates were added to the number. We know that at the neighbouring Asklepieion of Cnidus similar precepts termed the "Cnidian sentences," written by the physician Euryphon, were in like manner exposed to view. The records of honours gained by Coan physicians were also conspicuously placed here.

The south side of the quadrangle consisted of a lofty wall with buttresses supporting the second terrace. This wall was interrupted by a flight of steps up to the second terrace, and by several drinking fountains, one in particular, the sacred spring, of which, no doubt, every patient was made to drink freely.

Ascending the stairs to the middle terrace we find ourselves in the most ancient part of the precinct, where the various buildings were arranged with much irregularity. Near the centre of the terrace stands the great altar, a structure measuring 12 metres by 8, approached by steps on its western side. In some respects it reminds the visitor of the great altar of Pergamon, though it is less in dimension. There is difficulty in judging from the remains what its exact details of construction were, but it appears to have been surrounded by a colonnade.

There was a close association between Cos and Alexandria, and I confess to the hope that a shrine of Iemhotep, the Egyptian God of Medicine, would have been found here (as there is reason to believe

it has at Epidaurus), but none has as yet been discovered. Another link, however, connects this altar with Alexandria. Herondas, the Alexandrian poet of the third century B.C., wrote eight "Mimeiambics," comic dramatic poems, the scenes of which are all laid at Cos. The action of the fourth of these takes place in front of this altar. Two Greek ladies, accompanied by their slaves, desire to offer a cock to Asklepios. During the sacrifice they chat with the pyrophorus, or sacristan, who shows much enthusiasm on the subject of fees, and they comment on certain works of art, a sacrificial procession by Apelles, and a figure of a small boy strangling a goose, which are placed near the altar.

Quantities of terra-cotta lamps and figurines were found round this altar.

To the west of the altar stands an Ionic prostyle temple, 16 metres in length and 8 in breadth. Its dedication is unknown. But there is evidence from inscriptions that a temple of Apollo formerly existed here; Apollo being one of the greater gods would have his temple facing the east, and this temple is the only one in the whole precinct which has the correct orientation for so great a deity. The worship of Apollo which perhaps took place here was probably superseded by that of Asklepios in later times. So this temple may have become the Asklepian shrine until the date when the great temple was erected on the upper terrace. It contains one curious feature: a large stone cyst or coffer composed of massive marble blocks forms a portion of the floor of the naos; it is about 5 feet long by 4, and is about 3 feet deep. The side blocks were connected by strong metal clamps. The weighty block forming the lid is pierced in its centre by an aperture some 6 inches broad. Dr. Herzog thinks it is a thesauros, or treasury. But the difficulty of removing the massive cover (which has no rings or handles for such a purpose) would render it almost impossible to deposit or remove such treasure.

It is known that at every Asklepieion the sacred serpents were worshipped as the incarnation of the god. They were tame and harmless, and were free to wander at will throughout the precinct. They probably were supplied with a den or hiding place to which they could retire, and it would obviously be convenient if this den were adjacent to the small altar where sacrifices were offered to them, and where they were fed with the sacrificial cakes, or "popana," by their votaries. At Epidaurus, a curious dark labyrinthine vault was provided beneath the thymele, or sacrificing place. This I believe to have been the ophiseion, or serpent house, from which the serpents emerged close to the altar when the sick came to sacrifice and to feed them. We know from a reference in the *Plutus* of Aristophanes that the serpents were accustomed to be summoned by the sound of a whistle. A somewhat similar structure, on a smaller scale, exists at the Athens Asklepieion. There are, however, other theories as to the

explanation of these buildings, but none can be said as yet to be proven.

To me the above explanation seems the most probable. I think it likely that the temple on the west of the altar, after ceasing to be the special shrine of Asklepios, continued to be the thymele, or sacrificing place, for the sacred serpents, and this coffer in the floor was the ophiseion, or snake house. Above or near it would stand a stone or bronze tripod altar, on which the incense and other bloodless sacrifices would be offered. Here the sick would come, accompanied by a priest, to offer their sacrifice and feed the serpents with popana, or sacrificial cakes. To the north of this temple stood a building divided into rooms, reconstructed in Roman times, which is thought to have been a house for the priests. Passing now to the east of the great altar, we come to a Doric peripteral temple, the most ancient in the precinct. It measures 16 metres in length by 10 in breadth, has six columns at each end, and nine on either side; it faces west. It is known that there were shrines of Hygeia, Aphrodite, Helios, Athena, the Fates, and Hemera in the precinct, perhaps it was dedicated to one of these. To the north of this temple, adjacent to the great flight of steps, a lofty supporting wall was built against the face of the cliff. Between it and the peripteral temple is a large exedra, or semicircular seat, where doubtless the convalescents sat to enjoy the glorious view and the sweet sea and mountain breezes. On the east of the peripteral temple there remains a series of irregular foundations on which stood probably small temples and shelter porches, the latter of which would serve the same purpose as the exedra. No colonnades are found on this second terrace.

Ascending the great flight of steps, and passing on the way a large base which may have supported a colossal statue, we reach the highest terrace, and find facing us the great temple of Asklepios. This is a newer building, dating from the latter years of the third or beginning of the second century before Christ. It is peripteral Doric, and measures 33 metres by 18. Six columns stand at each end and eleven on either side. The foundations are chiefly trachite, but the temple and columns were marble. Each column was $1\frac{1}{4}$ metres in diameter at the base. There remain traces of two groups of statuary. After the fall of the temple (probably in the earthquake of 554 A.D.), a Byzantine chapel was constructed in the remains of the pronaos.

Little is known about the interior. A great figure of Asklepios would stand in the naos, but it is not known what was its material. It was probably marble, as remains of a large marble serpent have been discovered.

Great porticos surrounded the temple on the east, south, and west, the whole structure measuring about 108 metres east and west, and 70 north and south. It seems probable that the east and west wings of the stoa were occupied as abatons, or sleeping places, for the sick, like those at Epidaurus, one for male, the other for female

patients. Here they reposed on their couches for the night (and a few also during the day), hoping for illuminating nocturnal visions from the god, for visits from the sacred serpents, and for miraculous healing. Here the evening prayers were recited to the gods to whom gifts were presented on the tables and altars within the abatons, and all the occupants were encouraged by the priest to hope for succour from Asklepios and Hygeia. The abaton was a lofty colonnade freely open to the mountain breezes, and much resembled the shelter balconies used in our modern sanatoria. The mere exposure to a pure atmosphere was a most potent health giver. It is interesting to note that the idea of incubation close to a temple or church as a means of cure for the sick still exists at Tenos and other of the Greek Islands. (*Vide* the writings of Dr. Rouse.)

The sacred grove of cypresses surrounded the upper and middle terraces.

Higher up in the hills were two remarkable springs. One known as the fountain of Hippocrates may be approached by a short tunnel of Mycenæan architecture, at the end of which is a curious dome-like chamber with seats round its walls and a fountain in the centre. The second is the celebrated "red water," or chalybeate, spring, no doubt of great service in cases of anæmia. Convalescents were encouraged to mount the hill, and drink from one or other of these springs at its source. It is not yet decided whether any of the numerous lines of earthenware piping discovered near the Asklepieion conveyed these waters down to the various fountains and baths in the precinct.

Multitudes of works of art existed here in ancient times, but all have been stolen or destroyed. A celebrated statue of Alexander the Great of bronze is recorded to have had rather rough hair on the head, in the interstices of which there grew a seedling lily. Many small fragments of marbles of great artistic merit have been discovered, and vast numbers of remains of inscriptions, into details of which limitation of space prevents my entering. No theatre, or stadium, existed at the Asklepieion itself, those on the way to the town of Cos being employed for the entertainment of the sick and at the time of the great festivals.

The research thus far has proved extremely interesting, and Dr. Herzog is to be congratulated on his learning and success as an excavator.

With the exception perhaps of the mysteries of Eleusis nothing in the religious life of the Greeks was more solemn or more beautiful than the ritual of Asklepios, and thus it proved the most enduring form of paganism, out-living the worship of Zeus, Hera, Poseidon, or the other deities. It long held out against the efforts of the Christian teacher, and in the end a Christian object of devotion took the place of Asklepios, while the incubation and medical treatment went on as before.

A Health Temple such as this presented a scene attractive from its peace and beauty. In a situation of remarkable charm by reason of the mountains, plain, and sea, the rich vegetation, beautiful flowers, and verdant grove, all that supreme art could offer to please the eye was presented to the visitants of the sanctuary in the form of architecture, painting, and sculpture. The gods of the heathen pantheon were shown in their most attractive guise, suggesting the brightness and hope of human life to the young and to those who were likely to recover ; while to the old and to those whose sickness was incurable the calm and solemn forms of Demeter and Persephone suggested patience and the hope of a pure spiritual after life free from all forms of bodily infirmity.

The priest-physicians were commonly men of education and philosophic training, who taught the skilled culture of life and the need to live simply and according to nature, along with the wisdom of seeking happiness in the love of all that was good and beautiful in nature, art, and literature.

The daily routine of treatment by baths, exercises, the use of medicaments, and regulated diet, amid this pure mountain air, was varied by the solemn religious processions of the white-robed priests and priestesses, with music of flute and cithara, and the singing of pæans and Orphic hymns, by solemn prayers and sacrifices. One of these prayers has come down to us :—

> Oh, ye children of Apollo, who have oft stilled the waves of suffering among men, and lighted the lamp of safety for those who sojourn by sea and land, though your glory be great, accept this prayer, which in sleep and vision ye have inspired. I pray you order it aright, according to your loving kindness for men. Preserve me from sickness, endue my body with such health as may suffice it to obey the soul within, that I may pass the days of my life unhindered and in peace.

Sources of interest were supplied to the visitants to the temple by the performance in the theatre of the tragedies of Sophocles, Euripides, and other poets, or by such comedies as those of Aristophanes. These plays would so immerse the invalid and the convalescent in pathos or in merriment as to banish for the time individual troubles. The studious man would at his pleasure repose in the shelter-seats and dream over manuscripts of history, drama, or poesy, which he borrowed from the library.

A routine of life such as this would tend to a calm and hopeful condition of mind, eminently helpful to recovery from the minor forms of illness.

One cannot but suppose that in this Asklepieion in particular the influence of Hippocrates was great and beneficial. His intense earnestness, his devoted and life-long labours to help the sick and the maimed, to lessen suffering of all kinds, and to learn and to teach new truth must have been priceless.

His influence tended alike to the acquisition of what was new

and valuable, and to the denial and the casting off of all that was useless and superstitious.

While he reverenced the supreme gods, he had more confidence in rest, pure air, exercise, diet, remedies, and on the restorative powers of nature than on the interposition of Asklepios or the influence of the sacred serpents.

In fact, in this building, under the guidance of Hippocrates, medicine probably arose as a helpful instrumentality, based on foundations scientific and practical, and in a nobler form than the world had ever seen, for the relief of the sufferings of mankind.

[R. C.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1906.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S.,
President, in the Chair.

Frank Green, Esq., F.S.A,
Alfred William Oke, Esq., B.A.
Newman Mayo Ogle, Esq.
Henry Fletcher Pooley, Esq.
Henry Taverner, Esq.
Alfred Bramwell Thomas, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

Lords of the Admiralty—Nautical Almanac, 1909. 8vo. 1906.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XV. 1° Semestre, Fasc 1–2. 3vo. 1906.
Classe di Scienze Morali. Vol. XIV. Fasc. 7–8. 8vo. 1905.
Amalgamated Press—Daily Mail Year Book, 1906. 8vo.
American Academy of Arts and Sciences—Proceedings, Vol. XLI. Nos. 14-15. 8vo. 1905.
American Geographical Society—Bulletin, Vol. XXXVIII. No. 1. 8vo. 1906.
Astronomical Society, Royal—Monthly Notices, Vol. LXVI. No. 3. 8vo. 1906.
Automobile Club—Journal for February, 1906.
Bankers, Institute of—Journal, Vol. XXVII. Parts 2–3. 8vo. 1906.
List of Members, 1906.
Belgium, Royal Academy of Sciences—Bulletin, 1905, No. 12. 8vo.
Birmingham and Midland Institute—Meteorological Observations, 1905. 8vo. 1906.
Boston Public Library—Monthly Bulletin for February, 1906. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIII. Nos. 7–8. 4to. 1906.
British Astronomical Association—Journal, Vol. XVI. No. 4. 8vo. 1906.
Memoirs, Vol. XIII. Part 3. 8vo. 1906.
Brooklyn Institute—Science Bulletin, Vol. I. No. 7. 8vo. 1905.
Buenos Ayres—Monthly Bulletin for December, 1905. 4to.
Caracristi, C. F. Z., Esq. (*the Author*)—The Trans-Pecos Sulphur Field. 8vo. 1905.
Carnegie Institute, Washington—Year Book, No. 4, 1905. 8vo. 1906.
Chemical Industry, Society of—Journal, Vol. XXV. Nos. 3-4. 8vo. 1906.
List of Members, 1906. 4to.
Chemical Society—Proceedings, Vol. XXII. Nos. 304–305. 8vo. 1906.
Journal for February, 1906. 8vo.
Editors—American Journal of Science for February, 1906. 8vo.
Analyst for February, 1906. 8vo.
Astrophysical Journal for January, 1906. 8vo.

Editors—continued.
Athenæum for February, 1906. 4to.
Author for March, 1906. 8vo.
Brewers' Journal for February, 1906. 8vo.
Chemical News for February, 1906. 4to.
Chemist and Druggist for February, 1906. 8vo.
Concrete for March, 1906. 8vo.
Dioptric Review for February, 1906. 8vo.
Electrical Engineer for February, 1906. 4to.
Electrical Review for February, 1906. 4to.
Electrical Times for February, 1906. 4to.
Electricity for February, 1906. 8vo.
Engineer for February, 1906. fol.
Engineering for February, 1906. fol.
Homœopathic Review for March, 1906. 8vo.
Horological Journal for March, 1906. 8vo.
Journal of the British Dental Association for February, 1906. 8vo.
Journal of State Medicine for February, 1906. 8vo.
Law Journal for February, 1906. 8vo.
London County Council Gazette for February, 1906. 4to.
London University Gazette for February, 1906. 4to.
Machinery Market for February, 1906. 8vo.
Model Engineer for February, 1906. 8vo.
Motor Car Journal for February, 1906. 8vo.
Musical Times for February, 1906. 8vo.
Nature for February, 1906. 4to.
New Church Magazine for March, 1906. 8vo.
Page's Weekly for February, 1906. 8vo.
Photographic News for February, 1906. 8vo.
Physical Review for February, 1906. 8vo.
Public Health Engineer for February, 1906. 8vo.
Science Abstracts for February, 1906. 8vo.
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Zoophilist for February, 1906. 8vo.
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Grimaldi, Rev. A. B., M.A. (*the Author*)—Catalogue of Zodiacs and Planispheres. 8vo. 1905.
Lehmann, Dr. O. (*the Author*)—Papers on Crystallography, with Micro-Photographs (29). 8vo. 1906.
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Madrid, Real Academia de Ciencias—Revista, Tomo III. Nos. 3–4. 8vo. 1905.
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Anuario, 1906. 16mo.
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Mexico Geological Institute—Parergones, Tom. I. Num. 9. 8vo. 1905.
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Annual Report, 1904–5.
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Ricco, Prof. A.—Memoire della Società degli Spectroscopisti Italiani, Vol. XXXV. Disp. 1. 4to. 1906.
Rio de Janeiro Observatory—Bulletin, January-March, 1905. 8vo.
Royal College of Physicians—List of Fellows, 1906. 8vo.
Royal Engineers, Corps of—Journal for March, 1906. 8vo.
Royal Irish Academy—Proceedings, Vol. XXVI. Section C, No. 1. 8vo. 1906.
Transactions, Vol. XXXIII. Section B, Part 1. 4to. 1906.
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Proceedings, Vol. LXXVII. A, No. 515; B, No. 517. 8vo. 1906.
Report on the Ceylon Pearl Oyster Fisheries, Vols. III.–IV. 4to. 1905.
Mediterranean Fever Commission, Part IV. 8vo. 1906.
Year Book, 1906. 8vo.
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Western Society of Engineers—Journal, Vol. X. No. 6. 8vo. 1905.
Yorkshire Archæological Society—Journal, Vol. XVIII. Part 4. 8vo. 1905.
Zoological Society of London—Transactions, Vol. XVII. Part 5. 4to. 1905.

WEEKLY EVENING MEETING,

Friday, March 9, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Robert Hutchison, M.D. F.R.C.P.

Some Dietetic Problems.

The lecturer began by pointing out the great awakening of interest in Dietetic Problems which had taken place in recent years, and which had exhibited itself on the practical side in the promulgation of various systems of diet, more or less heterodox, for which their advocates claimed many advantages both economic and hygienic. With these systems the lecturer did not propose to deal, but rather to make clear the nature of the scientific problems which lie at the basis of the whole subject, and to which answers must be furnished before any acceptable system of practical dietetics could be formulated. In order to approach the subject in the clearest way some elementary preliminary matters had first to be considered. The two functions of food —(1) as a source of energy, and (2) as replacing waste—were therefore emphasized. Of the constituents of ordinary articles of food the proteids, carbohydrates, fats, water and mineral matters, along with gelatine and alcohol, were alone of nutritive value, and in considering subsequent problems the first five of those need alone be taken into account. Emphasis was laid upon the fact that the proteids, mineral matters and water are the nutritive ingredients which are concerned in replacing waste, whilst the proteids, carbohydrates and fats are the chief energy-yielding constituents of the food. Reference was made to the large or kilo-calorie as the unit of energy employed in dietetics, and the caloric value of some typical foods was exhibited by the aid of a diagram. The lecturer then passed to the first of the problems which had to be considered, viz. how much energy (in calories) must the daily diet contain? It was pointed out that a reply to this question might be arrived at (1) scientifically, by estimating the daily expenditure of energy in different forms by a subject confined in a respiration-calorimeter, or (2) empirically, by calculating the average amount of energy (in calories) contained in the freely chosen diets of a number of individuals doing a moderate amount of work and whose weight was stationary. In this way such a balance sheet as the following could be constructed.

Metabolic Balance Sheet.

Man of 56 kilos.

Expenditure of Energy.		Cals.
1. Internal work (heart, respiration, heat-production) ..		1550
2. Digestive work		240
3. External or muscular work:		
(*a*) Actual work done	250	
(*b*) Increase under (1)	590	
		840
Total		2630
Balance..		425

= 45 grms. fat, or 2 oz. adipose tissue.

Income as Food.	Cals.
118 grms. proteid	484
56 ,, fat	521
500 ,, carbohydrate	2050
Total	3055

Attention being first confined to the expenditure side of the balance sheet, the nature of the outgoings of energy in each item under this heading was briefly explained. It was pointed out that the exact amount of energy expended under each head varied greatly in different circumstances and individuals.

(1) The amount of "internal work" depended upon body-weight, the proportion of fat and muscle, the age, and most of all, upon the extent of body-surface. The latter factor was of the greatest importance as determining heat loss, and variations in the amount of energy consumed by different persons could be largely explained by it. This was illustrated by the fact that if we stated the amount of energy required at different ages in terms of body-surface, the results were surprisingly uniform.

(2) The amount of the digestive work varied with the composition, and especially with the bulk of the food. The influence of bulk was so important that it had been calculated that if a horse were fed upon hay alone, 48 per cent. of the energy in the hay was expended in its digestion.

(3) The influence of external work upon expenditure required little explanation, but stress was laid upon the large increase which even slight degrees of work might entail. Thus 12 per cent. more of energy was expended when standing at attention than when standing at ease, whilst an hour's brisk walk increased the output of energy by 260 cals. Further, one must remember that any increase in external work increased also the expenditure under all the items classed as "internal work" as well. Apart from such variations as these, the question was raised whether there might not be individual variations in metabolism of which we are not yet able to give an exact account. Temperament, for example, was of importance, as determining the performance of superfluous movements, and in the rate at which muscular efforts took place. Apart from this, it was

possible that some individuals might be "more economical machines" than others, producing, for instance, more work and less heat for a given supply of energy. The human body was certainly not a thermo-dynamic machine; it might be thermo-electric, chemico-electric, or chemico-dynamic, or even transform energy in some as yet unknown way, and so long as we were ignorant of its exact mode of working, the possibility of variations in the economy of the machine could not be absolutely denied.

Passing to the income side of the balance sheet, it was shown that this must be determined empirically, by a study of the composition of the diets actually consumed by persons of stationary weight under different conditions. A selection of the results yielded by such study was exhibited with the aid of a diagram, and from them the following "standard diet" had been deduced for a man of average weight, doing a moderate amount of work :—

			Cals.
Proteid	118 grms.	=	484
Fat	56 ,,	=	521
Carbohydrates	500 ,,	=	2050
Total energy			3055

Assuming that the individual whose expenditure was studied in the above balance sheet was put upon such a diet as this he would have a positive balance of 425 calories = 45 grms. fat, or 2 oz. of adipose tissue daily. The question was raised whether this balance was necessarily stored as fat or whether it might not circulate in the blood in some unknown form, and be broken down in abnormal ways giving rise to manifestations of disease. Upon the hypothesis that this could take place Dr. Francis Hare had founded his doctrine of Hyperpyraemia.

Assuming the standard diet above detailed to be that usually consumed, the further question arose could not economy be effected? The lecturer suggested two directions in which this was possible: (1) by a lessened heat production, (2) by a reduction of body weight. It was pointed out that in civilised conditions fat had ceased to be of value as a reserve of food, whilst the mere transport of several pounds of it entailed a considerable expenditure of energy. How much fat it was advisable to harbour in the body was an individual question to which no general reply could be given; it depended upon the "fighting weight" of the individual, i.e. the weight at which his mental and physical efficiency was greatest.

Summing up, the lecturer pointed out that no definite reply could be given to the problem how much energy must be supplied in the daily ration owing to the great variations in expenditure above described. Empirical observation showed that 3000 calories was the average amount taken in, but the trend of scientific opinion was in favour of the view that this was perhaps needlessly liberal.

Turning to the second great problem underlying the science of dietetics—how much proteid is required daily in order to make good the daily waste of the body—the lecturer pointed out that it was impossible to construct a balance sheet for proteid in the body as could be done for energy, for the reason that the organism always tended to get into nitrogenous equilibrium on any quantity of proteid which was supplied up even to the limits of digestive capacity. It was shown by the aid of a diagram that, if an equal number of molecules of proteid, carbohydrate and fat were brought into the neighbourhood of a cell, the cell did not break down equal proportions of each, but that the highest percentage of proteid molecules was destroyed ; next in order of destructibility came carbohydrate, and last fat. This was probably due to the greater instability of the proteid and carbohydrate molecules, an instability which, in the case of sugar at all events, was the result of the presence in the molecule of an aldehyde or ketone group. If, on the other hand, a small number of proteid molecules and a relatively large number of those of carbohydrate and fat were brought simultaneously within reach of the cell, the mass influence of the more numerous molecules asserted itself and, to use a simile, so distracted the attention of the cell that some of the proteid molecules escaped destruction. Hence the term "proteid sparers" as applied to carbohydrates and fats. It was pointed out that the theory of proteid sparers might furnish a reconciliation between apparently opposed systems of diet which yet seemed to give the same therapeutic results. It explained, for instance, how gout, which was presumably due to the presence in the blood of imperfectly oxidised proteid, might be treated successfully either upon a diet containing very little total proteid or upon one in which the proteid sparers were largely excluded (e.g. the "Salisbury" system). Another deduction which might be made from the theory was that it must always be easier to attain nitrogenous equilibrium upon a minimum supply of proteid if the latter was taken along with carbohydrates, so that both nutritive ingredients came under the action of the cells simultaneously.

Thus to take most of the animal part of the diet at one meal and carbonaceous foods at others was wasteful of proteid, whereas a vegetarian diet in which carbohydrates and proteid are so intimately mixed that they reach the tissues at the same time, was that on which it was most easy to attain nitrogenous equilibrium on a relatively low proteid intake.

It followed also from the doctrine of proteid sparers that the amount of proteid necessary for the establishment of nitrogenous equilibrium must vary with the composition of the diet as a whole. It might be supposed that the nitrogenous output of fasting would provide an index of the proteid minimum. This, however, was found by experiment not to hold good, for proteid seemed so to stimulate the vital activity of all the tissues as to increase their power of oxida-

tion; hence if nitrogenous equilibrium is to be attained more proteid must be taken than is the equivalent of the nitrogen output of fasting.

The question for practical dietitians, however, was not what is the proteid minimum, but what is the proteid optimum? This question could only be solved empirically by observation of the amount of proteid actually consumed by persons in nitrogenous equilibrium. From such observations Voit had fixed the proteid optimum at 118 grms. daily. The lecturer then described the experiments of Chittenden, which seemed to show that health could be maintained on a much lower amount than this, even on as little as 60 grms. Did Nature furnish no hint as to the correct standard? It was suggested that in the proportion of proteid in human milk such guidance might be found. Assuming that an infant of six months consumed milk to the value of 578 calories daily, containing 14 grms. of proteid, and that the average energy-value of an adult diet was 3000 calories, it followed that if the adult was to take in the same proportion of his energy in the form of proteid as the child does, the standard for the adult would be about 74 grms. of proteid, it being further assumed that growth in the child might be set off against the greater wear and tear in the adult. The results of this method of attacking the problem were in striking harmony with those arrived at empirically by Chittenden.

The lecturer then touched upon the relative advantages and disadvantages of a low nitrogenous intake, emphasising the possible value of a proteid-rich diet in increasing the powers of resistance to infective disease, especially tuberculosis, and the danger of having no margin of circulating proteid to draw upon in case of emergency.

Summing up the reply to the second problem it was shown that the proteid optimum must vary greatly in different circumstances and individuals, and in accordance with the composition of the diet as a whole, but that it probably stood nearer to the proteid minimum than had hitherto been supposed. The old and new dietary standards were then contrasted as regards both energy and proteid, and in conclusion the bearings of an acceptance of the new standard upon such practical systems of diet as vegetarianism were briefly indicated.

[R. H.]

WEEKLY EVENING MEETING,

Friday, March 16, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

W. Duddell, Esq.

How to Improve Telephony.

In my discourse to-night I propose to strictly limit the word "telephony" to the art by means of which sounds and speech are electrically transmitted to a distance. The loudness, and articulation or clearness of the transmitted speech, and the distance over which it can be transmitted, are the main directions in which improvement is required. The questions, how Mr. A. in London shall get connected to Mr. B. in Glasgow with the minimum of loss of time and temper, and how to find the number of the person you wish to communicate with, when his entry in the directory, in small type, is sandwiched in between two advertisements, are questions with which I do not propose to deal—not that there is any lack of room for improvement in these directions.

Before proceeding with our subject, it will be necessary to consider briefly what constitutes sound, and more especially articulate speech, so as to form a clear idea of what we want to transmit. The sensation of sound, as is well known, is produced by the vibration backwards and forwards of the particles of the air about their position of rest, and the character of the sound depends on the quickness and the form of the vibrations. Thus, in the case of a musical note, the air particles vibrate in a perfectly regular manner, and the number of complete vibrations in a second, or the frequency determines the pitch; and the amplitude, or distance the air particle moves from its position of rest, determines the loudness of the note. In speech, however, the vibrations are very complex, and in order to form any clear mental idea of their character, it is necessary to represent the movements as curves, which I will call sound patterns. Various observers have made records of these patterns; among the earliest and best, are those obtained by Fleeming Jenkin, and Ewing (Edinburgh Phil. Trans.), who magnified the impression obtained on the cylinder of a tin-foil phonograph. Figs. 1 to 6 are some typical sound patterns obtained by another method, to be explained later, which illus-

FIG. 1.—Vowel ā in Ma.

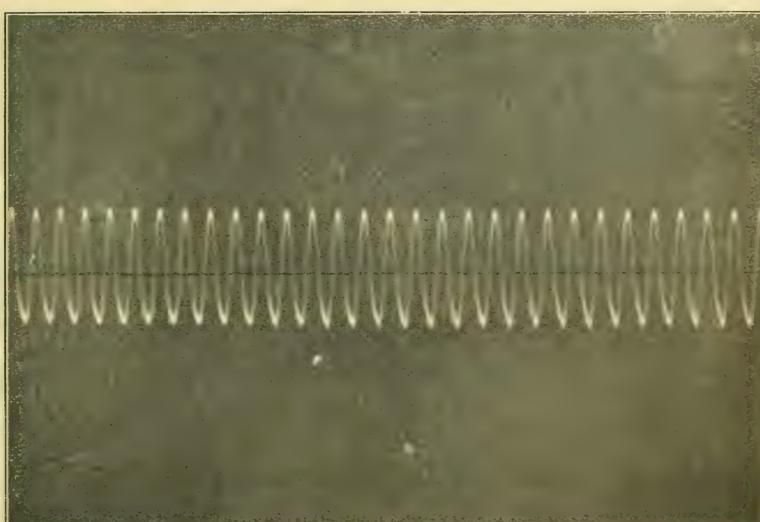

FIG. 2.—Simple form of ōō sound in Coo.

trate how very complex the movements of the air particles become in the case of speech.

The problem in telephony is the accurate reproduction at a distance of these complex vibrations of the air : the more nearly the movements of the air at the receiving point correspond to those at the transmitting point, the better will be the quality of the telephony. If the movements at the receiving station are similar in pattern but of less amplitude than those at the transmitting station, then we have simply attenuation of the sound ; if, however, the sound pattern at the receiving point is distorted, then loss in articulation takes place. It happens, very luckily, that the ear has a wonderful power of recognising a sound pattern even when considerably distorted ; if it were not for this latitude, as we may call it, in transmitting our sound pattern, telephony would not be practicable over anything like the distance already attained.

Let us now come back to electric telephony, and examine what takes place between the original air movements and their final reproduction at a distance. The movements of the air are first converted into movements of a diaphragm, which movements are again mechanically transmitted to the carbon grains in the microphone, thus altering its electrical resistance. The varying resistance of the microphone causes the current through it and the connected transformer to vary and so induces varying currents in the secondary of the transformer. This secondary is connected to the line, so that the currents are conveyed to the receiver at the distant place. The currents are here transformed into a varying magnetic field which acts on a diaphragm and causes it to vibrate and thus start the air around it in movement. In all this long train of transmissions and transformations the character of the original sound pattern must be preserved sufficiently well to enable the ear to recognise it in its final form. When we consider that at every one of these steps distortion and loss of energy must take place, it is not surprising that there are difficulties in the way of telephony. In fact, it is a matter for wonder that electric telephony is possible at all.

Telephony has already reached a very high degree of excellence, how can it be improved ? At every step in the long train of transformations we must inquire : What are the losses ? What are the distortions introduced ? How can we avoid them ? Answers to these questions can only be given by systematic accurate measurements and experiments.

If one consults the literature on telephony, one is surprised how little quantitative data is available on any given point in comparison with that in other branches of engineering. Experimenters of late seem to have avoided telephony ; dozens of investigations are published on the efficiency of induction motors, and hardly one on the efficiency of telephone induction coils ; yet while the former are made in thousands, the latter are made in millions. Telephone engineers

are not wholly to blame for this state of affairs: the difficulties of making the measurements and the lack of suitable apparatus have largely contributed to it. In what follows, I propose to draw attention to some existing apparatus and methods which can be applied to this purpose.

An investigation has been recently made by Professor P. E. Shaw* on the amplitude of the movement of a telephone diaphragm by means of an extremely sensitive micrometer which he has devised. I will cite one result as showing how extremely small are the quantities with which we have to deal in telephony. He finds that the movement of the diaphragm corresponding to a just comfortably loud impulsive sound is only one twenty-thousandth part of a millimetre, and that something less than one-fiftieth of this is still just audible.

The diaphragm has a frequency of vibration of its own which in ordinary receivers may be about 500 complete vibrations per second; it will therefore tend to reinforce (due to resonance) notes having the same frequency as itself, i.e. about the octave above middle C. This leads to the very unpleasant accentuating of certain notes, when music is transmitted telephonically. It seems as if this might be overcome by applying some form of damping to the diaphragm or by making its frequency of vibration very much higher.

To test the electrical part of the apparatus, we require some means of measuring small alternating currents of fairly high frequency and also some method of producing these currents. At first sight it would seem comparatively easy to construct an alternator to produce these currents, as the highest frequency does not exceed about 2000 periods per second, and alternators have been constructed to give very much higher frequencies.† The real difficulty is to obtain a machine which will give a strictly sinusoidal current under all conditions. This is necessary to enable the experimental results to be easily compared with theory.

There are other methods of producing high-frequency currents, such as: (1) the Humming Telephone ‡; (2) the Musical Arc §; (3) the Musical Vacuum Tube, which is produced by shunting a vacuum tube, supplied with high voltage *direct* current, with a condenser and self-induction in series in a similar way to the musical arc; (4) the vibrating bar of Mr. Campbell ‖ which works in a manner analogous to the electrically maintained tuning fork, except that the contact is replaced by a small microphone. This latter apparatus gives a very constant frequency and current.

Although electromagnetic instruments such as dynamometers have been constructed sufficiently sensitive to measure telephonic currents,

* Proc. Roy. Soc., lxxvi. pp. 350–366.
† Proc. Phys. Soc., xix., p. 299, 1905; also Phil. Mag.
‡ F. Gill, Journ. Inst. Elec. Eng., xxxi. p. 388, 1902.
§ Journ. Inst. Elec. Eng., xxx., 1901; and Proc. Roy. Inst., Feb. 1902.
‖ Proc. Phys. Soc., xix., p. 171, 1904.

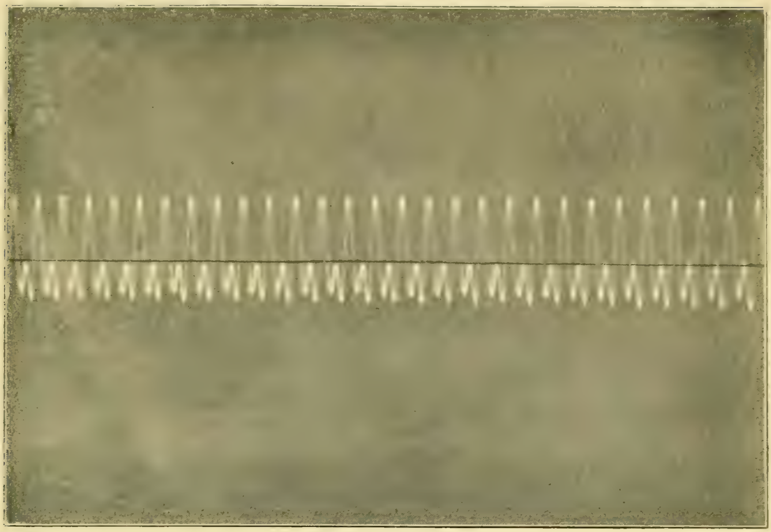

FIG. 3.—Complex form of ōō sound in Coo.

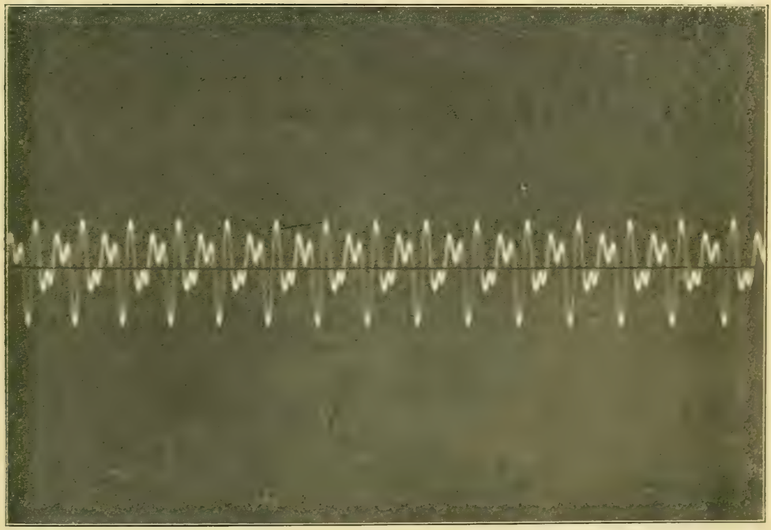

FIG. 4.—Vowel ō in Ho.

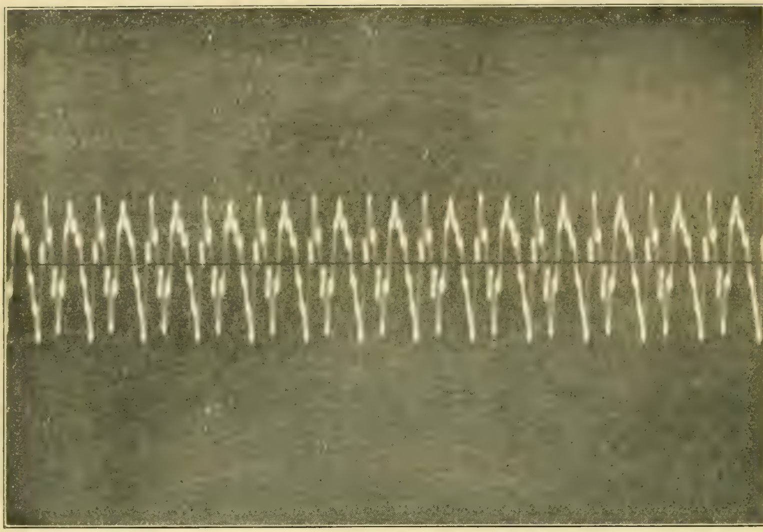

FIG. 5.—Vowel ē in Me.

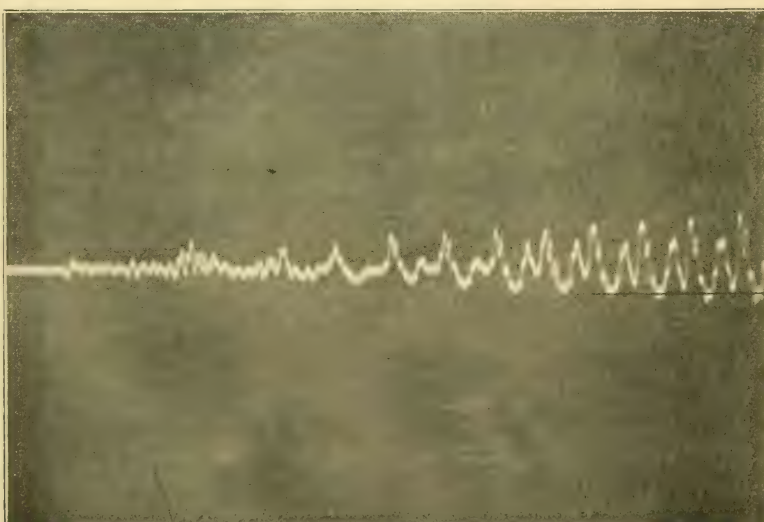

FIG. 6.—K and first part of e in Key.

the relatively high self-induction of these instruments has prevented their general application. Practically all the instruments which are at present being applied to the measure of high-frequency currents are thermal instruments, that is to say, they depend for their action on the heating produced by the current when it flows through a suitable small high-resistance conductor. These instruments may be broadly divided into three classes, according to whether the rise in temperature of the conductor and consequently the current is measured by (1) the expansion of the conductor ; (2) the change in its resistance ; (3) the E.M.F. of a thermocouple either forming part of or near to the heated conductor.*

The first method, viz., the use of the expansion of the conductor as a measure of the current, has not up to the present lent itself to the production of very sensitive instruments. The second and third methods above have each given instruments of high sensibility such as the "barretter" employed by Dr. Kennelly,† and the thermo-galvanometer.‡ From the point of view of ultimate sensibility there is very little choice between these two instruments, but the simplicity and ease of standardisation of the thermo-galvanometer make it the more convenient in practice.

Some very interesting results have been obtained by Dr. H. V. Hayes § on the attenuation of the current through cables and long overhead lines, and on the improvement that can be obtained by adding self-induction to the line. These experimental results amply bear out the theoretical conclusions of Heaviside as to the great advantage of increasing the self-induction, or "loading" the line for long-distance transmission. The great importance of avoiding reflection of the current at the terminal apparatus, and the means of reducing it by the use of a "terminal taper" is also very clearly shown. It is greatly to be hoped that these investigations will be actively pursued, and a satisfactory design of loading coil will be developed, as increasing the self-induction of the circuit gives great promise of successfully increasing the length (now limited to about 50 miles) of subterranean or submarine cable through which telephony can be commercially accomplished.

So far the methods of measurement dealt with only give the root-mean-squared or heating value of the current. To investigate the distortion in the sound pattern when translated into a varying electric current as it flows along the line and through the different pieces of apparatus, we require to be able to record the current at every instant and also at two or more points in the circuit. This can be easily accomplished by means of an oscillograph, and the sound patterns given at the commencement of this discourse were thus recorded.

* See further, L'Électricien, xxxi., p. 145, 1906.

† Internat. Elec. Congress, St. Louis, 1904.

‡ Internat. Elec. Congress, St. Louis, 1904.

§ Proc. Phys. Soc., xix., p. 91 ; and also Phil. Mag., 1904.

Mr. A. Blondel, and the Engineer-in-Chief of the Post Office, Mr. Gavey,* have published many results obtained in this way.

A more complete insight into the distortion produced by different parts of the apparatus and line can be obtained by gearing small mirrors to both the transmitter and receiver diaphragms, so that records can be obtained simultaneously of the movement of the transmitter diaphragm, the current flowing into the line or cable, the current flowing out of the line, and the movement of the receiver diaphragm. [With the above apparatus the effects produced by resistance, capacity, and self-induction, both separately and in combination, and also distributed, in the form of an artificial cable, were demonstrated at the discourse.]

There still remains much work to be done in devising new methods of measurement, and in improving the present apparatus: nevertheless the existing methods and apparatus are already sufficiently perfect to enable a large number of investigations to be successfully undertaken.

If the necessarily brief references to the complex problems of telephony and the résumé of the methods of measurement available to attack them which I have given to-night should inspire any of our engineers or scientists to undertake systematic quantitative measurements with a view to improving the transmission of sounds and speech, then this discourse will have accomplished its aim, and, I think, justified its title—"How to Improve Telephony."

[W. D.]

* Journ. Inst. Elec. Eng., xxxvi., p. 32, 1905.

WEEKLY EVENING MEETING,

Friday, March 23, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

The Right Hon. Field-Marshal Earl Roberts, V.C. K.G. G.C.B. O.M. G.C.S.I. G.C.I.E. D.C.L. LL.D.

Imperial Defence.

I need not dwell upon the assurance that it is a source of the greatest satisfaction to me to be permitted to address this audience, amid surroundings associated with the best intellectual traditions of the country, upon a subject which I believe to be, if rightly understood, the most important and the most urgent which can be placed before the thoughtful consideration of patriotic men. That subject, as you are aware, is a matter which has been recognised at all times in history as the first and most momentous duty to which statesmanship can ever address itself—I mean the subject of National Security; and, with the length of service that now lies behind me, and in the circumstances under which I have worn the uniform of the Sovereign, you will readily believe that nothing but the strictest sense of duty could have induced me to devote to an arduous, and it may be prolonged, crusade whatever of life and strength under Providence may still remain to me.

There is a special fitness in our assembling here together, under the roof of the Royal Institution, to recognise at the outset that the proper place of a sound military organisation, in the whole scheme of National Defence is a subject which is no longer regarded as a merely professional or ornamental matter, but is henceforth a serious and necessary part of our national interests as a whole.

National efficiency depends upon dealing with all these subjects in a proper relation to each other. We must realise that the whole of our national life hangs together, and that the question of real preparation for the stern emergencies of war is one which affects every aspect of the ordinary existence of the country. All scientific progress influences the machinery and the spirit of war, and every great war exercises in its turn a far-reaching effect upon human thought and upon the conditions of commercial enterprise throughout the world.

We now have a Secretary of State for War who has dwelt during

the last few weeks with so much force and eloquence upon the fact that efficient and sufficient preparation for war means nothing more nor less than the application of all that is best in the mind and intelligence of a country to the business of National Defence. More will depend in the long run upon the amount of thought you devote to your Army and your Reserve than upon the amount of money you spend upon them. We have before us on the Continent of Europe, the example of a nation of thinkers whose strength and prosperity are the creation of thought, and there are few more convincing and suggestive utterances in history than the remark of King Frederick William III. of Prussia in 1809, at the opening of the Berlin University after an era of military misfortunes—and just three years after the Battle of Jena—"We must regain in intellect what we have lost in territory." When King Frederick William made that remark Prussia was then in her darkest hour, dismembered as Napoleon had left her. But Wilhelm von Humboldt had suggested the Berlin University as one means of regenerating the nation, and Scharnhorst had just hit upon the idea of building up a great National Reserve by passing recruits rapidly through an apparently small Army.

It has become evident to me that, if the main elements of this problem are to be grasped by the nation at large, we must distinguish radically and effectually between the two branches of the subject which are commonly confused in argument and have too often been confused in practical treatment. The proposition to which I invite your attention is elementary, but its importance cannot possibly be over-estimated. My proposition is that in this country, and in this country alone, there is not only *one* Army question to be dealt with but *two* Army questions. Consideration has more and more convinced me—as I hope to convince this audience—that our chief difficulties have arisen in the past, and may arise in the future, from the people of this country being unable to distinguish between these two questions; an inability quite natural to an insular and naval nation, which up to the outbreak of the South African War, had enjoyed nearly a century of practically continuous peace.

The first question is that of the Regular Army; the second question is that of the Reserve. Now, what of the Regular Army? It is a small professional force, recruited upon a basis of long service at a high rate of pay, and devoid, by reason of its constitution—and mark me, necessarily, inherently, and permanently devoid—of that power of large expansion, when a crisis arrives, upon which every Continental Army depends for its success in war.

Continental armies, in ordinary times, are not what our Army is, and not what they are ordinarily supposed to be. They are entirely different. They depend, when called to arms, not so much upon the comparatively small numbers of men who are at any given period with the colours, but upon the vast numbers who have passed through the ranks. The Continental armies, as they stand at any moment

before the order for national mobilisation is given—and we pray that it may be long before that dread signal is made which summons millions of men from the paths of civil life, and plunges whole nations into conflict—the Continental armies, I repeat, are at ordinary times not fighting machines, but training machines. Short service upon the system which we call "conscription," but which the Germans call by a strong and homely word of their own meaning "defence duty," is designed to pass as many citizens as possible through the ranks, so that their country may count upon the largest number of her sons in the hour of need, and to return them as rapidly as possible to the pursuits of civil life in which the vast majority of them live and die in peace, just as the vast majority of our citizens live and die in our own favoured island. If there were fewer of these trained men, the risks of their being called upon for war would, I venture to think, be much greater, much more frequent. The stupendous dislocation of civil life by setting in motion these enormous masses of men is the strongest safeguard of the peace of the world to-day. These conditions make wars terrible when they come, but they make wars rarer, and prolong the intervals of peace to an extent that no other conditions in the modern world have ever secured.

For every other European nation but ourselves, the problem of military organisation is not a double problem. Continental armies create their own reserves by a perfectly continuous and natural process. The very object of the Continental short-service system, in time of peace, is to create the great reserve upon which the country will depend in time of war. There is no possibility of that confusion of practice and idea which, owing to our unique circumstances, has perplexed and thwarted the efforts of generations of Army reformers, and disappointed the hopes and expectations of the country.

In this country alone we possess an Army by means of which it is impossible to create an adequate reserve. I leave the United States of America out of my calculations, as the conditions of her existence are altogether different, and she is not burdened with Imperial responsibilities such as ours. I shall put the question in an even simpler form, if I say that the British Regular Army is the only Army which does not create, in any full sense, its own Reserve. This is a necessary consequence of the fact that it is based on a voluntary long-service system, and that the greater part of it is always on service in various quarters of the Empire. It is not mainly a training organisation. It is a separate and complete machine in itself which exists as a garrison of the Empire, to keep watch and ward over our frontiers, and to maintain the *pax Britannica*. It is always on duty as I have said, and is, at all times, in spite of its comparatively small scale, an actual working Army, fulfilling important and indispensable duties in time of peace. It does not, therefore, carry out the ordinary purpose of a training Army as the Continental armies do.

My object to-night is not to show how an adequate power of expan-

sion for the emergencies of war may be secured in this country, without resorting to any methods which can justly be compared to the Continental system of universal and prolonged barrack-training. That is a question to which I hope to address myself upon an early occasion. The first duty of Army reformers, as we have already seen, is to clear the ground of the confusion which has been hitherto the greatest obstacle to progress. What you will permit me perhaps to show, upon the present occasion, is that denunciation of our military inefficiency and unreadiness during the last few years has been, to a very large extent, upon the wrong tack. We have busied ourselves with the problem of the Army when we ought rather to have devoted a special and adequate share of our attention to the problem of the Reserve which I have ventured to describe as forming, for this country, a separate but supremely important part—in some respects the most important part—of our double military question. No country expects to engage successfully in a serious struggle without using its reserves, and we are the only nation which possesses nothing in the shape of an efficient and sufficient Reserve, notwithstanding our many Imperial responsibilities, our distant and extensive frontiers to be defended, and our vital political and commercial interests in every quarter of the world.

If we are ever to have the power of expansion in emergency, upon which the war readiness of every other nation depends, we must set up some entirely different system from what any other nation possesses, apart from our Regular Professional Army.

I am now brought to the further and main point of my argument to-night—that criticism of the Regular Forces and their management has been in the past very largely misdirected. It has been excessive and undeserved. Criticism upon the score of efficiency has been too generally separated from the question of sufficiency. The best tailor must cut his coat according to his cloth. No one would expect a Fleet to be successful if the number of battleships it contained were admittedly too few to enable it to grapple adequately with its task. A small Army, in the same way, no matter how highly trained, no matter how well equipped according to its scale, cannot do the work of a large Army. Our position is that our Regular Forces must always remain, in the nature of things, too limited in numbers to be capable of bringing any serious struggle on land to a successful conclusion by their own unaided resources. When the problems of pay, recruiting, training, and organisation have been dealt with as satisfactorily as the most exacting critic can conceive, the problem of numbers will remain.

What I desire to insist upon, therefore, is that the most perfect Regular Army conceivable will not place this country in a state of preparedness for war, or remove the danger of dislocation, expense, and reverses recurring in some future crisis, under conditions, perhaps as unexpected as were the conditions of six years ago in South Africa.

In spite of all the lessons we then received we are not secure, nor by the reorganisation of the Regular Forces alone can we ever make ourselves secure. If all the initial efficiency of the professional Army of which complaint was so loudly, and to a certain extent justly, made during the South African War—were all that initial inefficiency finally and completely remedied, the insufficiency of our trained numbers would remain. We should still be without a real National Reserve; we should still be without the power of immediate expansion upon the outbreak of serious hostilities. We could only grapple with the situation as before, by pulling society to pieces in the endeavour to improvise a real War Army, while a more numerous enemy would be already making dangerous progress upon the scene of action, and already bringing masses of fully trained Reserves into play.

In other words, no matter how highly efficient our small Regular Army might be, our numbers would still have to be largely supplemented by more or less haphazard methods, and by the use of raw and therefore untrustworthy military material. We cannot tell what new emergencies may occur, or under what military conditions the destinies of the British Empire may once more be at stake, directly or indirectly, upon our own frontiers or elsewhere. Amid nations which are ready for war we shall remain unready, and we shall be liable to pay the penalty, perhaps even upon a larger scale than before, and with more irretrievable, more fatal results.

As I have said on previous occasions, the Regular portion of the Army is no doubt improved from what it was at the outbreak of the South African campaign; but there is no expert of military reputation, there is no man who has given really serious and deep attention to the subject, who believes that our existing arrangements can promise us success, or ensure us against disaster in any great emergency. We have been proceeding on the assumption and the hope that no great emergency will occur. We all pray that that hope may be realised, but I should be wanting in my duty if I failed to urge upon my fellow-countrymen, with all the conviction I possess, that that hope is a broken reed to depend upon.

Whether the Regular Army is slightly larger or slightly smaller, it cannot settle the problem or provide a reasonable guarantee for national security while the National Reserves, which ought to supplement it on any serious emergency, remain altogether non-existent. Our National Military problem has been rather confused than simplified in the minds of the people by the thoughtless condemnation to which the Regular Army has at times been subjected. No doubt it was not, and may not yet be perfect. Much has been done for its improvement in the last few years, but much remains to be accomplished. The Regular Army has profited, as any Army must always profit, by the searching light thrown upon it by actual war; but, as some remarks of my own upon this subject have recently been misunderstood, I wish very strongly to declare my opinion that the short-

comings of the Regular Army were not the sole nor the main cause of our lamentable experiences in the first phase of the War; nor were the deficiencies in the training and aptitude of the Professional Forces the principal cause of the conflict dragging on for nearly three years at an utterly disproportionate cost in casualties from disease, in general suffering and hardship, and also in the enormous expenditure of money by which our national finances have been permanently burdened. To some contributory extent this was undoubtedly due to the inefficiency of preparation in our Regular Army, but it was due to a main and decisive extent to having no Reserve of sufficiently trained men to fall back upon.

What were the conditions of the South African War? It is needless to repeat that the war was waged at a distance of six or seven thousand miles from what was, and is, the true military base of our Empire—the mother country. That was a factor involving great difficulties of transport, and which interfered for a long time with the rapid organisation of our movements. But these six thousand miles of sea having been covered and the men disembarked, I want you to picture to yourselves the situation and follow in imagination the movements of the troops.

The theatre of war was of unparalleled extent, and, for all purposes of a campaign between civilised races, of equally unparalleled barrenness, I might almost say, emptiness; for the country itself afforded next to nothing in the shape of supplies. The vast business of transport thus involved had to be worked, for the greater part of the way, over a single track of railway more than a thousand miles long, passing through desolate regions, and vulnerable at every point. It was the vital artery of our operations: a block on it or a break in it meant starvation for our troops. The region through which it passed was traversed at will by a hostile and elusive population, and by antagonists offering scarcely any fixed points to be struck at, and, though relatively few in numbers, perhaps the most mobile enemy which has ever appeared in modern war. It was not enough to scatter their organisation to pieces unless it was possible to crush the fragments to powder. When a commando had been broken up, it appeared again shortly afterwards in some other part of the country. At the same time, wherever the railways extended, this most elusive of foes had a fixed objective against which to operate. The tradition of the British Army had been mainly an Infantry tradition, but in order to cope with the ubiquitous Boer, an enormous mounted force had to be improvised out of men who for the most part had never before been on a horse's back, and in the end it was found necessary to sweep the whole of South Africa into an immense military net, extending over a tract of country as large as France and Germany put together. Now, as all who took part in the War will readily admit, in face of conditions to which no army in the world could have accommodated itself—to say the least—without considerable difficulty, there were

doubtless many grave military mistakes. There were personal errors and errors of system, and perhaps I shall not be thought to trespass unduly upon ground which is not my own, when I say that many of these military shortcomings were the natural and inevitable result in the field of the mental habits then prevailing in all classes of society, and of political circumstances for which all political parties must bear their share of blame. In short, there were mistakes which more careful preparation beforehand could certainly have prevented ; there were others which, by the light of the experience then acquired, a wiser system may fairly hope to prevent in the future ; and there were others again of a kind which no system can ever effectually guard against.

My purpose is to place before you as clearly as I can that, if the Regular Army at the outbreak of the South African War had already reached the level of efficiency upon which it now stands, without being more adequate in point of numbers than it then was, no amount of skill and devotion upon the part of the officers and men could have saved us from many of the checks and disappointments met with in that struggle, or could have forced it rapidly to a decisive issue, or could have prevented it from laying a heavy financial burden on posterity.

I have recalled the physical and strategical conditions of the South African campaign, because those conditions had a remarkable effect in elucidating the problem of the numbers necessary for success. Adequate numbers proved, as is almost invariably the case in war between civilised peoples, to be the decisive influence required to bring the struggle in which we were engaged to a favourable issue. The Boers, brave and stubborn opponents, had no lines of communication, and so were able to use every man for fighting purposes. They knew every inch of the country, most of the inhabitants of which were on their side and eager to assist them, and they could strike wherever they liked. We had to be prepared at all points. The defence of our lines of communication absorbed the great bulk of our forces. It absorbed a number of men several times more numerous than the whole of the Boer Army, and the mobile forces we were able to use for offensive purposes at any given moment never outnumbered our antagonists in anything like the proportion that is generally imagined. It was very speedily realised that to prosecute the War with success far larger forces would be required than our Regular Army organisation had ever been expected to provide. The supreme lesson of the South African struggle was not the inefficient character of the Regular Army, but the utter lack of anything like a National Reserve behind that Army.

How did we succeed in the long run in providing the numbers necessary? We did it partly haphazard, partly, as I have already said, by pulling society to pieces, and partly by an enormous expenditure of money. We denuded these Islands of troops to an extent

which, I do not hesitate to say, would be impossible in the event of any complication with a foreign power. When we had used all our available trained material, we had to offer five shillings a day for the services of men who were totally untrained, and who had to be turned into soldiers in the midst of war. We had at one and the same time to use an Army and to make an Army, and it is difficult to imagine a greater strain upon any administration than was involved by such a double task.

We were fortunate in the circumstances which enabled our great Colonies to come to our aid, and in the fact that the peace of the seas was not troubled by a shot; transport between the scene of war on the one hand, and Canada, the Antipodes, and the mother country upon the other, was carried on without the slightest interruption, and, I might almost say, with railway regularity. The British population in South Africa gave priceless assistance, and I think it is hardly realised in this country that they supplied local forces equal altogether to the whole number of British troops under the Duke of Wellington's command at Waterloo. The assistance thus afforded made up in a measure for the absence of a large Home Reserve, and enabled us to use a certain number of improvised troops against a small enemy, wholly enclosed in our power and unable to reinforce itself—a condition which afforded time and opportunity for retrieving initial disaster, but which is not possible to occur in a serious war with any country other than South Africa.

We know not the day nor the hour when the horizon may darken, when we may find ourselves engaged against some infinitely more numerous, better trained and prepared, more favourably situated, and altogether more formidable enemy than our antagonists in the late campaign. The means by which we were enabled to make good our opening mishaps in the struggle of six years ago would avail us little in any theatre upon which the colossal forces of the Great Powers could be set in motion.

If such a new and grave emergency should occur in the next decade—and no man can say with certainty that it will not occur—and if it should find us still without a National Reserve behind a Regular Army, we may have to pay for the absence of that Reserve a price from the effects of which succeeding generations may never be able to recover, and for which they will never cease to upbraid us.

As I have already remarked, valuable progress has been made as regards the efficiency of the Regular Army, but we have made no effort whatever to cope with the other military problem which, as I have endeavoured to explain, is peculiar to ourselves—the problem of building up an adequate National Reserve by some means entirely apart from, and outside of, the system by which our small long-service Army is provided. It is possible that we could despatch to any part of the world 70,000 to 120,000 men, who would be sooner ready and would be better trained, and a finer and more warlike force than any

similar number of troops than have ever previously been despatched from our shores at any one time. That number, however, would be as totally inadequate for any grave emergency of the future as our available forces at the outbreak of the South African War were inadequate to grapple by themselves with the crisis at that time. Even 120,000 men, or something like three Army Corps, would be utterly insufficient as reinforcements in time of need for the defence of our Asiatic frontiers. How much less would they suffice to enable us, under modern conditions, to fulfil such of the historic purposes of our policy in Europe as circumstances may again compel us to pursue. The destinies of England may once more be decided in the future, as they have been many times in the past, on Continental soil.

There are neutral Powers which we could not with safety allow to be blotted from the map, and thus transfer to other hands the keys of sea-power itself.

We have again reached a period where the maintenance of the *status quo* in Europe, as well as in Asia, must be regarded for many obvious reasons, upon which it is unnecessary to enlarge, as one of the paramount interests, and even one of the unwritten obligations, of British policy. But with a system aiming to provide at the utmost an Expeditionary Force of two or three Army Corps, it is obvious that we can take no independent action abroad in defence of neutrals, and that we cannot hope even to throw enough weight into the balance to turn the scale in favour of our allies. In other words, we cannot exercise that general influence upon the side of peace which it is desirable in our own interests that we should be able to exercise. An Army and a Reserve must be combined to make an efficient organisation for war. We have one part of the machine, but we are entirely without the other. The Regular Army, whether, as I have said, its numbers are to be in the long run slightly more or slightly less, is moving upon the right lines, and we may hope that it will continue, irrespective of our party system, to progress in the direction of developing the highest degree of active fighting efficiency which it can be made to yield in proportion to its numbers. So far, we must all feel ourselves in hearty agreement with the many admirable passages in the speech—in some respects an epoch-making speech—made by Mr. Haldane in explaining the Army Estimates. But the points to which I have endeavoured to call your attention to-night is that, altogether apart from the professional organisation of the Regular Army, the fundamental problem of readiness for war is one that has remained wholly untouched since the South African War, and it is the one really decisive and searching problem of the present and the future in connection with all that is vaguely known as Army Reform.

The question upon which earnest and intelligent discussion must more and more concentrate itself is the question not of Reform within the Regular Army, but of securing some national power of military expansion outside the necessarily extremely limited resources of that

Army. How that National Reserve is to be created it does not fall within the limits of my object or opportunity to-night to show. But without it, believe me, no modern nation can stand upon a footing of reasonable preparedness for war, or enjoy full security for the maintenance of its territory and interests, and for the maintenance of peace.

In a few months from now it will be seven years since the first blow was struck in the South African War. It is disheartening to reflect how little has been done in that period to place ourselves in a better position than we were then, and it will take a much longer time than that to build up a sound military system, and to root in the youths of the country the aptitude for arms and a proper feeling of patriotism.

There is a great and a supreme problem to be solved, and if we hope to retain our vast possessions, and maintain the responsible positions we hold amongst nations, we must have not only a powerful Fleet, but an efficient and sufficient Army, composed partly of regular troops, but chiefly of the manhood of the country.

I give place to no man in my admiration for, and my belief in, our Navy, but it seems to me little short of madness to suppose that the Navy will always, and under all circumstances, be able to prevent the invasion of these Islands, or to secure the defence of the Empire. We must, as I have just said, have, in addition, a suitable Army, and this we shall never get until the whole nation realises that it is the duty of every able-bodied citizen to fit himself to take his share in the defence of the country.

It is because I fear that nothing short of a national disaster will make the people of this country realise this—for long years of immunity from home trouble have engendered a feeling of security which has no justification at the present day, and have induced a taste for ease and luxury to which everything must give way, and which causes the calls of duty to be felt as an intolerable interference with their pleasure and recreation—it is because of this fear that I so earnestly press for the boys and youths of Great Britain to be given an education which will teach them their duty to their country and imbue them with that spirit of patriotism without which no nation can expect to continue great and prosperous.

[R.]

WEEKLY EVENING MEETING,

Friday, March 30, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Professor P. Zeeman, *Hon. Mem. R.I.*, of the University, Amsterdam.

Recent Progress in Magneto-Optics.

It is my intention this evening to give you a general review of the experimental researches which have occupied me during the last few years. They all refer to the relation between magnetism and light, a relation the first and fundamental example of which was discovered in this very Institution, by Faraday, in 1845.

Surely every physicist should feel inspired by the idea of having the privilege to address an audience in the same lecture room, where so often some of Nature's deeper mysteries were revealed ; and I feel the uplifting force of this inspiration all the stronger, as my own work for many years has been so closely connected with one of Faraday's discoveries. Faraday discovered that the plane in which the vibrations of light take place, *rotates* whenever a ray of light is propagated parallel to the magnetic lines of force through some substances, such as Faraday's own heavy glass ; this fact we now indicate by the name of the magnetic rotation of the plane of polarisation. The discovery of this fact opened the chapter of magneto-optics.

Faraday's mind again and again returned to the relation between magnetism and light, and incessantly he sought for closer and more intimate connections ; in one experiment in March 1862 (which is said to have been his last) he tried to observe a change in the spectrum of a flame, when acted on by a magnet. The entry in Faraday's note book, preserved in this Institution with pious care, concludes with the words, "not the slightest effect on the polarised or unpolarised ray was observed." As we now know, the means of Faraday's time were not powerful enough to observe the effect sought for. Various physicists since Faraday have sought in the same direction ; some have recorded their negative results, others have not, for most physicists have an almost invincible dislike for the publication of negative results, though a collection of such unsuccessful attempts, if precisely stated, would be most interesting, and should afterwards prove very valuable.

Magnetisation of the Spectral Lines.

In my own case, the thought to submit a source of light to the influence of magnetism occurred to me during a quantitative investigation of the effect discovered by Kerr, concerning the light reflected by magnetised mirrors. I was working at the time in Leiden, in Professor Onnes' laboratory. The account of Faraday's negative experiment encouraged me in my endeavours, and also an argument in 1856 by Lord Kelvin, referred to by Maxwell as the "exceedingly important remark of Sir W. Thomson." If it might be accepted, that the forces operating during the propagation of light in magnetised substances exist also whenever the source of light is in the magnetic field, we can expect some direct effect of magnetism on radiation.

My own successful experiments date from 1896 to 1897, whereas three years earlier I also had recorded a negative result, not having then used adequate means.

As you know, a sodium flame chiefly emits two kinds of yellow light, and accordingly its spectrum when analysed with one of Rowland's large concave gratings, shows two yellow lines. With a grating of medium size, these lines have a distance of one millimetre; they are rather narrow as shown in the slide. In August 1896, I found that when a sodium flame is placed between the poles of an electromagnet, and is looked at with a spectroscope in a direction at right angles to the lines of force, the yellow lines in its spectrum become somewhat wider when the magnetic field is put on.* This fact can be expressed in a different way by saying that besides the original vibrations, a flame in a magnetic field emits other vibrations, of which some have a somewhat greater, and some a somewhat smaller frequency than the original vibrations.

This observation of a small change in a spectral line was the origin of my subsequent work. I realised that this change, however small, was worth a closer examination. Indeed, it seemed clear at once, that here we had a means of studying the internal vibrations of a molecule by modifying in a simple way the conditions under which they are going on. Of course, the result was verified in all directions. As there is now, I think, no doubt as to the reality of the observed changes, I shall only refer very briefly to this stage of the work. In the first place the widening of the lines was observed in the direction of the lines of force also. Then the fact was established that to the observed *direct* effect, there corresponds an *inverse* one. When white light traverses the incandescent sodium vapour, we observe the absorption lines; these also are widened when the vapour is subjected to magnetic forces. Secondary influences were discarded

* Zeeman. Verslagen Kon. Akademie v. Wetenschappen, Amsterdam October and November, 1896. Phil. Mag., March, 1897.

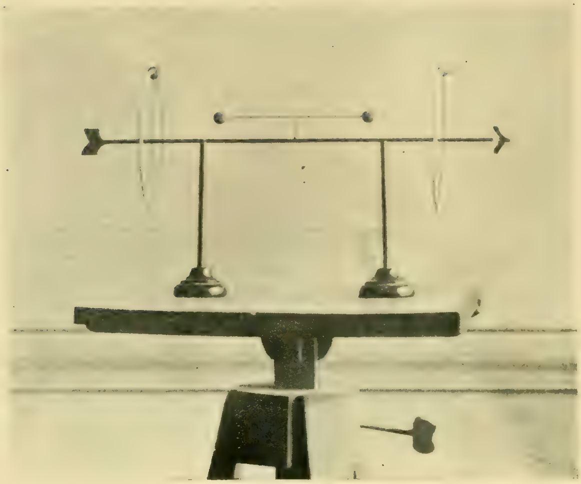

FIG. 1.

FIG. 7.

by suitable modifications of the experiments. In one case no change was observed. The spectra of fluted bands, such as those of iodine, carbon, or nitrogen did not show any effect; nor could Becquerel and Deslandres using increased power discover it.

Before I could answer the different questions which presented themselves, I had the advantage that the beautiful theory of the electromagnetic and optical phenomena, developed by my friend Prof. Lorentz, gave its quickening influence on my experimental work.

In this theory, it is supposed that the material world is built up of three things: ponderable matter, ether, and electrons. I think it is rather superfluous to remind you here in the land of Maxwell, Kelvin, Crookes, J. J. Thomson, Schuster, Larmor, Heaviside, and Johnstone Stoney, that electrons or corpuscles are exceedingly small, electrically charged particles, which are supposed to be present in all material bodies.

These electrons can perform oscillations, under the influence of the forces which attract them to their position of equilibrium. Because they are electrified, they have sufficient hold on the ether to excite in it the electromagnetic vibrations, which, according to Maxwell's theory, constitute light.

The oscillatory periods of the electrons determine the position of the lines in the spectrum, and with every change in the period of oscillation we observe a displacement of the corresponding line.

In Lorentz's theory the explanation of the effect of a magnetic field is as simple, as it is beautiful.

The forces operating on the vibrating electron in a magnetic field are fairly well known. These forces are the same which curve the path of the kathode rays in a vacuum tube which is acted on by a magnet. All motions of the electrons in the molecules of a flame may be supposed to be made up of three particular motions, chosen in such a manner that the action of the magnetic field on each of them can be easily foreseen. The light of the flame is exactly the same as it would be, if the flame contained three groups of electrons, vibrating in these simple ways.

In this model, the electrons are represented by red balls, the black arrow indicates the direction of the magnetic force. (Fig. 1.)

As a first simple motion we choose a vibration parallel to the lines of force. On the group of electrons which possess this motion, the magnetic force has no influence; the period, which we call T, remains unmodified. The other two simple motions are circular motions, clockwise or anti-clockwise, in planes perpendicular to the lines of force.

An electron performing either of these rotations, will be acted on by a force which is directed towards or from the centre, dependent on the direction of the rotation. The magnetic field must, therefore, cause the speed of the electron either to increase or to decrease, and so will either increase or diminish the period. Therefore, instead of one motion with period T, we get under the influence of the field

three motions with periods T, $T+v$, $T-v$, v being a small quantity. To each motion of the electrons there corresponds a luminous vibration, according to the electromagnetic theory of light. Observing with a spectroscope we must, therefore, see each spectral line divided into three lines; each line becomes a *triplet.**

I will show you a few examples of lines which are really divided into three components, in accordance with Lorentz's theory. (Fig. 2, iron; Fig. 3, zinc; Fig. 4, part of iron spectrum; Fig. 5, part of same spectrum.)† You will notice that each of the components remains very narrow; it is not a hazy effect, but a very definite one. This certainly would not be the case if all molecules did not behave in the same manner, and if certain conditions of isotropy of the molecules were not fulfilled.‡

The consideration of the model may illustrate some other points which were foreseen by Lorentz's theory.

Consider the light emitted at a right angle to the lines of force. The three kinds of light seen in this direction are each due to vibrations of one kind, and therefore polarised. We can, therefore, extinguish the light of the central component, or of the two external components of the triplet by a Nicol. In one half of the slide shown, the external components are extinguished; in the other half, the central one. (Fig. 6.)

So, for the first time, we were now able to get polarised radiations from the molecules of a gas. All attempts to produce such simple vibrations from gaseous molecules had hitherto failed.

With some lines, the central component and the outer ones differ much in intensity. If this be the case, the spectroscope can be dispensed with entirely, and we may observe a partial polarisation of the light emitted by the vapour in the field as found by Egoroff and Georgiewsky.

We shall now consider the light emitted in the direction of the lines of force. (Fig. 7.)

It is seen at once that each line must split up into two components. Moreover, both lines must be circularly polarised, but in opposite directions. With suitable arrangements, in one half of the field of view the one, in the other the second component can now be extinguished. I observed this circular polarisation for the first time in the case of the sodium lines now shown. You see how complete

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* Zeeman. Verslagen Kon. Akademie v. Wetenschappen, Amsterdam, Mei, Juni, October, 1897. Phil. Mag., July and September, 1897.

† The photographs illustrating this lecture are, excepting the diagrams, enlarged copies from negatives. The scale is different in the various cases. The separation of the outer components is of the order of one-sixth of the distance of the sodium lines (the vertical lines in Fig. 12). No. 2 is a copy of one of the first photographs I obtained. The author is indebted to Prof. Runge for Nos. 21, 22, 24. The nonet is not distinctly shown in the latter reproduction.

‡ Lorentz. Annalen der Physik, Bd. 63, p. 278, 1897.

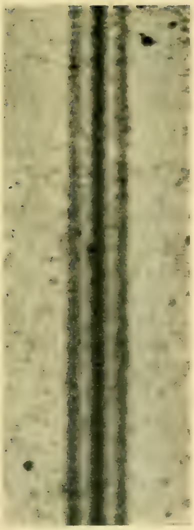

Fig. 2.

Fig. 3.

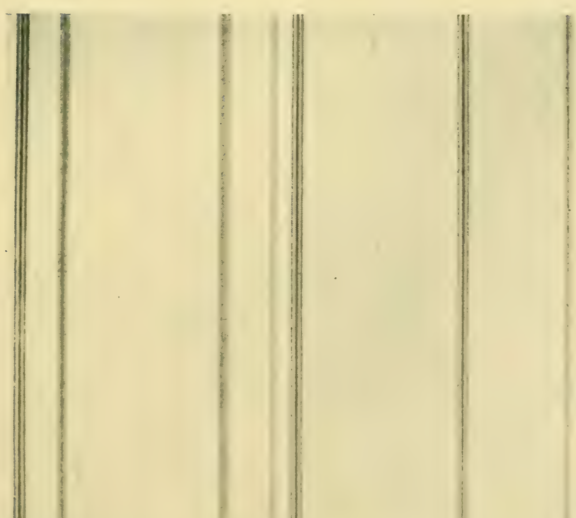

Fig. 4.

FIG. 5.

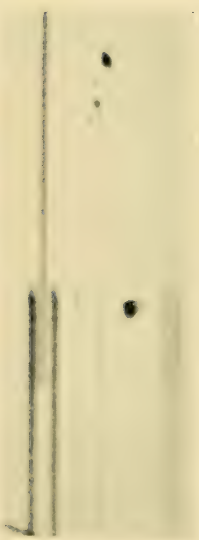

FIG. 6.

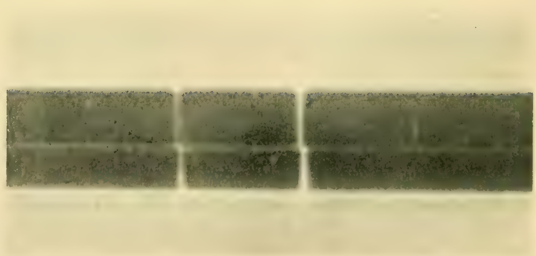

FIG. 8.

the circular polarisation is. There is no trace of rectilinear or of elliptic polarisation.* (Fig. 8.)

When I first looked for this circular polarisation, I did not have the field of view divided into two parts, but the position of the line was determined by means of a spider's thread. On the reversal of the magnetising current, the luminous line moved. I do not wish to disguise the fact that no observation has ever afforded me so much pleasure as this one.

It has already been remarked that we can also study the absorption lines which become visible when white light is transmitted through the vapour. We then study the inverse effect. I shall use it to show you at least something directly depending upon the effect, because the effect itself is too young to appear before so large an audience. The inverse effect for light parallel to the lines of force plays a part in an experiment due to Righi.† Consider a horizontal ray parallel to the axis of an electromagnet with pierced poles, and let crossed Nicols be placed before and behind the instrument, as in Faraday's experiment. A sodium flame in the field emitting two kinds of circularly polarised rays, absorbs these same radiations, but does not stop the radiations polarised in the opposite direction. These remaining circularly polarised rays cannot be extinguished by a Nicol.

The brilliant yellow spot which appears on the screen as soon as the current is put on, is due to such rays. The explanation of this experiment is not complete however, at least not for denser vapours. The Faraday rotation of the plane of polarisation then plays a part, as we shall see further on.

The magnetisation of the spectral lines allows us to determine whether positive or negative electrons are vibrating in a flame. From the phenomena in the direction of the lines of force, it follows that in a luminous gas *the negative* electrons give rise to all vibrations. It does not follow, however, that the luminous molecules have a negative charge. On the contrary, the researches of Lenard and Stark show that at least part of the luminous spectra is emitted by positively charged atoms.

When a line is split up into a triplet, we can, by measuring the amount of the effect, find out how much matter is loaded with the revolving electron, or in other words, we can determine the ratio of the charge e to the mass m of the electron. In this manner, I have made the first determination of this notable number $\frac{e}{m}$, and found it of the order of magnitude of 10^7 electro-magnetic units per gramme.‡ The most accurate measurements of the present time for different

* Cf. Larmor. Aether and Matter, p. 345, 1900.

† Righi. C.R. 127, p. 216, 1898. C.R. 128, p. 45, 1899. Nuovo Cim. (9), 8, p. 102, 1898.

‡ Zeeman. Verslagen Kon. Akademie, Amsterdam, November, 1896. § 23.

spectral lines yield values ranging between 1·4 and 1·8 by 10^7. This number is about 1500 times the corresponding number for hydrogen as deduced from the phenomena of electrolysis.

We must, then, conclude that at least a majority of spectral lines is due to the vibrations of the negative electron. This conclusion is not only valid for incandescent sodium or mercury. All elements, which can give colour to a flame, or which can be evaporated in a spark, show the magnetisation of the spectral lines, and hence in all elements these negative electrons are present.

Independent experimental evidence for the existence of electrons has been derived from the study of the kathode rays in a vacuum tube. The discontinuous structure of electricity was also proved by other phenomena, and in this way physicists were led by purely experimental methods to the negatively charged corpuscle of J. J. Thomson, 1500 times smaller than the hydrogen atom, in full accordance with the electron necessitated for the explanation of the magnetisation of the spectral lines.

All fundamental characteristics of the magnetic resolution of the spectral lines were then explained, and the truth of the explanation proved beyond the possibility of doubt. More detailed knowledge of the effect has been greatly extended by a whole series of investigators, especially by Becquerel, Cornu, Cotton, Michelson, König, Righi, Runge and Paschen, and in this country by Gray, Preston, Lodge, Lord Blythswood and others, and from the theoretical side by Larmor, Fitzgerald, Jeans, and J. J. Thomson.

Not all spectral lines are tripled, some are split up into quartets, others into sextets. The lines D_1 and D_2 in strong fields are an example. (Figs. 9 and 10.) The whole of such a system of lines is, even in the strongest fields, confined to the space of one-sixth of the distance of the sodium lines. In some cases still more complicated sub-divisions have been observed, especially by Michelson. In such cases the simple electro-magnetic model of a molecule emitting light is insufficient. We shall return to this subject afterwards, and first proceed to a discussion of *phenomena accompanying the inverse effect.*

This investigation, which I carried out in Amsterdam together with my pupils, Drs. Hallo and Geest, was suggested by a theoretical investigation by Professor Voigt, of Göttingen. Lorentz's theory relates to one single vibrating particle and can only be applied to substances of very small density, which emit very narrow spectral lines. With greater density, and therefore broader spectral lines, the mutual influence of the molecules must be taken into account. It seems, however, that a theory of emission of a system of reciprocally reacting molecules is rather difficult. In the case of absorption the problem is easier and is considered by Professor Voigt, in his theory of magneto-optical phenomena.* He does not deal with the

* Voigt. Annalen der Physik, Bd. 67, S. 345, 1899.

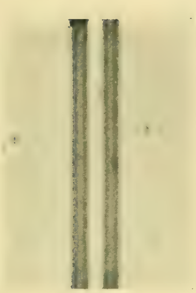

Fig. 9.

Fig. 10.

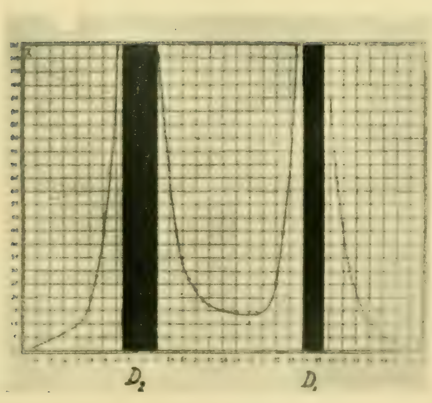

Fig. 11.

electrons directly, but adds suitable new terms to the equations of motion in an absorbing medium. His method establishes a connection between the rotation of the plane of polarisation and the resolution of the spectral lines, a connection almost simultaneously pointed out by Fitzgerald. This also led to an interesting result, till then missed by the electronic theory, namely :—

*Rotation of the Plane of Polarisation close to an Absorption Band.**

Faraday's rotation of the plane of polarisation is extremely small in all gases, also in sodium vapour. Only within a very narrow range close to the sodium lines the rotation is positive and very great, a fact discovered by Macaluso and Corbino. In a recent extremely interesting paper, Professor Wood has given measurements of observed rotation of four complete revolutions.† This, however, was in rather dense vapour, at least dense in comparison with the vapour used in the experiments now to be described, in which vapour containing about one-millionth gram of sodium per cm.^{3} was used.

The magnitude of the rotation close to the sodium lines is illustrated by measurements made by Dr. Hallo in the Amsterdam laboratory. It is clear that on both sides of an absorption line the rotation is in the same direction. (Fig. 11.)

We may attenuate the vapour still further so that the doublet in the direction of the lines of force becomes visible. What is the rotation then between the components of the doublet?

It is easily deduced from Professor Voigt's theory that in very diluted vapours the rotation must occur, in a sense, opposite to that outside the components, and therefore negatively, and also that it must be very great. In the case of sodium vapour, I had the pleasure to confirm this theoretical result, and to observe rotations of $-400°$.

In these experiments interference fringes in the spectrum were used, established by means of a system of Fresnel quartz wedges (a method used by Voigt, Corbino, and others in similar cases). I will project these fringes on the screen.

If a plate of quartz, which rotates the plane of polarisation, is held in the ray, you will notice a displacement of the fringes. A plate of glass has no influence of course. I have here a quartz plate which rotates the plane of polarisation through 90°, and you will notice a displacement of half the distance between two fringes. A displacement of the entire distance between two fringes corresponds to a rotation of half a revolution.

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* Zeeman. Proc. Ac. Sciences, Amsterdam, May, 1902. Hallo. Thesis, Amsterdam, 1902, Archiv, Néerl., ser 2, T. 10, p. 148, 1905.

† Wood. Phil. Mag., October, 1905.

Analysing the light by means of a Rowland grating we can produce such a system of fringes for all wave-lengths, and we can consider the rotation for wave-lengths close to the controlling absorption bands. On the screen I will first project the fringes close to the sodium lines with the field off. The dark vertical lines are the sodium lines. They are broad, because the vapour is rather dense. The horizontal bands are the interference fringes. (Fig. 12.)

With the magnetic field on, the image now projected is seen. (Fig. 13.)

You see how fast the rotation increases in the vicinity of the absorption lines, becoming more than 180° closer to the bands. In the interior of the bands only a hazy fringe is seen. A remarkable equation, first deduced by Becquerel,* gives the law of the rotation.

The phenomenon is more beautiful as soon as the vapour is so thin that the doublet is seen. (Fig. 14.)

Outside the components of the doublet the fringe rises *upwards*. But inside the components the fringe has moved *downwards*, the rotation is negative there. The rotation is $-90°$ for D_1, nearly $-180°$ for D_2. It is very interesting to watch the movement of the fringes in the spectroscope as the field is increased or the density of the vapour changed.

Double Refraction and Resolution of the Absorption Lines.

In the second place we will now consider the *double refraction* which occurs whenever light traverses a vapour at right angles to the magnetic field. A plane wave with vibrations parallel to the field has a velocity different from that of a wave with vibrations at right angles to the field. It is only close to the absorption band that the difference becomes perceptible. Sodium vapour in a magnetic field behaves as a double refracting crystal for light close to the sodium lines. This result of Voigt's theory was verified by him in conjunction with Wiechert in the case of dense vapours, and commented upon by Becquerel and Cotton.

With great density, and using the same system of interference bands, the phenomenon assumes the appearance now projected. Whereas the rotation of the plane of polarisation was symmetrical on both sides of the absorption band, you see that the double refraction is not. On one side of the absorption line, sodium vapour behaves like a positive crystal, on the other side like a negative one.

With very dilute sodium vapour, and with a magnetic field strong enough to resolve the sodium lines, the theory must be extended. There is no difficulty here.

The observations made by Mr. Geest, as well as by myself, con-

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* Becquerel. C.R. 125, p. 679, 1897. Cf. also Schuster. The Theory of Optics, p. 291–294, 1904. Siertsema. Proc. Ak., Amsterdam, 12, p. 499, 1903.

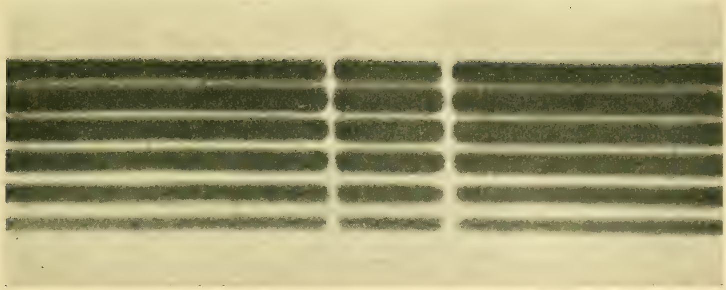

FIG. 12.

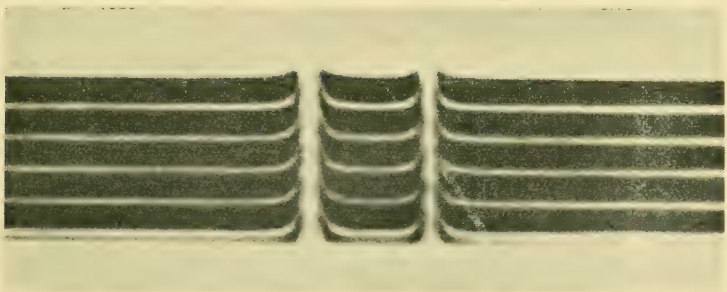

FIG. 13.

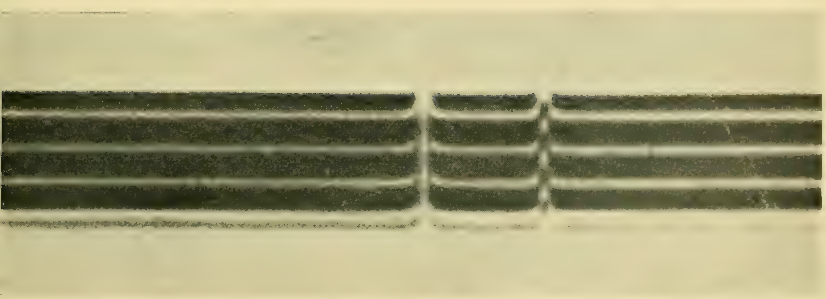

FIG. 14.

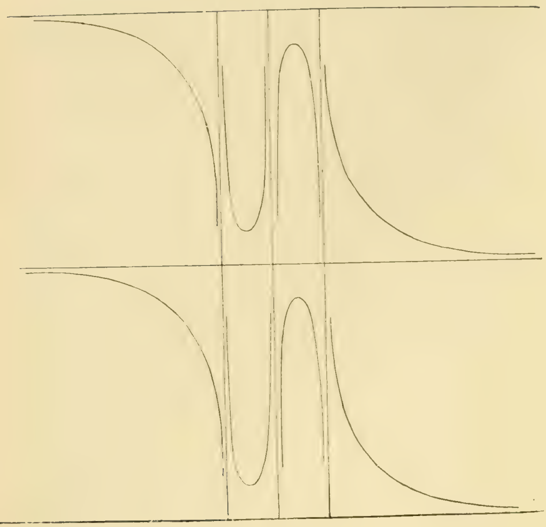

Fig. 15.

Fig. 16.

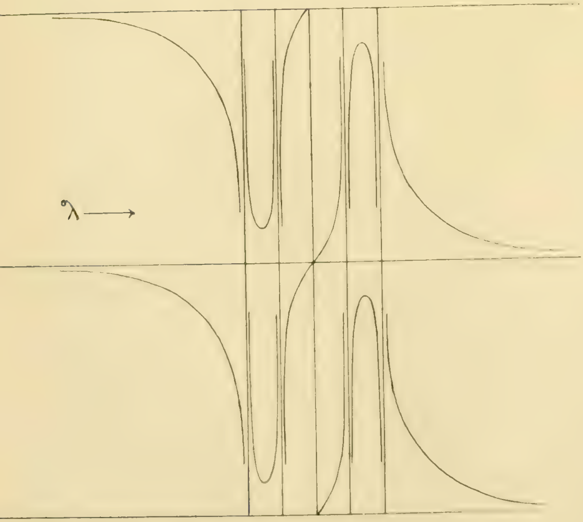

Fig. 17.

Fig. 18.

cerning the details of this double refraction, have fully confirmed Voigt's theory.*

The slides shown always refer to *one* of the yellow sodium lines, and hence the structure seen is almost entirely confined to the extremely small region between the components of one line.

The line D_2 splits up into three components in a moderate field. The theoretical course of double refraction is given in a diagram; next to it the result of observations is given. (Figs. 15 and 16.)

On a somewhat larger scale the appearance is as now shown; with greater density the characteristic sinuous line undergoes transformation.

The line D_1 splits up into a quartet. Besides the concave parts you will now notice a line with a point of inflexion in the theoretical and in the observed curves. (Figs. 17 and 18.)

The same phenomenon is again illustrated by the next slide, where also the change which occurs with greater density is manifest. In a very strong field the line D_2 is resolved into a sextet. The inverse sextet can be readily seen with the means at our disposal. But the phenomena occurring between these narrow spaced components could only be seen with difficulty. Only under very favourable circumstances Mr. Geest observed the image now projected.

All the described phenomena are qualitatively in excellent accordance with Voigt's theory. It is certainly very interesting that the theory is able to explain the complicated course of double refraction by the difference between the velocities of propagation of vibrations at right angles and parallel to the field.

Magnetic Resolution and Intensity of Field.

Let me again refer to our first subject, the magnetic separation of the lines. The magnitude of this separation is proportional to the intensity of the field in which the source is placed. We may, therefore, deduce the intensity of the field from the magnitude of the magnetic separation. We have only to measure the distance of the components of a suitable line. It is not generally known that this distance can be increased with great accuracy (with an error of far less than 1 per cent.). It is, therefore, far easier, if a relatively high degree of accuracy is necessary, to compare the intensities of field by measurements of the distance between the components than by direct magnetic measurements.

All methods used for the measurement of magnetic fields give us the intensity in a point. On the other hand, the magnetic resolution of spectroscopic lines can give us the intensity *in all points belonging*

* Zeeman and Geest. Proc. Acad. of Sciences, Amsterdam, May, 1903, December, 1904. Geest. Thesis, Amsterdam, 1904, Archiv. Néerl, sér 2, T. 10, p. 291, 1905.

to a line. Moreover, in this manner, we make direct use of a property of the atom.

You see here a vacuum tube with some mercury. We heat the tube and excite it with the coil. You notice the brilliant light which is, however, greatly increased when the tube is placed in a magnetic field.*

For a given density of the vapour, there is a definite intensity of field for which the luminosity is a maximum. You can see this when we put on the current in the electro-magnet, the intensity of the field then rises gradually.

We project an image of the tube on the slit of a spectroscope. This spectroscope must be so arranged that to every point of the slit there corresponds a point of the image. The blue line of mercury (4359) resolves into a sextet. Using this line the field of a du Bois electro-magnet with a pole distance of 4 mm. is mapped out in the spindle-shaped optical magneto-grams now shown. (Fig. 19.) We may, of course, extinguish the light of the inner components. In some cases a triplet will give more accurate results. The method sketched will, of course, only be applied in difficult cases. As long as our spectroscopes of great resolving power are rather cumbersome, there is no practical application for the method.

By means of this method we may also study some questions as to the way in which certain phenomena, which accompany the resolution, depend on intensity of field.

We have no time, however, to discuss this further, because I should like to refer to the important subject of the

Behaviour of the Different Lines in the Magnetic Field.

In many metallic spectra a number of lines occur which are closely related and form so-called series of lines. The important discoveries of Hartley, Liveing and Dewar were followed by the discovery of series, owing to the indefatigable efforts of Balmer, Kayser and Runge, Rydberg and Schuster.

The plate shows diagrammatically the arrangement of the three connected series which are found in the spectra of the alkalies and other elements, and which are distinguished by Professor Schuster † as the trunk series (Kayser and Runge's "Hauptserie"), the main branch series (Kayser and Runge's "Zweite Nebenserie"), and the side branch series (Kayser and Runge's "Erste Nebenserie").

The laws of these series are simpler than those governing acoustical vibrations. They are of an entirely different character; for instance, the members of each series approach some definite limit of frequency, whereas the number of acoustical vibrations may increase indefinitely.

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* *Paschen.* Physik. Zietschr., I. S., 478, 1900.
† Schuster. The Theory of Optics, p. 282, 1904.

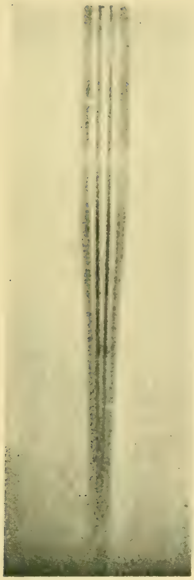

Fig. 19.

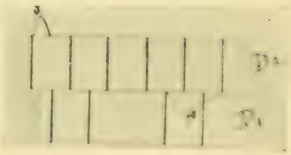

Fig. 20.

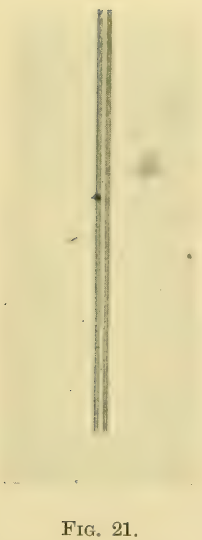

Fig. 21.

Fig. 22.

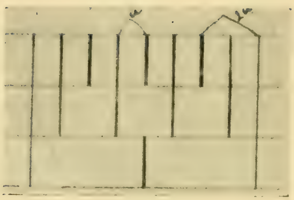

FIG. 23.

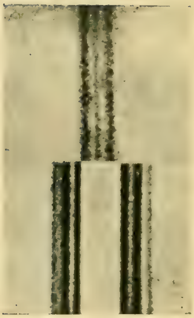

FIG. 25.

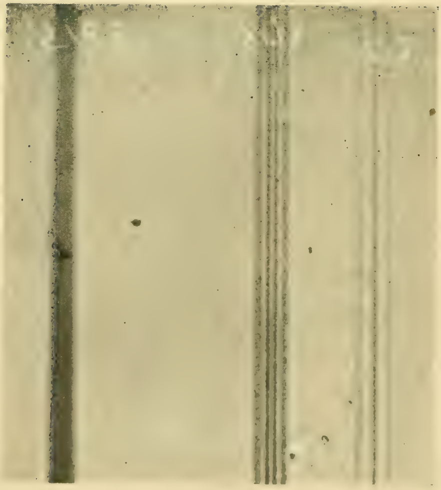

FIG. 24.

My first measurements already made it evident that lines of different series behaved entirely unlike each other.* Hence the ratio of charge to mass could not be the same for all vibrating electrons.

Runge and Paschen have proved in a most beautiful and systematic investigation † that all the lines of a trunk or of a branch behave in the same manner. This result was first announced by Thomas Preston,‡ but it is not stated to what degree of accuracy and for how many lines he investigated the subject.

All lines of the same series are split up in the same manner, e.g. all lines are resolved into triplets or all into nonets. Moreover, not only the general type of sub-division is the same, but even the amount of separation, when measured in oscillation frequency.

The second law discovered by these physicists is this: That corresponding series of *different* elements show the same type of resolution, and the amount of separation is the same when measured on the frequency scale.

In the alkalies, each line of the trunk series is double, and we may speak of a twin trunk. The yellow sodium lines are a typical example. The type of resolution of the two lines is shown in the diagram. (Fig. 20.) Here we have again our old sodium lines in the field. The same division occurs in all cases when twin trunks exist. Substances so different in chemical behaviour as sodium, copper, silver, and calcium (e.g. the well known lines H and K), split up in the same manner. And I think that even Sir William Crookes will be surprised to hear that his thallium lines are in the magnetic field only counterfeit sodium lines. I can show you the splitting up of these beautiful thallium lines in the slide. (Figs. 21 and 22.)

With zinc, cadmium, mercury, calcium, there are *three* main branches associated with each other. The amount of separation is the same in each of these branches. The type of resolution is shown in the diagram. (Fig. 23.) I can show you further lines of mercury, the triplet, the sextet, the nonet. Another example of the same sextet is given by a zinc line. The next slide refers to some beautiful magnesium lines, exhibiting the same three types of resolution. (Fig. 24.)

We see that in these cases the simple image of an oscillating electron does not apply. I regret to say that the electronic theory cannot yet give us the explanation of the more complicated resolutions, even for the quartet, we are yet in want of a model.

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* Zeeman. Verslagen Ak. v. Wetenschappen, Amsterdam, December, 1897. Phil. Mag., February, 1898.

† Runge and Paschen. Berl. Akad. Abhandlungen, Anhang, 1902. Sitzberichte, Berlin, p. 380, p. 720, 1902. Runge. Physik Zeitschr., 3. Jahrgang, S. 441. Kayser. Spektroscopie. Band 2. Kapitel IX., 1902.

‡ Preston. Dublin Trans. (2) 7. p. 7–22, 1899.

The laws discovered, however, seem to point to the conclusion that all the lines of a series are emitted by one oscillating system, that there are, therefore, as many series in the spectrum of a substance as oscillating systems in its atom; moreover, that the oscillating mechanism is the same in different elements. We are reminded here of the view advocated by Sir Norman Lockyer that the different elements have something in common.

The relation between these spectral series and resolution in the magnetic field is so close that we may expect that the solution of the problem of the series will give at the same time the solution of the magnetic separation problem.

That Lorentz's theory is on the right track even in the case of the more complicated magnetic effects appears from the polarisation of the nonet shown in the slide. (Fig. 25.)

Three groups of vibrating lines here correspond to the three lines of the triplet.

The circular polarisation corresponds also to that of the doublet indicating that it is always the negative electron which executes the vibrations.

There is yet room enough for experimental work in extending these investigations in different directions and to other elements.

Much light on our present subject will be thrown undoubtedly by the activity in adjacent chapters of physics. I can only mention in this relation the extremely interesting experiments by Lenard and Stark on the centres of emission of different spectral series, and the important theoretical work by Drude* on the optical properties and electronic theory.

Maxwell has said "an intelligent student armed with the calculus and the spectroscope can hardly fail to discover some important fact about the interior structure of a molecule." I think this statement remains as true now as it was thirty-two years ago.

There can be no doubt, I think, that spectrum analysis and especially the magnetisation of the spectral lines will give us a clue to the inner structure of the atom.

I hope that I have succeeded in imparting to you this, my conviction.

[P. Z.]

* Drude. Annalen der Physik, pp. 677, 936. Bd. 14, 1904.

GENERAL MONTHLY MEETING,

Monday, April 2, 1906.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Matthew Atkinson Adam, Esq., B.Sc.
Walter Andrew Harper, Esq.
Thomas Bell Lightfoot, Esq.
Gerald Allen Moore, Esq.
John Perry, Esq., LL.D. D.Sc. F.R.S.
Max Hermann Karl Poser, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on th table, and the thanks of the Members returned for the same, viz. :—

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WEEKLY EVENING MEETING,

Friday, April 27, 1906.

SIR WILLIAM CROOKES, D.Sc. F.R.S.,
Honorary Secretary and Vice-President, in the Chair.

PROFESSOR JOHN W. GREGORY, D.Sc. F.R.S.

Ore Deposits and their Distribution in Depth.

PRIMITIVE man obtained his limited store of metal by gathering scanty grains and pebbles of ore from the beds of streams; but even in pre-historic times he worked shallow mines, for Job tells us that the gold miners of his day not only diverted rivers from their courses, as in modern alluvial practice, but they put forth their hands upon the flinty rock, by which he doubtless meant quartz. Still less do alluvial ores satisfy modern requirements, even though the great metal-using countries place the rest of the world under tribute of its mineral wealth; and owing to the exhaustion of the richer ores in the more accessible mining fields, miners are now thawing the frozen gravels of Klondike, washing the gold-bearing loams in the Siberian rivers during their short summer flow, mining in the malarial jungles of West African coast lands, and sluicing low grade drifts in the heart of Africa, in Katanga.

We cannot expect many more discoveries of alluvial deposits as rich and as extensive as those of California and Australia; nor can we go back to the low metal output of a century ago. The exhaustion of coal we destroy so wastefully might not be an irremediable disaster, for there are other sources of heat and power; but the metal famine that would follow a return to the metal supplies of 1800, or even 1850, would destroy the whole fabric of our civilisation.

The supply of metals can be maintained in two ways. We may work superficial alluvial deposits with more refined methods of metal recovery; or we may mine the more deeply buried primary ore-deposits.

The simplest method of collecting ores is to wash the gravels that contain them in a tin dish; but this method is only profitable with ground so rich that it can be handled in small quantities. Poorer material can be washed in simple machines, but they are only successful economically, when the ore has undergone preliminary concentration by nature. Sluicing and dredging recover profitably the minute grains of ore scattered through thick sheets of barren material. Dredges work with marvellous economy. A dredge will

haul up a ton of gravel from a river bed, sort it, wash it, and extract its gold, at the cost of three half-pence. It has been predicted that this invention, which we owe to New Zealand, will so greatly increase the gold yield of the world, as to repeat the revolutionary effect on prices occasioned by the gold discoveries of California and Australia in the middle of the last century.

But economical though these dredges be it is doubtful whether they can maintain an adequate metal supply, and we are becoming increasingly dependent on the mining of deeper ore-deposits. They belong to two main groups :—

1. Ores of alluvial and sedimentary origin ;
2. Ores deposited in lodes and masses.

Deep alluvial and sedimentary ores only pay to work for precious metals, and practically only for gold. They are of two main types : deep leads, and buried sheets of gold-bearing conglomerates. The deep leads are the beds of old rivers, which have been buried under thick sheets of river deposits or lava flows. In working these deposits the course of the old rivers has to be discovered by boring through the overlying rocks ; and when the old river system has been mapped out, the miner can pump the gravels dry, then dig them out, and extract from them their gold.

The only deeply buried, wide sheet of gold-bearing sedimentary rock, which is of first-rate mining importance, is the banket of the Transvaal. The banket is a marine conglomerate, composed of pebbles of quartz, and some pebbles of what is now pyrites. The rock was doubtless formed on a sinking shore-line. According to the explanation which seems to me most probable, the Rand gold was derived from the wearing away of gold-bearing quartz lodes. The sheets of pebble-reef have been traced east and west through the length of the Rand ; and they may occur to the south of Johannesburg, in the bottom of a great basin, far deeper than they can at present be profitably worked. Preparations are being made for mining the banket at the depth of 5000, and even 6000 feet, and if the gold be alluvial in origin, the limit of working will be determined by expense, and not by any limit in its distribution. It is, however, authoritatively held that the Rand gold is not alluvial in origin, but has been formed by impregnation, as in ordinary gold-quartz lodes. If so, the banket is only a very abnormal lode, and the downward limit of its ores will depend, not on the depth to which the conglomerate goes, but on the factors that control the deposition of ores in lodes.

Lode-Caps.

The future metal supply of the world will doubtless come from ores deposited by secondary processes in lodes or masses, and the depth

to which these ores extend depends on the depth at which these secondary processes can take place.

The lodes are generally first found and worked on the surface. The lodes may occur as walls of rock, left upstanding by the wearing away of the soft slates beside them. Lode mining began in Australia by a party of miners chipping out the pieces of quartz that contained visible gold from such an outcrop, and then breaking up the quartz with hammers.

The exposed part of the lode having been used up, the miners follow it downward as deeply as it pays them to go with the appliances available to them. Mining has shown that there are two main types of lodes; some ores are in flat sheets or veins, others in huge masses, which may consist of solid blocks of ore, or of rock impregnated by veins, veinlets, or even scattered grains. Both types of ore-deposits when followed down, are often found to become thinner and perhaps to pinch out altogether. Even if the lode continue downward, there is soon a marked change in the character of the ores. Thus, in a gold-quartz lode, the work is easy near the surface, till the miner reaches the level where the rocks are charged with water; this change occurs at a depth which varies indefinitely, and may be as much as 1600 feet, but is often met at from 80 to 150 feet. Then comes a great increase in the difficulty of mining, and a great decrease in its profit.

Above water level the rocks are soft and decomposed, and full of cavities; below they are hard, and compact; above that level they are dry, below they are wet, so water oozes into the excavations which have to be kept empty by costly pumping. The ore above water level is stained rust-red, and the gold is visible, so the good and barren grounds are readily distinguished; below that level the ore is dark in colour, and usually charged with pyrites, with which the gold is combined, so it is no longer to be seen by the eye. The gold, moreover, may be associated with minerals, which may hamper its extraction. Hence, instead of the miner having to deal with an ore from which the gold can be easily obtained, it becomes refractory and may require difficult metallurgical treatment. Again, the upper part of the lode being full of cavities, from which heavy materials have been removed, a cubic foot of it weighs less than a cubic foot of the compact lode below, so the gold, if equally distributed, is richer per ton weight of ore above than below water level.

Hence a party of miners finds that a lode, which has paid them handsomely at first, in time becomes quite unprofitable; and the only thing to be done, when the incoming of the sulphides announces the end of the easily mined ore, is to sell the mine to a company, and start farming with the money.

The Lateral Secretion Theory.

The conversion of successful working miners into farmers was once stimulated by the widespread belief that these changes in the lodes indicated that gold is a surface formation, and that gold quartz lodes would not persist below the depth of about 300 feet. Thus according to Sir Roderick Murchison, "as frequently as deep mines enriched the speculators who sought for copper and silver, so surely gold mining in the solid rock proved abortive, owing to the slender, downward dissemination of gold in a hard and intractable matrix." And he maintains that it is an "indisputable fact that the chief quantities of gold, including all the considerable lumps and pepitas, have been originally imbedded in the upper parts of the vein-stones." Murchison insisted repeatedly on the essentially superficial distribution of gold ores.

It is true that many of the earliest students of ore-deposits regarded all ores as derived from a "Pluto's Hoard" in the inner recesses of the earth. But this view was opposed to many of the most obvious facts. Thus lodes are often found to be closed below, in which case it is easier to explain them as filled from above, or from the sides, than from below. Again, lodes are often poor below and rich above, whereas the contrary would be expected if the heavy ores came from the interior of the earth. The veins known as gash-veins, which are entirely confined to one bed of rock, can, therefore, only have received their contents from it; yet the contents of these gash-veins are identical with those of the great fissure lodes, and so they also may have obtained their materials from the rocks beside them.

A further strong argument in favour of the local origin of the lodes is the character of the vein-stuffs, the non-metallic lode materials in which the metallic minerals occur. These vein-stuffs vary according to the rock beside the lode. Where a lode traverses sandstone or other silicious rocks, the vein-stuffs consist mainly of quartz; when it traverses limestone or igneous rocks rich in lime, calcite is a common vein-stuff. In such cases it is obvious that the vein-stuffs have been brought from the sides and not from below; and the ores are so intimately mixed with the vein-stuffs that it was natural to infer that both had been introduced by the same agency, and had been derived from a common source.

The lodes, moreover, in many mining fields show essential dependence on the rocks beside them, and apparent complete independence of the rocks below. Thus in Cumberland, the lead lodes are apt to be broad and rich where they occur in limestone, and narrow and barren where they traverse shale and sandstone. Again, in the famous copper-field of Thuringia, the ores are confined to those parts of the veins that occur in a narrow band of slate, above and below

which they are practically always barren. It was claimed that the ores had been originally precipitated from the water of the sea during the deposition of the copper-slates, and subsequently concentrated in the veins.

The first adequate scientific statement of the theory that the ores in lodes are leached out of the rocks in which the lodes occur, was by Bischof in 1847. His pupils endeavoured to justify this lateral secretion theory by demonstrating the existence of metals in ordinary rocks; but for thirty years their efforts were practically in vain. Then Fridolin Sandberger varied the method of search.

Most sedimentary rocks contain small quantities of accessory minerals, consisting of fragments of the more durable constituents of igneous rocks. The bright idea occurred to Sandberger that if any metals occur in sedimentary rocks, they would be found in these accessory minerals. An element present as a mere accessory constituent, in a scarce accessory mineral, would form an infinitesimal proportion of the rock, and it would thus escape detection on a bulk analysis. So Sandberger first concentrated the accessory constituents, and then analysed these concentrates. The result was the discovery that minute quantities of various metals, including lead, zinc, copper, cobalt, nickel, etc., are widely distributed in rocks and especially in the old slates, in which most ores occur.

This discovery removed the one objection to the lateral secretion theory that had appeared insuperable, for it showed that the common rocks, in which lodes occur, contain a supply of metal which lateral secretion can collect into lodes.

It is always a pleasure to solve a difficult problem by substituting known forces, working on accessible materials, for unknown agents, acting under conditions and on materials that must ever remain beyond the range of experiment and direct observation. So the lateral secretion theory at once sprang into popularity; and it was applied in one after another of the great mining fields of the world. Thus Emmons attributed the origin of the silver-lead ores of Leadville to lateral secretion from the overlying porphyrites. Becker derived the silver of the Comstock bonanzas from the adjacent diorites and andesites; Chamberlin and Kendal taught us to look for the source of the lead in the gash-veins of Wisconsin and of the north of England, to the limestone in which the veins occur; Howitt and Rickard advocated the view that the gold in the quartz lodes of Australia has been leached out of slates, into which it had been precipitated from the water of the Silurian seas.

But this fascinating and simple lateral secretion hypothesis is now generally abandoned. To mention only one strong objection, it does not explain the occurrence of lodes with different ores in the same country rock. Moreover, it supplies no explanation of the original source of the metals and the ores. For the metals found in the accessory minerals of sedimentary rocks are not necessarily present in

them as primary constituents ; they appear to have been subsequently introduced, for they are associated with minerals, which the microscope proves to be secondary in origin.

The study of ores in recent years has made great progress by recognition of the fact that an ore is a rock, and must be studied on the same lines as ordinary rocks. The microscopic study of ores is throwing much the same light on their depth and distribution, as it has done on the depth and genesis of igneous rocks. A rock is investigated both in the field and under the microscope in reference to three main problems : first, the origin of the materials of which the rock is composed ; second, the nature of the agents which have carried the materials to their present position ; and third, what deposited them there.

The materials of any rock or ore can only come from one of three sources :—

1. Matter dissolved in the waters of the sea.
2. The igneous rocks which, either directly, or indirectly as the sedimentary rocks derived from their destruction, form the whole of the earth's crust.
3. The interior of the earth.

Sea Water as a Source of Gold.

Gold is the only metal for which the sea is considered a possible original source. The existence of gold in sea water was first seriously advocated about fifty years ago to explain the occurrence of gold in some plates of Muntz metal that had been used as the sheathing of a ship, which had been trading for three years in the Pacific. It is true that plates of Muntz metal fresh from the works also contained a trace of gold. But the amount in the plates that had been exposed to the sea was larger than in new plates, and the excess was explained as due to gold electrolytically deposited from sea water. This view appeared the more plausible when, in 1872, Sonstedt announced that he had detected the presence of gold in minute quantities in the water of the Irish Sea. Professor Liversidge of Sydney, who has followed up this question with greater perseverance than any other chemist of equal distinction, has detected from $\frac{1}{2}$ to $1\frac{1}{2}$ grains of gold to the ton of sea water from samples collected on the coasts of Australia, and in the middle of the Indian and Pacific Oceans.

A grain of gold dissolved in a ton of sea water is not a very large amount. One grain per ton is about 0·0000006 per cent. And condensed sea-salts, even if none of the gold were lost in evaporation, would have but one grain of gold in 78 lb. of sea-salt, or one part in 546,000. To detect these amounts requires chemicals of wonderful purity and chemists of the highest skill. It is not, therefore, surprising that some chemists have failed to find gold in the sea, or

have found it in far smaller quantities than Professor Liversidge has obtained.

But if gold be present in sea water to the amount of only a grain to the ton, the gross amount is temptingly large, for it has been calculated that at that rate, there is enough gold in the sea to supply every human being with the useless dowry of 75 tons of gold.

Geologists accept the presence of gold in the sea on the authority of the chemists. But that the sea is the source of the gold in our lodes is a very different matter. The amount of gold in the sea is so small that it does not appear to follow the ordinary processes of precipitation. Gold is a metal that is very readily precipitated from solution. It is most likely present in the sea as some chloride, and gold is thrown out of chloride solutions by the action of light in the presence of organic matter. But some careful analyses of mud from the sea-floor, at positions where gold should be steadily precipitated by these agencies, have shown no trace of it.

Moreover, the essays of Dr. Don, confirmed by those of Mr. W. H. Bailey in the laboratory of the Mines Department of Victoria, have shown that the slates, from which the quartz lodes were supposed to have got their gold, contain none, except when they also contain pyrites: and the microscope proves that the pyrites was formed after the rock, and the gold was doubtless introduced simultaneously with the formation of the pyrites.

It is, in fact, far more probable that whatever gold the sea may contain was dissolved from the land, than that the gold on the land was derived by precipitation from the sea.

Igneous Rocks as a Source of Ores.

The igneous rocks are natural sources of iron and its allied metals, for iron is an essential constituent, often to a considerable extent, of the basic igneous rocks. Iron, moreover, is a material which, in the ample time available for ore formation, is easily rendered soluble by natural agencies. Hence the weathering of basic igneous rocks gives an ample supply of iron. It is removed in solution and re-deposited in the films of rust, which colour red sandstones, and yellow sands and loams. Iron is the most widely diffused of all colouring matters; but rocks in which it may be conspicuous as a pigment, may be of no use as an ore. A deposit is no commercial value as an iron ore unless it contains at least 30, and usually from 50 to 60, per cent. of iron. But the agencies that have leached out the iron from igneous rocks, and with it painted our landscapes, continue their action, and concentrate the iron as a serviceable ore.

Thus, the precipitation of iron carbonate by organic agencies in swamps, in lagoons, and along muddy shores, forms beds of clay ironstone, such as the black band ironstones associated with our coal

measures, the marlstones of the Lias, and the ores which in Roman and mediæval times, maintained the iron industry of the Weald.

Such ores are deposited contemporaneously with the rocks in which they occur; but other iron ores have been formed by the infiltration of iron salts into pre-existing rocks. Thus have been formed the masses of valuable kidney iron ore in the limestones of the North of England. They have been formed where a bed of limestone is covered by a sheet of sandstone stained red by iron oxide. Rain water, charged with carbonic acid from the air, or with organic acids derived from the decay of plants, percolates through the red sandstones, removes the iron as a soluble carbonate, and carries it down into the underlying limestone. Here, the soluble iron salt is acted on by the limestone, some of which is removed in solution, and iron oxide precipitated in its place; and thus, the masses of kidney ore are slowly formed, and grow at the expense of the surrounding limestone, as descending waters nourish them with fresh supplies of iron.

The distribution of such ores is limited by two factors. They are limited in area to localities where there was once an overlying, permeable, iron-stained bed to supply the iron; and they are limited in depth by the thickness of the limestone, and that to which water can descend through that rock, before the precipitation of all its iron.

The great deposits of hæmatite in the Lake Superior region, which have given the United States its supremacy in iron production, were, according to Van Hise, similarly formed by descending oxygenated solutions, carrying down iron from beds of iron carbonate. As the oxygenated waters necessary for this process cannot descend to any great depth, it is probable that the main mass of these ores will be limited to the depth of 1000 feet.

The beds of clay ironstone formed as contemporary deposits, may on the other hand go down to great depths, when sunk in folds beneath thick beds of overlying sediments. The depth of such ores is a simple problem in stratigraphical geology, and the limit to which they can be worked is determined only by the cost of mining.

These iron ores derive their materials from igneous rocks indirectly: but there are some iron ores of direct igneous origin. They have been formed by the segregation of the metallic constituents of a molten magma into masses, or into bands along the cooling edges. The existence of such primary ores is indisputable; but their amount is still largely in doubt. They include masses of magnetite, and also ores of chromite, and according to some authorities, masses of copper pyrites, and of nickel-bearing pyrhotite, and even cassiterite. There is no reason why these segregations should not form at any depth at which molten rocks consolidate; for both the pressure and heat would facilitate their formation, by allowing the molten work to cool so slowly that the segregation can collect the metallic constituents from a vast bulk of rock. Such igneous segregations usually occur

in scattered patches ; and it is only where they have been formed on the cooling margins of the rock, that we have much clue to their distribution. But these ores are of secondary importance ; because iron ores do not pay to mine at great depths ; and it is only for iron ores that this igneous origin is conclusively established. There is no equal reason for regarding ores of such metals as copper, lead, zinc, gold, and silver as derived from igneous rocks.

The arguments for regarding igneous rocks as the source of ores in general, are their constant companionship and the frequent occurrence of traces of various metals in igneous rocks. Neither fact seems to me convincing. The frequent association of igneous rocks and ores is natural, for the intrusion of the igneous rocks supplies the heat, and makes the fractures necessary for ore formation. In regard to the second argument, it is true that minute traces of various metals have been detected in many igneous rocks, but usually in those of mining fields. It is very doubtful whether the metals are primary constituents. It is far more probable that they have been introduced after the consolidation of the rock, and at the time of the formation of the associated lodes.

The Barysphere as the Source of Ores.

For most ores, we are therefore driven back to the third possible source—the hidden interior of the earth, which is called the centrosphere from its position, and the barysphere on account of the weight of its constituents. This is an ample source of metals, for we may regard the earth as a vast ball of iron, hardened, like a modern projectile, by nickel, and no doubt charged with other metals. The rocks of the earth's crust are the stony slag given off from the cooling metallic mass. The one objection to this source is the great depth of the metallic barysphere ; the specific gravity of the rocks at the surface is about 2·5, and that of the earth as a whole about 5·0. At the depth of 100 miles, the specific gravity of the rocks is estimated to be about 2·8, or merely that of the basic igneous rocks. But in all probability the surface of the barysphere is not regular ; projections from it rise into the lithosphere, and the mining fields probably lie over these projections.

There are areas, such as the Scotch Highlands, traversed by faults of colossal size, and with the rocks intensely metamorphosed and charged with quartz veins, but these veins are not, to use the usual phrase, mineralised ; there are enough quartz veins to maintain a large mining field ; but, with insignificant exceptions, they are barren. The barysphere may be so far below, that the quartz veins have not been charged with metals ; whereas in other places, owing to the barysphere being nearer the surface, the veins received metallic, as well as non-metallic, minerals.

The Transporting Agents.

Accepting the barysphere as the source of metals, we have to consider the possible agencies by which they have been raised to their present level.

Iron has been brought to the surface of the earth by the intrusion of igneous rocks from the deeper parts of the lithosphere ; and some quartz veins are also of igneous origin, having been formed as intrusions of molten rock. But these igneous quartz veins are barren, probably because they were too hot to be a suitable medium for the deposition of metals.

The great majority of ores have been formed by percolating water, as is shown by their arrangement in mass, by their microscopic structure, and by the minerals associated with them.

The introduction of ores in solution is indicated by what is known as " crustification "—their deposition in layers along the two walls of an ore-filled fissure ; also by the frequent occurrence of geodes—cavities filled by the slow infiltration of material through cracks ; also by the association of ores with such minerals as calcite, which are of aqueous origin, and the rarity of the typical minerals of igneous rocks, such as the felspars and the pyroxenes ; and when lodes contain minerals, such as quartz or mica, common to both igneous and sedimentary rocks, the varieties present are those deposited from solution or formed by the re-crystallisation of materials by crushing along fractures. Ores, of course, occur in igneous rocks ; but in such cases they have usually been introduced in solution, and have replaced the original minerals.

The Descent of Meteoric Water.

The formation of most ores by water is now universally accepted, so their distribution in depth is controlled by the distribution of underground water. There are two sources of subterranean water. The first is the rain, which supplies the meteoric water. Some of this water runs off in rivers to the sea ; another part of it, sometimes the whole, is restored to the air by evaporation, and the rest of it disappears by percolation underground.

The amount of this percolation seems to me to have been greatly exaggerated in the past. The amount lost by evaporation has been underestimated, and the rocks have, therefore, been regarded as far more permeable than they really appear to be. This exaggerated estimate of the descent of the surface waters has been due in the main to three ideas :—

1. An underestimate of the amount of water available from other sources for the supply of deep wells. It was necessary to believe that a large percentage of the rain must percolate underground, and that rocks are very permeable, so long as the deep artesian wells of arid

regions were regarded as maintained by the rainfall on the distant hills, as when the flowing wells of lower Egypt were held to be nourished from the mountains of Darfur, and those of Central Australia by the rains on the distant Queensland Hills.

2. That water could descend by capillary action to great depths, even against high internal pressure, was maintained on the basis of an experiment by Daubrée. He inverted a vessel of water over a slab of porous rock, forming the roof of a cistern filled with high pressure steam. The water percolated downward in spite of the upward pressure of the steam; and hence it was claimed that capillarity could suck water downward against the resistance due to the earth's internal heat. But this experiment, as shown by Fisher and Kemp, proves nothing, as the movement was toward a free air space, and there is no free air space in the earth, corresponding to the cistern in Daubrée's experiment.

3. The third argument for the descent of the surface waters to great depths is based on the hydrodynamic principle, invoked by Van Hise, that a current of water will use the whole area of any channel open to it. If a stream of water be allowed to enter a deep trough, the current does not flow in a straight line from the point of inflow to the exit, but it slowly spreads downwards and upwards till the whole of the water in the trough takes part in the movement. Similarly, according to Van Hise, water percolating downwards at one place and re-discharged elsewhere in springs or wells is not confined to the direct line between the two places, but it spreads sideways through a great width of country and sinks downwards to the greatest available depth. Hence, meteoric water should be universally diffused through a zone at a shallow depth below the surface. According to this conception the whole of one zone of the earth's crust is saturated by a great subterranean sea; and as its waters would be superheated, they would have great solvent powers, and being universally diffused, they would come in contact with all the metallic grains that may occur scattered through this zone of the earth's crust.

But the universal existence of this subterranean sea, seems to be abundantly disproved by the evidence of many deep mines and wells. In Bendigo, e.g., the ground water is confined to the surface zone, below which the rocks are dry, except where the levels happen to tap a spring of hot alkaline waters, which are probably of plutonic and not of meteoric origin. They are waters given off from the cooling magma of the earth, and belong to the same category as the fluids that, scattered in innumerable microscopic cavities, give quartz its milky whiteness and form the vast steam clouds, when, through a volcano, molten rocks reach the surface of the earth. These hot waters are, therefore, sometimes called magmatic on account of their origin from the rock magma of the interior, or juvenile, by Professor Suess, as they are making their first appearance on the earth's surface, or plutonic from their deep-seated origin.

This plutonic water answers all requirements for a depositor of ores. It is coming from the proximity of the metalliferous interior of the earth: it is superheated water—so it is a ready solvent of materials which are insoluble in a less heated water; and it is usually alkaline, and therefore is the readiest solvent of metallic sulphides—the primary condition of the vast majority of ores.

Changes in Plutonic Waters during their Ascent.

From whichever source the water comes, it is obvious that aqueously deposited ores must be limited to the depth at which water exists and works; and the range of ores of economic value depends upon the conditions which govern the precipitation of metals from underground solutions. Water, although its constituents may come from vast depths within the interior, is limited to a depth of perhaps only six or seven times the depth of existing mines. The lower limit is due to the internal heat of the globe, which increases from the surface at a rate which may be taken as 1° F. for every 55 feet of descent. This is the traditionally accepted average; and the conditions are too uncertain to make it worth while at present to re-average it. Now water cannot exist at a temperature higher than its critical point, 687° F. At the assumed rate of increase, that temperature would occur at an average depth of about 37,000 feet; the depth is probably much less in the neighbourhood of igneous rocks and in areas of recent volcanic action; and it may be much lower in ancient rocks in areas which have long remained geologically undisturbed, as e.g. in the Transvaal.

The figures that can be quoted are only rough approximations, and they will vary in different fields according to the conditions of increase of underground temperature.

At depths below about 37,000 feet, the temperature would be above the critical point of water, which therefore could not exist as such. Its elements would be given forth as separate gases from the slowly cooling magma; the gases would rise, and having passed into a zone with a temperature below the critical, would combine to form water. This water would be at temperatures far above the normal boiling point; but it would be kept liquid by the immense pressure of the overlying rocks. This water, owing to its tension, would tend to force its way to the cooler areas through any lines of passage open to it. They will be minute capillary passages, as nothing else could exist at such depths. Nearer the surface the pressure is less, so the spaces will gradually increase in size, and in them the water will collect and continue its upward flow. At length, it would reach a level, somewhere about the average depth of 18,000 to 20,000 feet, where the temperature would be about 400° F., at which water has its maximum capacity for solution; up to that level the water will

be steadily dissolving materials, but above it the water will begin to deposit the material, which it has brought with it from greater depths. The water has left the zone of accumulation, and has reached that of deposition.

According to the mode of deposition, there are three main types of ores :—

1. The steady ascent of the solution carries it continuously to regions of less and less pressure and of lower temperatures. Both changes favour deposition, and as the changes both in pressure and temperature are gradual, the material will be deposited continuously, and fairly uniformly over a great vertical range.

2. A more rapid and patchy deposition occurs where the solution happens to come into contact with some rock, such as limestone, which decomposes the dissolved salts, and leads to the immediate precipitation of the metals, probably as sulphides.

3. When the solution approaches the surface, the rate of cooling is accelerated, and it may be mixed with surface waters ; and thus the balance of the metals is quickly precipitated, and a high-grade ore results owing to its deposition in a narrow range.

Hence most lodes have their richest ore bodies on the surface, for the agencies that lead to ore formation tend to rapid precipitation there, as the conditions are more complex and the variations in conditions more sudden.

Surface Enrichment—Primary and Secondary.

This primary richness of the outcrop is re-enforced by a secondary enrichment, to which the richest bonanzas and prizes of the mining industry are due. The secondary enrichment is, from a mining point of view, and especially in the valuation of a mining field, the more important process. Its nature may be illustrated by reference to a gold quartz vein, formed by an ascending solution, which had deposited gold uniformly in a vertical lode ; if the ground containing the lode is slowly lowered by denudation, the gold in the uppermost part of the lode is dissolved by a solution formed by the action of surface waters on the iron pyrites in the lode. The gold is carried a stage lower, where the ferrous sulphate is reduced and gold and pyrites re-deposited. So the cap of the lode now has concentrated within it the metals originally distributed through twice the length. The process is repeated again and again until a rich gold patch, such as the famous Londonderry pocket, may cap an otherwise worthless lode.

It is as an index of the extent to which ores have been thus secondarily enriched that the mineralogical study of lodes is of high economic importance.

All ore formation is essentially a process of concentration ; the specific gravity of ordinary ores may be taken as 3·4 ; the rocks of

the lithosphere have a specific gravity of 2·5 ; the barysphere must contain masses of a specific gravity of at least 6 and 7, and the average specific gravity of the average zone at the depth of 37,000 feet will be only 2·52. But as ore formation probably takes place where the barysphere is nearer the surface, its specific gravity at the critical temperature of water beneath a mining field in process of formation will probably be higher. But it can hardly be more than 3·0. So the metals in this zone of the crust will be more diffused than in a lode. The process of ore formation is the collection of the scattered particles, their concentration in a lode, and then the sorting out of the mixed, complex, primary constituents into purer, simpler, secondary minerals.

Thus, gold in a deep lode is always alloyed with silver, and probably with lead, zinc, and antimony as well. It is only near the surface that gold occurs with only 3 parts in the 1000 of silver ; there are veins of native metals.

Similarly with silver : its deep ores are always alloyed with lead. It occurs as native silver and as horn-silver only in shallow ores.

Again, with copper, the primary ores are complex sulphides combined with iron. Thus the commonest primary ore is of copper chalcopyrite with 34·5 per cent. of copper, and 30·5 per cent. of iron ; and the mineral occurs as streaks in iron pyrites, so that the copper is often only 1 or 2 per cent. of the ore. This material is attacked, its constituents dissolved and re-deposited, probably as veins of bornite, which contains 55·5 per cent. of copper, and only 16·4 per cent. of iron ; and bornite often occurs in comparatively pure veins, the great bulk of the iron having been deposited apart as veins of hæmatite. The secondary bornite is then in turn attacked, and the copper re-deposited as the tertiary minerals, covellite, with 66·4 per cent. of copper, or cuprite, with 88·8 per cent. and no iron ; and in the last stage in the process the copper occurs in veins or masses of native copper.

The depths to which ores will descend depend on the factors that control both their deposition and concentration. In considering the future of any mining field, the question of primary importance is whether the ores have been deposited by ascending or descending solutions, and we are beginning to acquire sufficient knowledge to form some idea as to the probable depths to which various types of ore-deposits will go.

Thus, there is no reason why gold quartz lodes should not go down to depths of about 18,000 feet below the level of the surface at the time of their formation. Hence, in estimating the depth to which any particular lode will go, we have to consider how much of the upper part of the lode may have been destroyed by denudation. In two adjacent mining fields, in one the lodes may continue to be auriferous to the depth of 5000 or 7000 feet from the present surface, whereas, in the other, where the lodes were formed in precisely the same way,

the ore shoots may be quite shallow. The disappointing result in the second case is due to the upper part of the lodes having been removed by denudation, so that only the barren roots remain.

To plunge into deep mining regardless of the geological structure and history of the field is to invite failure. And deep mining to be profitable must be so organised as to work low grade ores economically, and to reduce dead work to a minimum by a plan of mining adapted to the geological structure of the field.

Gold mines worked thus at Bendigo have already reached the depth of 4250 feet, and the success there is the more remarkable as the mines are not working continuous lodes, but isolated masses of quartz. The reefs are saddle-shaped folds of quartz, floating in folded beds of slate and quartzite. It was not until the clue to the distribution of these isolated quartz masses was discovered that there was any chance of working the mines deeply. And though the field shows the usual decrease in grade, there is no indication that the lowest reefs have been approached.

In some fields the mines will be doubtless more shallow. Thus ores in lavas and horizontal stratified rocks, whether formed as contact deposits near igneous intrusions, or along fault planes and fractures, up to which hot solutions have ascended, are likely to be patchy, owing to the sensitiveness with which such liquids respond to slight variations in the conditions. The depths of the ores will be directly determined by the thickness of the stratified rocks, and this is usually a simple problem of field geology. There is less probability of the ores continuing to as great a depth, as in infolded uptilted masses of ancient rocks.

The vast masses of pyritic ores represent a third type, which is of a special historic interest. They were at one time regarded, as they still are by some geologists, as bedded ores, precipitated in ancient lakes, by the action of decomposing organic matter, on materials introduced by streams. The ores were expected to continue to vast depths, because they are often hundreds of feet in width, and the horizontal extent of a sedimentary deposit is usually dozens, and may be hundreds, of times greater than its vertical thickness. So the depths of the ore-body was expected to be many times its thickness, and as one lenticular ore-deposit thinned out, others were expected to come in at slightly different levels. But these pyritic ore-masses have been found to be comparatively shallow in depth ; none has yet been worked to the depth of 1000 feet, and for their shallowness there are excellent reasons ; they are secondary deposits, formed by ore-bearing solutions replacing, particle by particle, shattered masses of rocks. The replacement has been so slow that the aspect of the original bedding is often still preserved, leading to the view that the pyrites itself was a bedded deposit. The shattered rock masses, necessary for the formation of these huge ore-bodies, can only occur near the surface ; for at greater depths the weight of the overlying rocks

would make the softer finer material flow into the interspaces between the harder fragments; the very force that fractured the rocks would also compress them, so as to render the crushed mass impermeable. Solutions might work their way along fractures, depositing ordinary fissure-veins, and replacement-veins on their edges; but there will be nothing like the widespread permeation which has produced the great pyritic ore-bodies. It is only natural that in these cases the mines should not reach the same depths as ores deposited along the channels of solutions escaping from plutonic depths.

Shallow and Deep Ore Types.

The distribution of ores in depth may, therefore, be usually inferred from a combined study of the geological structure of a mining field and the mineralogical characters of its ores. A definite conclusion is not always possible, as in the absence of these data, or when dealing with new types of ore-deposits. But in ordinary cases a general opinion can be expressed.

Thus there are three groups of ore-deposits which must be expected to have comparatively shallow limits. They are :—

1. Ores that have been deposited in fissures and open spaces, which cannot remain open at great depths. Hence, such ores if continued deeper, will be continued by ores of different character.

2. Ore-masses formed by the replacement of blocks of shattered rocks, because masses of crushed rocks at great depths would be closed by the flowing of the softer materials, and thus be impermeable to all ore-bearing solutions.

3. Ores formed by precipitation from descending solutions, including nuggets, bonanzas, and the various types of secondary enrichments, and also many ironstone masses and beds.

On the other hand there are three types of ore-deposits which may persist to great depths. They are :—

1. Lodes characterised mineralogically by low-grade alloys, and complex sulphides with small percentages of the more valuable metals, and geologically by their occurrence as replacement veins along great fault planes, or as isolated bodies along lines of special permeability in folded rocks.

The limit in depth of such ores is the level of maximum saturation, which must occur as a rule at the depth of about 18,000 feet, for at that depth the ascending plutonic waters must begin to deposit their metallic salts.

2. A second group of ores with a possible great extension in depth are those of sedimentary origin, in tilted rock masses; for they can extend as deeply as such rocks can be buried without being metamorphosed by the earth's internal heat.

3. The third group of possibly deep seated ores are those formed by the segregation of metallic minerals from molten rock magmas.

These three groups of ores hold reserves of ore, the deep working of which is controlled by the elastic limits of expense. The assumed boundary of practical mining is being continually pushed downward. Once the final limit was accepted as 3000 feet, but there are already gold mines working at the depth of 4250 feet, and copper mines at 5000 feet, and preparations are being made for mining at 6000 feet. The greatest obstacle to deep mining is the high temperature ; but that can be reduced by ventilation with compressed air or by the circulation of cold brine. It is unnecessary to suggest the powers given to the miner by liquid air. It gives him means of cooling and ventilation to which it is difficult to set a limit. There can be no doubt that the ore-deposits below the present limits of mining, for the existence of which the geologist can safely vouch, will one day be accessible. The profitable working of these deeper ores will require the close co-operation of the engineer and the geologist. Then the miner, armed with the invincible ingenuity of the engineer, and guided by the insight of the geologist, will follow the ore-shoots, in their irregular courses, into ever deeper layers of the earth's crust, and wrest from them their long hidden secrets and their well buried stores of wealth.

[J. W. G.]

ANNUAL MEETING,

Tuesday, May 1, 1906.

THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the chair.

The Annual Report of the Committee of Visitors for the year 1905, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Forty-five new Members were elected in 1905.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1905.

The Books and Pamphlets presented in 1905 amounted to about 254 volumes, making, with 697 volumes (including Periodicals bound) purchased by the Managers, a total of 951 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :

PRESIDENT—The Duke of Northumberland, K.G. D.C.L. F.R.S.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
SECRETARY—Sir William Crookes, D.Sc. F.R.S.

MANAGERS.

Sir William de W. Abney, K.C.B. D.C.L. D.Sc. F.R.S.
The Right Hon. Lord Alverstone, G.C.M.G. P.C. M.A. LL.D. F.R.S.
The Right Hon Earl Cathcart, D.L. J.P.
Arthur Herbert Church, Esq., M.A. D.Sc. F.R.S.
Francis Elgar, Esq., LL.D. F.R.S. M.Inst.C.E.
Donald William Charles Hood, M.D. C.V.O. F.R.C.P.
Maures Horner, Esq., J.P. F.R.A.S.
Sir William Huggins, O.M. K.C.B. D.C.L. LL.D. F.R.S.
The Right Hon. Lord Kelvin, O.M. G.C.V.O. P.C. D.C.L. LL.D. D.Sc. F.R.S.
Henry Francis Makins, Esq., F.R.G.S.
Ludwig Mond. Esq., Ph.D. F.R.S.
Sir Richard Douglas Powell, Bart. K.C.V.O. M.D. LL.D. Pres. R.C.P.
The Right Hon. Lord Sanderson, G.C.B. K.C.M.G.

MANAGERS—*continued.*

Alexander Siemens, Esq. M.Inst.C.E.
The Right Hon. Sir James Stirling, P.C. M.A. LL.D. F.R.S.

VISITORS.

John Mitchell Bruce, Esq. M.A. M.D.LL.D.
Dugald Clerk, Esq., M.Inst.C.E. F.C.S.
Sir John George Craggs, M.V.O.
Henry Cunynghame, Esq., C.B. M.A.
George Frederick Deacon, Esq. LL.D. M.Inst.C.E.
Edward Dent, Esq., M.A.
Robert Kaye Gray, Esq., M.Inst.C.E.
The Rev. John Henry Ellis, M.A.
Charles Edward Groves, Esq. F.R.S.
Frederick G. Henriques, Esq.
Alexander Ionides, Esq.
Carl E. Melchers, Esq.
Emile R. Merton, Esq.
Harold Swithinbank, Esq., J.P. F.R.G.S.
George Philip Willoughby, Esq., J.P.

WEEKLY EVENING MEETING,

Friday, May 4, 1906.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

The Hon. Charles A. Parsons, C.B. M.A. D.Sc. F.R.S. *M.R.I.*

The Steam Turbine on Land and at Sea.

It was with some diffidence that I accepted the subject of Steam Turbines on Land and at Sea for this evening's lecture, for since I had the privilege of dealing with this subject six years ago in this room, there seemed to me to be very little new to add, either from a scientific or a practical point of view, which had not then been to some extent considered. However, after consideration, there seemed to be a hope that an account of some further developments during the last six years on land and on sea, and a more extended description of the mechanics of the turbine and its applications, might prove of some interest, in view of the more general adoption of the turbine principle for the generation of electricity, for the propulsion of vessels, and for driving air-compressors, fans, and pumps.

Six years ago there were 75,000 horse-power of turbines on land, and 25,000 on sea. At the present time there are more than two million horse-power at work on land, and 800,000 horse-power at work or building for use at sea.

There are at present afloat, equipped with turbines—

3 Pleasure steamers.	6 Yachts.
9 Cross-channel steamers.	3 Destroyers.
5 Ocean-going vessels.	2 Cruisers.
3 Atlantic liners.	

Yet it cannot be said that the turbine engine is superseding the reciprocating engine generally, although this is undoubtedly to some extent the case in certain fields of work.

On land, the chief application of the turbine is found in large electrical generating stations, and its adoption in preference to the piston engine, in its most perfect development of compound, triple, or quadruple expansion engine, is becoming general in this field of work.

At sea, its use is commencing to extend for all the larger and

faster class of ships; for cross-channel steamers it has found great favour, and for Atlantic liners and ships of war it is being used to a more and more considerable extent, and this tendency is not confined alone to England, but is shown also on the Continent, and in the United States and Japan. It will give a clearer idea of the subject if we first of all examine more closely the characteristics of the steam turbine, and generally how it works.

All turbines derive their power from the impact of the steam, or, more correctly speaking, from the momentum of the steam, flowing through them, just as a windmill receives its power from the wind.

There are three principal types of turbines now in general use, as well as some which may be described as admixtures of these three classes. They differ essentially in some respects, more particularly in their methods of extracting the power from the steam.

The first to receive commercial application, 1884, was the compound or multiple expansion steam turbine; the second was the De Laval or single-bucket wheel, in 1888, driven by the expanding steam jet; and, lastly, the Curtis turbine, in 1896, which comprises some of the principal features of the others combined with a sinuous treatment of the steam.

In the compound turbine, the steam is caused to flow through a series of many turbine elements of gradually increasing size, graduated so as to allow of the expansion of steam in small increments of volume at each element, these increments of volume corresponding to the fall of pressure necessary to cause the steam to flow through each element. Each element consists of a row of guide blades and a row of moving blades. The guide blades are attached in circumferential rows to the case and project inwardly, and the moving blades are attached in rows to a drum and project outwardly. The ends of the blades throughout the turbine nearly touch the drum and case respectively.

To form some idea of the forces at work in a turbine we should consider, with approximate accuracy, that the steam flows through the turbine with a force about ten times as great as that of the strongest hurricane; and though the force acting on each blade is small, perhaps only a few ounces, or in the largest only a few pounds, yet in the aggregate the force is great and can propel large ships or drive large dynamos.

The important factors upon which the proportions of the turbine are based are the pressures, velocities and percentages of moisture in the steam, as it gradually expands from turbine row to turbine row.

The blades of the turbine are made of rolled and drawn brass, well shaped, and polished so as to reduce the frictional losses in the steam to a minimum. The steam enters all round the shaft and first traverses the shortest blades on the smallest drum, then through larger and larger blades set on larger and larger drums, and so on till as it leaves the last blades it is expanded about 100-fold in volume. At the opposite end to the blade drums are seen the balance pistons,

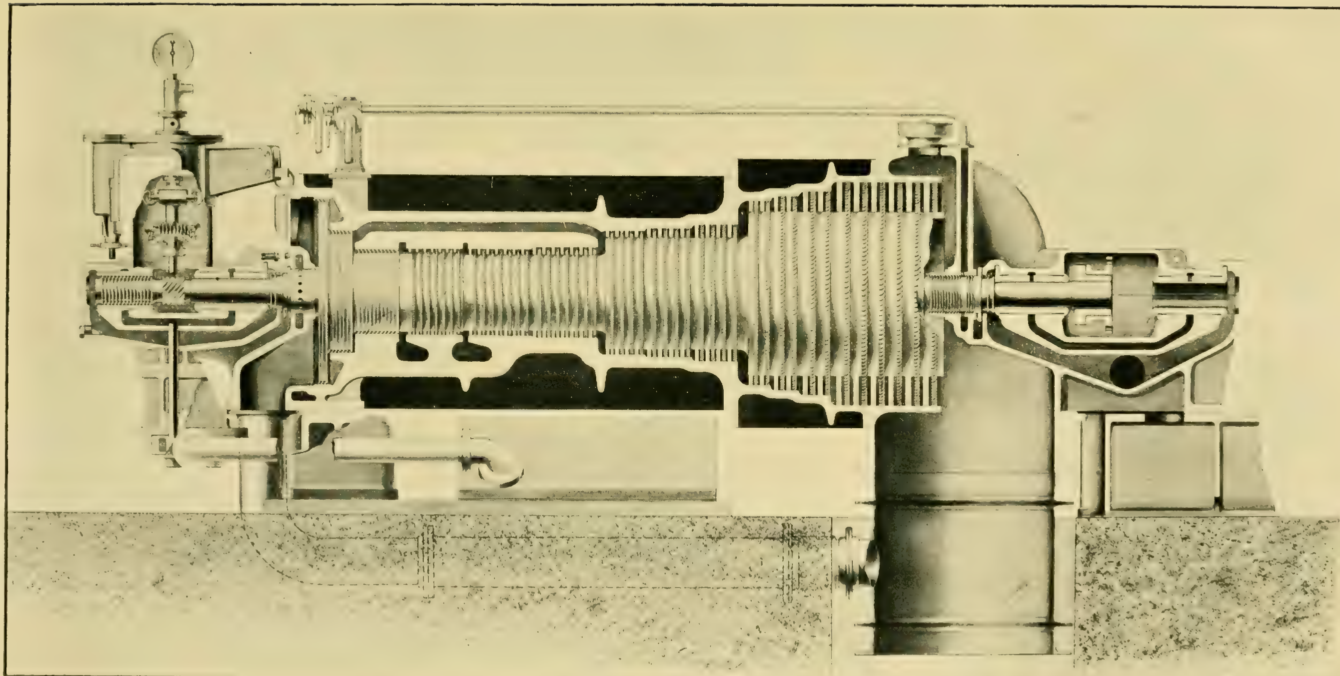


FIG. 1.—SECTION THROUGH COMPOUND STEAM TURBINE.

or dummy drums, which serve to balance the end pressure of the steam, and are kept steam-tight with the casing by packing grooves on the dummy drums which rotate in close proximity to corresponding but stationary brass rings keyed into the case.

In land turbines, for driving dynamos or other fast moving machinery, no end-pressure on the shaft is required, nor is it permissible because of the mechanical difficulties met with in thrust-bearings carrying heavy end-pressure and rotating at high speed, and therefore balance pistons are provided, which, while being practically steam-tight, serve to balance all end-pressure arising from the steam acting upon the rotating barrels and vanes.

In marine turbines, on the other hand, the dummy drums are so proportioned as to leave an unbalanced end-pressure, which counteracts and balances the thrust of the propeller, thus relieving the thrust-bearing from pressure.

The bearings of the engine, it will be seen, have only to support the weight of the rotating part of the engine ; this is comparatively small, and as continuous lubrication is provided by an oil pump which circulates the oil continuously through the journals round and round, there is practically no wear, even after years of continuous work ; and the maintenance of the shaft in a truly central position relatively to the casing, which is of great importance, is easily maintained in practice.

Before proceeding further with the examination of the compound steam turbine, let us consider the De Laval steam turbine introduced by Dr. De Laval of Stockholm in 1888.

In this turbine the steam at full pressure issues from a diverging conical jet, so formed and proportioned that the steam after passing through the neck of the jet enters a gradually divergent passage of increasing cross-section, in which it expands ; the result being that nearly the whole available energy in the steam is utilised in imparting to it a very high velocity, reaching, with 100 lb. boiler pressure and a good vacuum, as much as 4200 feet per second, and the discovery of this property of the expanding jet is due chiefly to Dr. De Laval.

This rapidly moving column of expanded steam is directed against cupped steel buckets on the periphery of a wheel made of the strongest steel, the wheel being shaped so as to permit of the highest peripheral velocity consistent with safety, which may be from 800 to 1200 feet per second ; the steam, by striking the cups and reacting, partly by velocity of flow and partly by elastic gaseous rebound from the concave surface of the cups, leaves the wheel with a considerable backward velocity, and to obtain the highest efficiency it is necessary to reduce this backward velocity by increasing the velocity of the wheel to the uttermost. The strongest materials, however, do not permit of a close approach to the speed necessary for the maximum efficiency ; yet in this turbine, owing to the comparative absence of losses, which are present to some extent in the other types (and which we will consider

presently), the efficiency of this turbine compares favourably for moderate and small powers.

In this beautiful construction, developed with mechanical skill and guided by an intimate acquaintance with the properties of steam and materials, there are many minor features of interest. Among them may be mentioned the elastic shaft, to permit of the rotation of the turbine wheel about its dynamic axis. A device, consisting of frictional damping washers, which had the same purpose as this elastic shaft, was used in 1885 in the early development of the compound steam turbine. It was superseded in 1892 by the damping effect of thin films of oil between several concentric loosely-fitting tubes surrounding the bearings.

The De Laval turbine has for many years been extensively used on the Continent and in this country, in sizes up to about 400 horse-power. Its chief use has been for the driving of dynamos, pumps, fans, and motive power generally; and, owing to its very high angular speed, it is necessary in most cases to use gearing, except when driving very fast-running centrifugal pumps and fans.

The gearing is of steel, and it is accurately cut with very fine spiral teeth, and it works satisfactorily even at the speed of 30,000 revolutions per minute.

Let us now consider the Curtis turbine. It ranks in a class by itself, because it comprises the principle of the sinuous treatment of expanded steam first put into extended commercial use by Mr. Curtis under the auspices of the General Electric Company of America.

This sinuous treatment of the steam consists in giving to it a high initial velocity by passing it through a jet of the De Laval type, or a group of such jets; it then impinges on a ring of bucket-blades like those used by De Laval, and after leaving the first row of such blades it is caught by a ring or a sector of stationary bucket-blades set in the reverse direction, and by them its direction is changed into that of the next succeeding row of moving blades (there may be three rows of moving blades in all and two sectors of fixed blades); and the height of each succeeding row is increased, to allow a greater area for the steam as it flags in velocity after each rebound between the moving and fixed blades.

The object of this treatment is to transfer a large percentage of the kinetic energy of the rapidly moving steam to the moving blades and wheel, without the necessity of very high peripheral speeds of blades, such as are necessary with the single-wheel type. As regards, however, "multiple series action," the principle resembles the compound turbine.

The expansion process in nozzles, and subsequent sinuous treatment of the steam, is repeated several times by four or more similar wheels on the same axis, but in separate steam-tight chambers, until the steam is fully expanded.

If there are four such operations, the velocity of outflow from the

nozzles will be about 2000 feet per second, and the peripheral velocity of wheel about 400 feet per second; and at each operation the steam is expanded through one-fourth of the whole range, and at each it is brought to rest before flowing to the next chamber through the jets.

A great many other varieties of the turbine have been proposed, and some have received a limited application. The Rateau, the Reidler Stumpf, the Zoelly, the Escher Wyss, and many others, might be mentioned as varieties of the three fundamental turbines we have considered; indeed in some cases the variation would appear to have been only a retrograde step, and represents some discarded form tried by one of the originators of the three fundamental types.

As far as we can gather from the history of the steam turbine, it may be said broadly that all the chief features at present in use in turbines have been suggested or described in the rough by experimenters long ago in the hundred and more patents prior to 1880.

For instance, Hero of Alexandria, B.C. 130, made a reaction wheel.

William Gilmore first suggested the compound steam turbine in 1837.

Matthew Heath first enunciated the principle of the diverging conical jet in 1838.

James Pilbrow in 1842 used cupped buckets, and suggested a sinuous treatment of the steam.

Robert Wilson developed the compound steam turbine to a considerable extent in 1848.

It would take too long to trace the initiation of each idea, but we may say, in the light of recent experience, that most, if not all, the designs showed a want of knowledge of the properties of steam and materials, and could not have given a satisfactory performance.

Let us again recur to the compound turbine, and look more closely into the principles of its working, and more particularly consider the course of the steam in its passage through the vanes or blades of the engine.

Viewing the turbine as a whole we see that the steam passes through the forest of fixed and moving blades just as water flows from a lake of higher level through a series of rapids and intervening pools to a lake of lower level. The boiler corresponding to the lake of higher level and the condenser to that of lower level.

In the flow through the turbine the steam is repeatedly gathering a little velocity from the small falls of pressure, which is as soon checked and its energy transferred to the blades, over and over again; 50 to 100 times is this repeated before it is fully expanded and escapes into the condenser.

The number of blades in a steam turbine is very great; in a 2000 horse-power engine it may be from 20,000 to 50,000 and the surface speed of the several barrels of the turbine will be from 150 to 300 feet per second. In such an engine it is arranged that the lineal

velocity of the blades will approximate to one-half that of the tangential component of the steam issuing from the guide blades. The blades, as we have seen, are curved, with thickened backs, and are smooth; the steam therefore flows around them, and past them, without much loss by shock or eddy current or frictional loss. The proportions of turbines as regards diameter, height of blade, and blade openings, are calculated so that, under average working conditions, the correct expansion of the steam shall be attained, and the fall in pressure and velocity of steam at each turbine of the series shall be such as to secure for it the highest efficiency.

When a turbine is tested the pressures at many points along the barrel are recorded, and the calculated pressures confirmed and verified by experiment, and these are usually in close accord. As the result of data accumulated from experiments on many turbines, the probable horse-power that will be obtained from a given design of turbine can be predicted with as much accuracy as in the case of the reciprocating engine. The best results that have been obtained from large turbines show that about 70 per cent. of the available energy in the steam is converted into brake horse-power; and where, we may inquire, has the other 30 per cent. gone?

The chief losses of efficiency in all steam turbines are due to three principal causes: firstly, to skin-friction of the steam coursing at high temperature through the small openings between the blades; secondly, to unavoidable leakages; and, thirdly, to eddy-current losses arising from insufficient blade velocity and errors of workmanship.

The first of these losses, the friction of the steam, is reduced by superheating, and thus partially removing the fluid frictional loss arising from the drops of condensed water mingled with the steam. In some cases this gain in efficiency is worth the extra cost of the superheater, but, unless intermediate superheaters are used, initial superheat cannot be raised high enough to maintain dryness throughout the major part of expansion without destroying the turbine. Moderate initial superheat, however, is generally used with some gain in economy, which in the compound turbine amounts to 1 per cent. for every 10° F. of superheat. The second loss, which is from leakage, is present in the compound and the sinuous types but not in the De Laval type. The amount of this loss decreases as the size of the engine increases. It is also chiefly consequent on the coefficient of expansion of metals, which is a bugbear to the turbine designer. If a metal with a much smaller coefficient of expansion than steel and iron could be obtained at a reasonable price and of suitable qualities for the construction of turbine cases, drums and shafts, a considerable increase of economy could be obtained, as it would allow of smaller working clearances and less leakage. The third loss, from insufficient blade-velocity, is not present to a material extent in the larger compound or sinuous course turbines, but is present, as already explained, to a considerable extent in the single-wheel type.

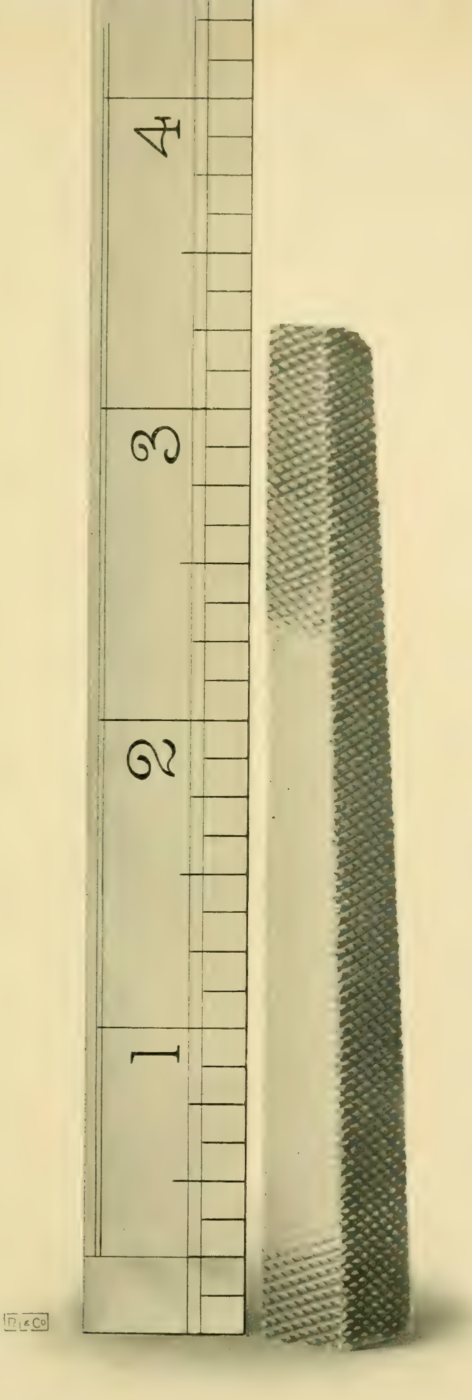
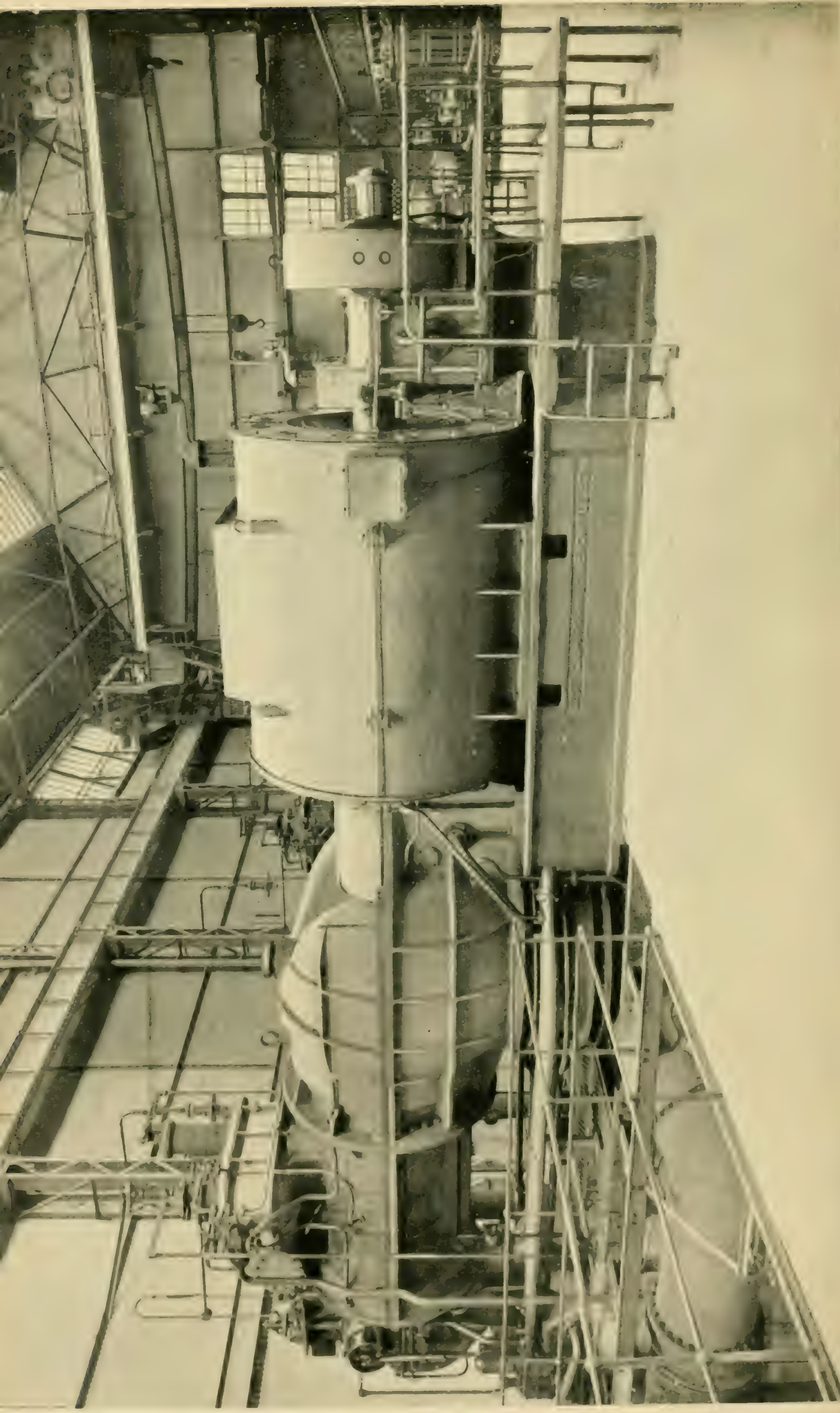


FIG. 2.—AN ENLARGED PHOTOGRAPH OF A HARDENED STEEL FILE, SHOWING THE DESTRUCTIVE ACTION OF STEAM AT HIGH VELOCITIES. This file was exposed for 145 hours to the action of a jet of steam at 100 lbs. pressure, discharging into a condenser pressure of 1 inch absolute of mercury.



Reviewing more closely the motion of the steam through the blades of a compound turbine, we see that the portion of its course during which it is travelling at relatively high velocity, and in close proximity to the blades, is short in comparision with the total length of its travel within the turbine. The passage-ways between the blades constitute virtually jets of rectangular cross section, but having easy curves, and the frictional losses are consequently small. After leaving the blades, it traverses the intervening space in the form of an annular cylinder with a spiral motion, the angle of pitch being about 30° to a plane normal to the axis: and, as the succeeding blades are moving in a similar direction to this flow, we see that the velocity with which the steam is cut by their frontal edges is much less—in fact, less than one half the velocity at which the steam has issued from the previous blades. From this we see how small is the loss due to the cutting of the steam by the frontal edges in the compound turbine, and also how small is the velocity with which drops of water strike the metal of the blades.

This is an important feature.

It has been shown by experiment that if drops of pure water, arising from the condensation of expanding steam, impinge on brass at a greater velocity than about 500 feet per second there results a slow wearing away of the metal. It is very slow, and would require about ten years to erode the surface to a depth of $\frac{1}{32}$ inch. In the compound turbine the striking-velocity is much below this figure, and the preservation of their form and smoothness of surface has been found to be practically indefinite.

It appears that the erosive power of drops of pure water moving at high velocity increases rapidly with the velocity, it may probably be as the square. Experiment has shown that if saturated steam at 100 lb. pressure be allowed to flow through a divergent jet into a good vacuum, attaining a velocity of about 4500 feet per second, and allowed to impinge on a stationary brass blade, the blade will be cut through in a few hours, and the hardest steel will be slowly eroded. The action seems to be the result of the intense local pressure from the bombardment of the drops, which may exceed 100 tons.

Owing to the receding velocity of the blades from the blast, and consequently reduced striking velocity, the erosion of the blades in impact turbines is much reduced, and in compound turbines there is complete immunity from such erosion.

It may be asked, how is it that the steam turbine in the larger sizes is more economical in steam per horse-power developed than the best triple or quadruple expansion reciprocating engine? The reason is, that all large steam turbines are able to take full advantage of the whole expansive energy of the steam, even when expanding to the very attenuated vapour densities produced by the best condensers. It is indeed easy to construct the low-pressure portion of the turbine to deal effectively with the very attenuated vapour, whereas the re-

ciprocating engine, from its nature, can only take full advantage of about two-thirds of the whole range of expansion, and is unable to deal usefully with very low vapour densities—the low-pressure cylinders cannot (because of structural difficulties) be made large enough, and the last part of the expansion has to be allowed to run to waste.

The growth in size of the turbine is perhaps interesting. The first practical steam turbine, constructed in 1884, was of 10 horse-power. By 1892 the largest size for driving dynamos had reached 200 horse-power. It has been continuously increasing, and has now reached 12,000 horse-power in one unit driving one alternating dynamo.

In 1894 the "Turbinia," of 2000 I.H.P. was commenced. The diagram, Fig. 5, shows her low-pressure and reversing turbine. The L.P. turbine is 3 feet in diameter.

The "King Edward" was built in 1902, 9300 I.H.P., and the diagram shows one of her L.P. turbines and reversing turbine in one casing, to the same scale.

In 1903 "The Queen," of 9000 I.H.P., commenced to ply between Dover and Calais. The diagram shows one of her L.P. and reversing turbines.

In 1905 the Allan liners "Virginian" and "Victorian," of 12,000 I.H.P., went on service between Liverpool and Canada. The diagram shows one of the L.P. and reversing turbines, which is 10 feet in diameter and 35 feet in length ; and in last December the "Carmania," of 30,000 tons displacement and 20,000 horse-power, commenced to ply between Liverpool and New York. The diagram shows her L.P. turbine, which is 14 feet in diameter.

The application of the turbine to the propulsion of vessels involved some interesting problems. The most important was, how slow could a turbine be made to rotate consistently with the maintenance of its efficiency in steam consumption, and at the same time be of moderate weight and cost ?

In the same problem naturally arose the question of how fast could a screw-propeller be made to revolve when propelling a vessel of a given size and at a given speed—in other words, when delivering a given propulsive horse-power at a given speed ? The first question as to designing a low-speed turbine was solved in 1894 to 1896, by the aid of the accumulation of accurate data from experiments on land turbines ; and the modification arrived at in the turbine has been chiefly directed to the splitting of it up into two or three or more turbines in series on the steam, and each working a separate shaft. This splitting up of the turbine results in a two-fold advantage. It makes the turbine (which otherwise would be very long) much shorter, and because of being shorter finer clearances and less loss by leakage results, and the whole engine is lightened. A secondary gain, resulting from the division of the power over several

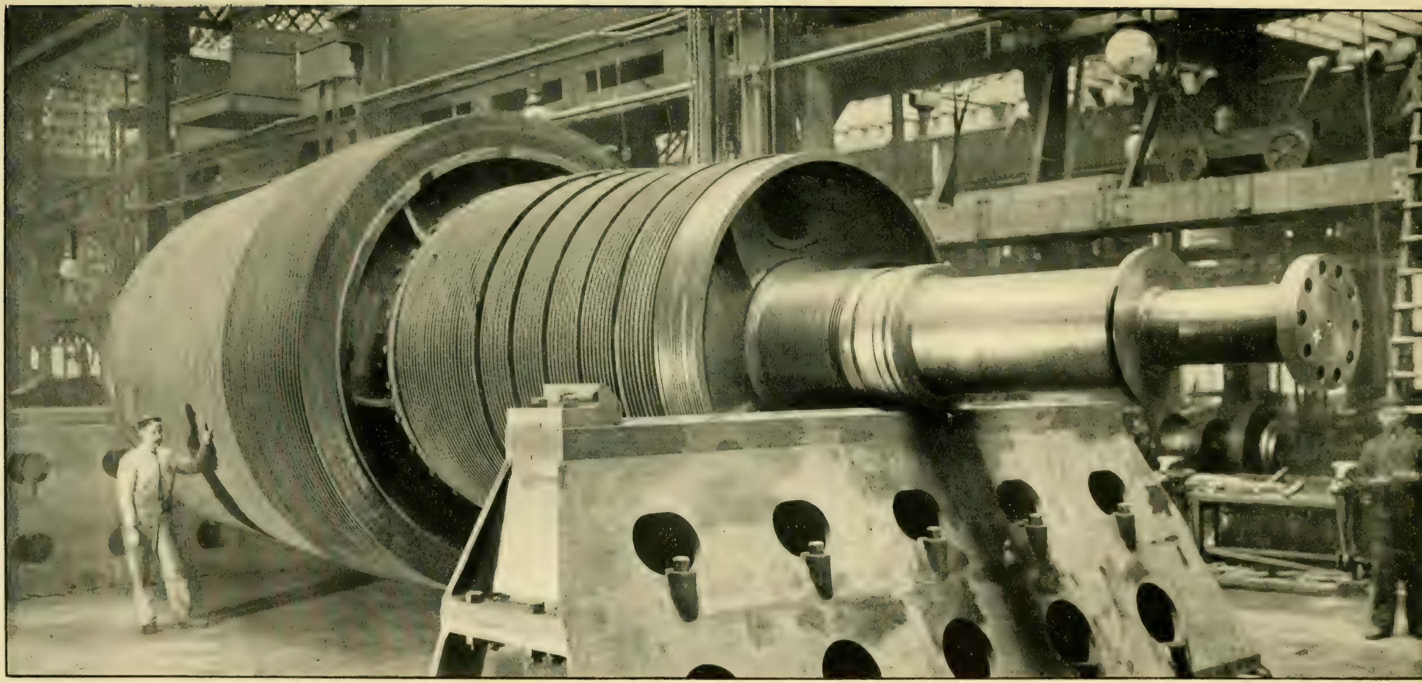


FIG. 4.—SHAFT OF A LARGE MARINE TURBINE.

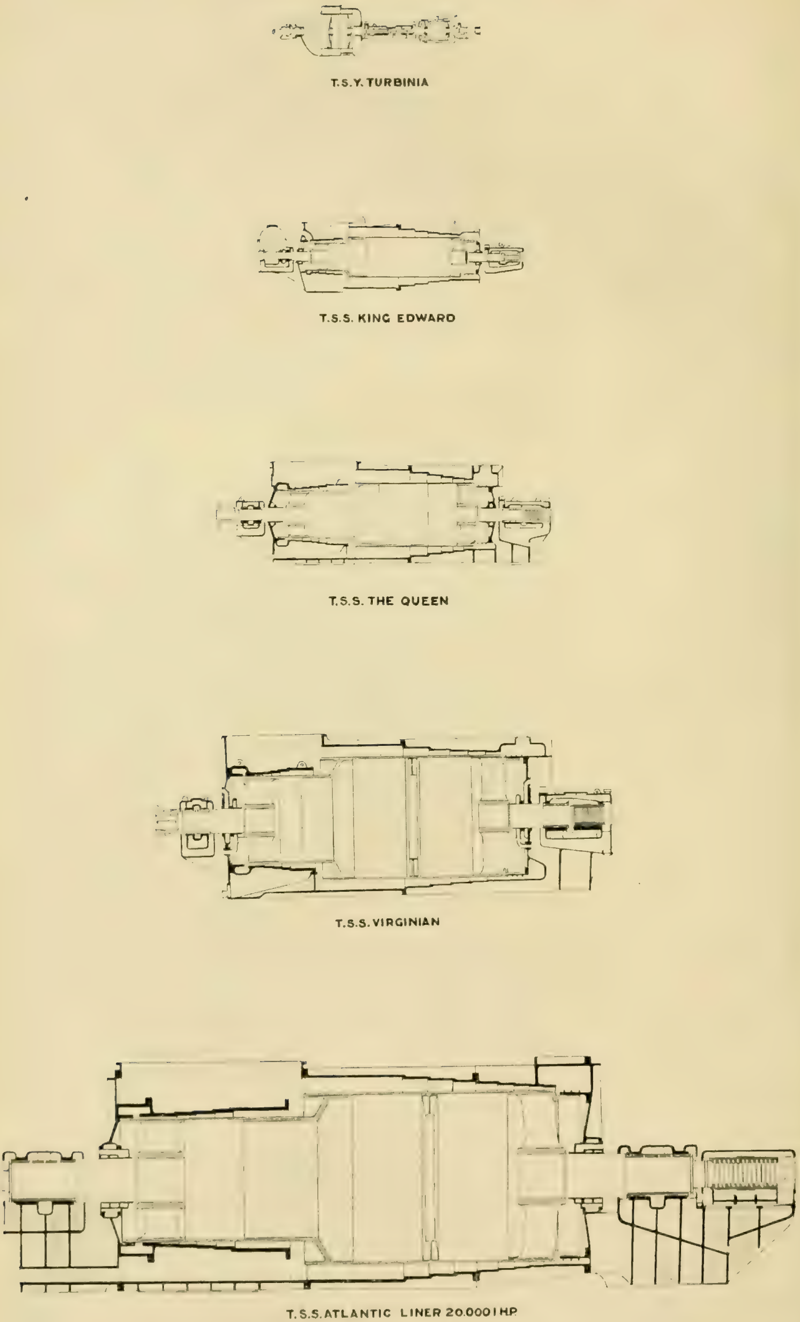


FIG. 5.—DIAGRAM, SHOWING INCREASE IN SIZE OF MARINE TURBINES.

separate shafts, arises from the fact that smaller propellers may be used, making higher speeds of rotation admissible, which again acts in lightening and improving the economy of the turbines.

The second question, that of the propeller, was much more difficult. It was not simply the problem of designing a screw with a moderate slip ratio and a moderate loss by skin-friction of the blades in the water, but it was complicated by cavitation, or the hollowing out of the water and the production of vacuous cavities caused by the force of the blades tearing through the water, a phenomenon first noticed by Sir John Thornycroft and Mr. Sidney Barnaby in 1893, and by them named cavitation. This apparatus shows the phenomenon.

[A small tank was shown, with a model of the screw of a cross-Channel boat or of an Atlantic turbine liner. It was pointed out that it was very difficult to make the screw cavitate, because it was especially designed not to cavitate; it was, however, made to do so in the tank by removing the atmospheric pressure from the surface of the water above the propeller by the air-pump. The removal of the atmospheric pressure, which helped to keep the water solid, enabled cavitation to be induced at a much lower speed of revolution. In the tank there was a head of about $1\frac{1}{2}$ inch of water above the topmost blades. If the tank had not been exhausted there would have been a head equivalent to 32 feet, plus $1\frac{1}{2}$ inch, plus capillary forces, tending to keep the water solid. Therefore, instead of 1500 revolutions (the speed of the propeller when serious cavitation was induced) a speed of at least 20,000 revolutions would have been required (because forces that induce cavitation vary as the square of the surface-speeds of the blades).] Serious cavitation causes an inordinate loss of power, chiefly because it disturbs the steam lines around the propeller blades, and it was proved by this experiment how easy it is to put too much work on a screw. There is a limiting thrust that it will bear, and if we exceed this thrust it will, so to speak, more or less strip its thread in the water and its efficiency will rapidly fall. The solution of the problem, as regards the screw propeller, has therefore resulted in a modification of the proportions of the ordinary propeller, and has lain in the direction of smaller diameters, wider blades, and a slightly finer pitch-ratio, which three slight changes have combined towards higher angular speeds of the propeller without material loss of efficiency.

Let us now turn our attention to the economic results of the steam turbine. In the case of large engines and dynamos that are coming generally into use, for the generation of electricity in this and other countries, of a horse-power of 1000 to 12,000 and upwards, the steam turbine with its accompanying dynamo is found to be cheaper in first cost, running expenses, and fuel, than the reciprocating engine and its slow-speed dynamo; and so much is this the case that it seems possible to generate electricity in colliery districts almost, if not quite,

as cheaply for electro-chemical purposes as it can be produced at Niagara and some other large centres of water power.

The chief items in which saving has resulted as compared with the reciprocating engine are : the total capital cost of the station is reduced by from 25 per cent. to 40 per cent. ; the reduction in the cost of fuel and boilers is between 10 per cent. and 30 per cent., and the consumption of oil is reduced to one-sixth, while the engine-room staff is reduced by 25 per cent. to 50 per cent.

As to the economic results of turbine vessels compared with vessels propelled with piston-engines, reliable statistics are available.

In 1897, the "Turbinia" was found to have an economy in steam per horse-power developed, equal to, if not superior to, that of similar vessels propelled by reciprocating engines ; and later, in 1903, she was again tried with modified propellers as now generally used which gave a further increase of efficiency of about 10 per cent. over the 1897 trials.

In 1902, the first turbine passenger boat, "King Edward," on the Clyde, was found to consume about 15 per cent. less coal than a similar vessel propelled by triple expansion engines and twin screws.

In the diagram, Fig. 6, is shown the principal running expenses of the turbine steamer "Queen," plying between Dover and Calais, compared with other three vessels on the same service. The cost of coal, engine-room staff, and oil, are shown in terms of the number of passengers each vessel is capable of carrying.

The statistics of the turbine vessels "Onward" and "Invicta," on the Boulogne and Folkestone route, have confirmed these results.

The trials of the third-class cruiser "Amethyst," in 1904, and of her sister vessel the "Topaz," propelled by triple expansion engines and twin screws, showed that, at a speed of 11 knots, the consumption of steam was the same in both vessels, but, as the speeds were increased, the turbine vessel gained relatively in economy, and at 18 knots was 15 per cent. more economical, and at $20\frac{1}{2}$ knots 31 per cent., and at full speed 36 per cent. Her superior economy in coal enabled her to reach a speed of 23·63 knots, or $1\frac{1}{2}$ knots more than the "Topaz," on the same coal allowance. The results of the trials also showed that, at a speed of 20 knots, the "Amethyst" could steam about 50 per cent. more miles than the "Topaz" on the same quantity of coal.

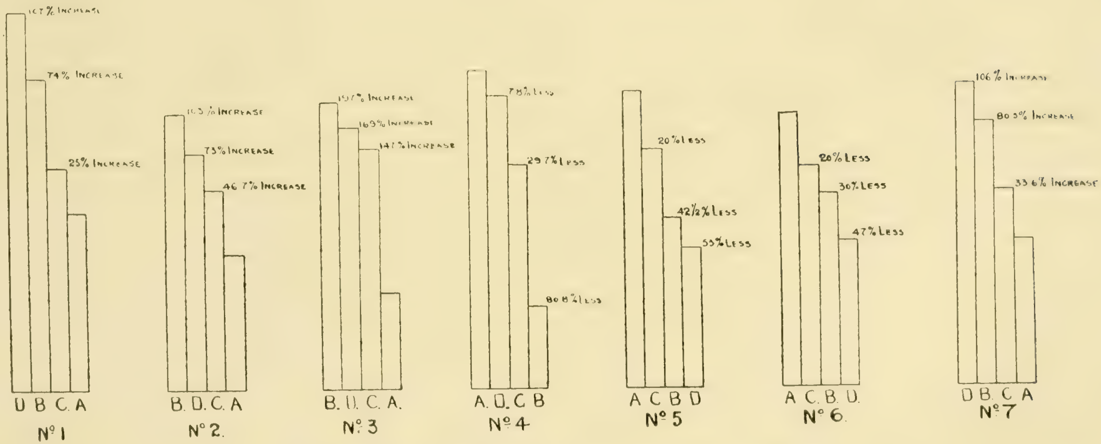
The experience as regards Atlantic liners is as yet limited to three vessels, the "Virginian," the "Victorian," and the "Carmania." The first two are of the Allan line, 520 feet in length, 15,000 tons displacement, and 12,000 horse-power, with a sea-speed of from 16 to 17 knots.

These vessels have been running since the spring of 1905, and the consumption of coal has been estimated to be no more, and probably less, than would have been the case had they been fitted with the most economical engines of ordinary type.

The Cunard liner "Carmania," of 672 feet in length, 30,000 tons

Diagram Shewing Comparison of Earning Power of Turbine Propelled Cross-Channel Steamer "The Queen" on Calais and Dover Route as Compared with Three other Vessels Fitted with Ordinary Engines on Same Route.

Nº 1. Pounds of Coal burnt per Each Passenger, Vessels are Certified to Carry.
Nº 2. Number of Engine Room Staff per Each Passenger, Vessels are Certified to Carry.
Nº 3 Oil Consumption per Double Trip per Each Passenger. Vessels are Certified to Carry
Nº 4. Number of Trips Made in 6 Months
Nº 5. Number of Passengers Vessels are Certified to Carry per Ton of Coal Burnt.
Nº 6. Power Developed per Ton of Coal Burnt per Double Trip and 24 Hours
Nº 7 Cost of Coal, Oil and E. R. Staff per Number of Passengers Vessels are Certified to Carry



A.	"The Queen" 323 × 43.	21 Knots	Average	Service	Speed
B.	324 × 34 9	18.	Do	Do	Do	Do
C	280 × 35.	18½	Do	Do	Do	Do
D	313 × 36.	17½.	Do	Do	Do	Do

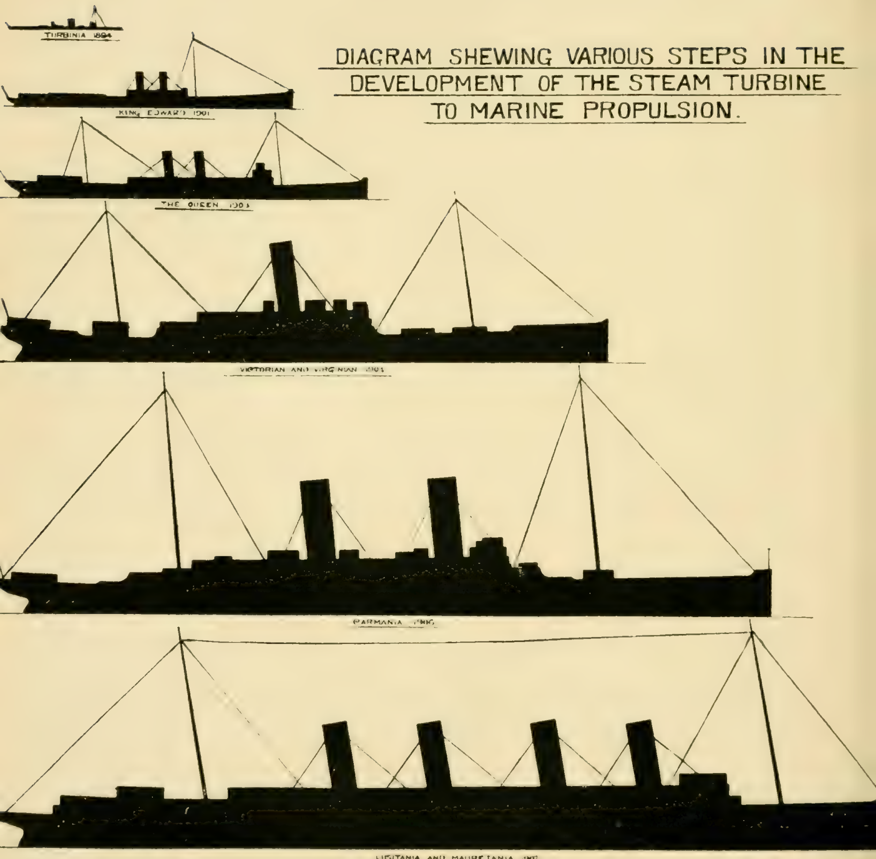


DIAGRAM SHEWING VARIOUS STEPS IN THE
DEVELOPMENT OF THE STEAM TURBINE
TO MARINE PROPULSION.
TURBINIA 1894
KING EDWARD 1901
THE QUEEN
VICTORIAN AND VIRGINIAN
CARMANIA
LUSITANIA AND MAURETANIA

FIG. 7.

displacement, and 21,000 horse-power, is a sister vessel to the "Caronia," propelled by quadruple expansion engines of the most economical type, and during the last four months the consumption of coal in the two vessels has been carefully measured, but it is too soon as yet to give the results. However, on the official trials, the turbine vessel exceeded the speed of her sister ship by one knot.

Some of the advantages found to exist with turbine propulsion are, that the propellers never race in the heaviest seas, and that, as a consequence, the speed is better maintained under all weather conditions; and the cause of this is to be traced to the smaller diameter of the propellers, wider blades, and deeper immersion. There is also much less vibration.

The tendency of late has been to increase the reversing or astern power of turbine vessels to such an extent that, in many cases, the stopping and manœuvring powers have been equal to those of twin screw vessels with reciprocating engines. The starting of turbine vessels is relatively quick, for the torsional force of a turbine, when starting from rest with full steam on, is at least 50 per cent. greater than the torque at the usual running speed, because the blades, when running slowly, meet the full blast of the steam instead of moving with it as they do at their usual speeds. With ordinary engines, the starting torque does not exceed the torque at full speed. When manœuvring, turbines cannot fail to respond when steam is turned on, for they have no dead centres upon which to stick, as in the reciprocating engine.

From the fact that the faster and larger the vessel the better has been the performance, it seems safe to infer that the two very large and fast Cunarders now building will give satisfactory results, and the same may be expected as regards new turbine construction in ships of war.

The diagram, Fig. 7, shows the various steps in the development of the steam turbine as applied to marine propulsion.

The total horse-power in steam-ships sailing under all flags is at present about eight millions. Of this total, about one quarter, or two millions, is in the faster class of ships to which turbines are suitable.

Of the remaining six millions horse-power, about three to four are in the larger class of ocean tramp, and the remainder in coasting steamers and small river boats, etc.

By a combination of the turbine with the reciprocating engine there seems to be no doubt that the three or four millions horse-power of large ocean tramps may be successfully propelled with a saving of from 15 to 20 per cent. in cost of fuel.

This combination has not yet been applied to any vessel. In it the reciprocating engine first expands the steam from the boiler down to about atmospheric pressure, and then passes on to the turbines, which complete the expansion down to the condenser pressure. The turbine thus utilises the lower part of the expansion, which the

reciprocating engine cannot do, and the combination is therefore a good one. For manœuvring or stopping the vessel, either the engine or the turbines, or both, may be used, and there seems to be no doubt that this arrangement will come into vogue for the slower class of vessels of larger size.

Turbines have been applied to other uses within the last ten years. The most important of these are for the working of rotary blowers, air-compressors, and water-pumps.

The photograph, Fig. 8, shows a cross-section through a turbo-blowing engine, capable of compressing 21,000 cubic feet of free air per minute to a pressure of 17 lbs. per square inch, which represents about 1000 horse-power in the air, reckoned in adiabatic compression. In general construction the turbine air-blower portion is similar to a steam turbine. The blades or vanes which propel the air are plano-convex in section, and set in rows at an angle similar to that of the blades of a ship's propeller. Between the rows of moving blades are rows of guide-blades inwardly projecting from the case. These latter are also of plano-convex section, and are set with their plane surfaces parallel to the axis ; and their purpose is to assist the flow, and to stop the rotation of the air after being acted on by the moving blades. Each row of moving and fixed blades adds a little to the pressure, and compresses the air gradually along the annular space between the drum and the case. Balance pistons or dummies are provided for balancing the end-thrust of the air, as in the steam turbine. The speed of rotation is 3600 revolutions per minute, and the tip velocity of the air blades about 400 feet per second.

[C. A. P.]

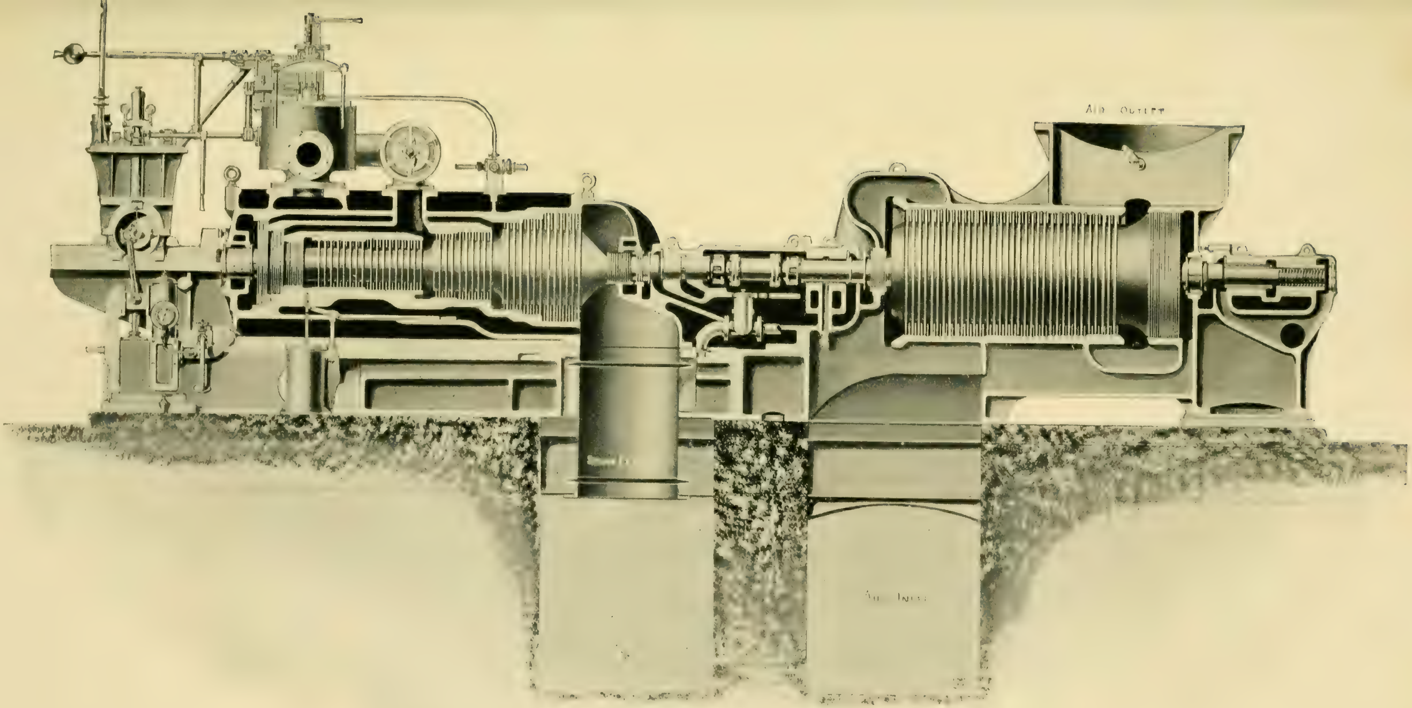


Fig. 8. Turbo-Blowing Engine. Section through Steam and Air Turbine Cylinders.

GENERAL MONTHLY MEETING,

Monday, May 7, 1906.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S. President, in the Chair.

Horatio Ballantyne, Esq.
Sir Walter Balfour Barttelot, Bart.
Gustav Hamel, M.D. M.V.O.
William Morris Mordey, Esq.
Captain Adrian Rose,

were elected Members of the Royal Institution.

It was announced that His Grace the President had nominated the following Vice-Presidents for the ensuing year :—

The Right Hon. Lord Alverstone, G.C.M.G. M.A. LL.D. F.R.S.
Sir William Huggins, O.M. K.C.B. D.C.L. LL.D. Ph.D. F.R.S.
The Right Hon. Lord Kelvin, O.M. G.C.V.O. D.C.L. LL.D. D.Sc. F.R.S.
Dr. Ludwig Mond, Ph.D. F.R.S.
The Right Hon. Lord Sanderson, G.C.B. K.C.M.G.
The Right Hon. Sir James Stirling, M.A. LL.D. F.R.S.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. (Treasurer)
Sir William Crookes, D.Sc. F.R.S. (Honorary Secretary).

The Chairman reported the decease of Professor Pierre Curie, and the following resolution of condolence passed by the Managers at their meeting held this day was read and was adopted.

Resolved, that the Managers of the Royal Institution of Great Britain desire to record their deep sense of the loss the scientific world has sustained in the decease of Professor Pierre Curie, one of the Honorary Members of the Institution.

In June 1903, he delivered a Friday Evening Discourse, giving an account of the epoch-making discoveries made by himself and Madame Curie on the isolation and properties of Radium, and also embodying the results of some low temperature experiments made by him in the Laboratory of the Royal Institution. In the following year he was elected an Honorary Member of the Royal Institution.

The Managers, on behalf of the Members of the Royal Institution, desire to offer to Madame Curie the expression of the most sincere sympathy in her bereavement.

The special thanks of the Members were returned to the Royal Institution Picture Committee, Dr. Ludwig Mond, and other members for their valuable gift of Mr. H. Jamyn Brooks' Picture of a Friday Evening Discourse by Professor Sir James Dewar in the Lecture Room of the Institution.

The Special Thanks of the Members were returned to a Member for his Donation of £26 5*s*. to the Fund for the Promotion of Experimental Research at Low Temperatures.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Memorandum on the Age Tables and Rates of Mortality of the Indian Census of 1901. 4to. 1905.
Report on Kodaikanal and Madras Observatories for 1905. 4to. 1906.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XV. 1° Semestre, Fasc. 5–6. 8vo. 1906.
Classe di Scienze Morali Rendiconti. Vol. XIV. Fasc. 9–10. 8vo. 1905.
American Academy of Arts and Sciences—Proceedings, Vol. XLI. Nos. 20–29. 8vo. 1906.
Memoirs, Vol. XIII. No. 3. 4to. 1906.
American Geographical Society—Bulletin, Vol. XXXVIII. Nos. 3–4. 8vo. 1906.
Astronomical Society, Royal—Monthly Notices, Vol. LXVI. No. 5. 8vo. 1906.
Automobile Club—Journal for April, 1906.
Bankers, Institute of—Journal, Vol. XXVII. Parts 4–5. 8vo. 1906.
Beckenhaupt, C., Esq. (*the Author*)—Die Tetraeder Theorie. 8vo. 1906.
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Boston Public Library—Monthly Bulletin for April, 1906. 8vo.
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British Astronomical Association—Journal, Vol. XVI. No. 6. 8vo. 1906.
Buenos Ayres—Monthly Bulletin for January, 1906. 4to.
Canada, Commissioner of Emigration—Canadian Life and Resources; 4to. 1906.
Canada for April, 1906. 4to.
Carnegie Institute, Washington—Report of Mt. Wilson Solar Observatory, Nos. 3–7. 8vo. 1906.
Report of the Director, 1905. 8vo. 1906.
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Journal for April, 1906. 8vo.
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Chicago, University of—Decennial Publications, Vol. XVI. 8vo. 1906.
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Editors—Aeronautical Journal for April, 1906. 8vo.
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WEEKLY EVENING MEETING,

Friday, May 11, 1906.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

PROFESSOR J. H. POYNTING, SC.D. F.R.S.

Some Astronomical Consequences of the Pressure of Light.

[ABSTRACT.]

THE experiments of Lebedew and Nichols and Hull have proved conclusively that light presses against any surface upon which it falls, and the extraordinarily accurate experiments of Nichols and Hull have fully confirmed Maxwell's calculation that the pressure per square centimetre is equal to the energy in the beam per cubic centimetre.

A clearer idea of the effect of light or radiation pressure is obtained by thinking of a beam of light as a carrier of momentum. We then see that not only does it press against a receiving surface, but also against the surface from which it started.

Some experiments by Dr. Barlow and myself appear to bring to the front this conception of light as a momentum carrier. If a beam falls on a black surface at an angle to the normal, there should be a tangential stress along the surface. An experiment was described in which light fell on a blackened disc at the end of a torsion arm, the disc being at right angles to the arm.* The disc was pushed round by the tangential stress. The experiment was carried out in a partially exhausted vessel, but the residual air was a source of disturbance by convection and radiometer effects. A better experiment was made by suspending a disc of mica blackened beneath, about 2 inches in diameter, by a quartz fibre, the disc being horizontal and suspended from its centre. When a beam of light fell at 45° on a part of the disc, the horizontal component of the beam being at right angles to the radius to the part where it fell, the disc moved round through the combined effects of convection, radiometer action, and the tangential stress. When the beam was allowed to fall on the same place at 45° on the other side of the vertical, convection and radiometer action were very nearly as before, but the tangential stress was reversed. The difference in torsion in the two cases was twice that due to the tangential stress. An experiment with prisms † was also described.

* *Phil. Mag.*, ix. (1905) p. 169.

† *Ibid.*, p. 404.

Regarding a beam of light as a momentum carrier, it is easily seen that if the receiving surface has velocity u towards the source and the velocity of light is U, the pressure is increased by the motion by the fraction $\frac{u}{\mathrm{U}}$. If the velocity is reversed, the pressure is decreased by this fraction. This is the "Doppler reception effect."

If the source is moving, and we assume that the amplitude of the emitted waves depends on the temperature and nature of the source alone, it can be shown that the pressure on the source is $\frac{\mathrm{U}}{\mathrm{U} \mp u}$ of its value when the source is at rest. This is the "Doppler emission effect."

In considering the consequence of light pressure, it is necessary to know the temperature of a body exposed to the sun's radiation. It can be shown that a small black particle, at the distance of the earth from the sun, has about the mean temperature of the earth's surface, say 300° Abs., and that the temperature of the sun is about twenty times as high, say 6000° Abs. The temperature of the particle varies inversely as the square root of its distance from the sun.

The direct pressure of sunlight is virtually a lessening of the sun's gravitation pull. On bodies of large size this is negligible. On the earth it is only about a forty-billionth of the sun's pull, but the ratio increases as the diameter decreases, and a particle one forty-billionth of the earth's diameter, and of the same density, would be pushed back as much as it is pulled in, if the law held good down to such a size. If the radiating body is diminished, the ratio of gravitation pull to light push is similarly diminished, and it can be shown that two bodies of the temperature of the earth's surface and of the earth's mean density would neither attract nor repel each other, if their diameter was about 1 inch. The consequence of this on a swarm of meteorites is obvious. It is probable that this balancing of gravitation and light pressure must be taken into account in the motion of the particles supposed to constitute Saturn's rings.

When we consider the motion of a small particle round the sun, we have, first, the direct pressure lessening gravitation. If it has density equal to that of the earth and diameter $\frac{1}{1000}$ inch, the lessened pull at the distance of the earth will imply a lengthening of the year by nearly two days. Secondly, the Doppler emission effect comes into play, for the particle crowds forward on its own waves emitted in front, and draws away from those emitted behind, so that there is increase of pressure in front and a decrease behind. Thus there is a force resisting the motion. The particle will then tend to fall inwards in its orbit, and in the case considered about 800 miles in the first year. It would probably move in a spiral into the sun, and reach it in less than 100,000 years. A particle 1 inch in diameter would reach the sun from the earth in less than a hundred million years.

The Doppler reception effect will not come into play in a circular

orbit, but in an elliptic orbit it acts as if it were a force resisting change of distance, and therefore it tends to make an elliptic orbit even more circular.

Applying these considerations to a comet regarded as a swarm of small particles coming into our system, a sorting action will at once begin. The smaller particles will have their period of revolution lengthened out more than the larger ones, and they will tend to trail behind. The Doppler emission effect will damp down the motion, and again, more markedly with the smaller particles, and all will tend to spiral into the sun. The Doppler reception effect will tend to destroy the ellipticity of the orbit, more especially with the smaller particles, and ultimately the particles of different sizes may move in orbits so different that they may not appear to belong to the same system. In course of time they should all end in the sun. Perhaps the zodiacal light is due to the dust of long dead comets.

It appears just possible that Saturn's rings may be cometary matter which the planet has captured, and on which these actions have been at play for so long that the orbits have become circular.

[J. H. P.]

WEEKLY EVENING MEETING,

Friday, May 18, 1906.

The Right Hon. Sir James Stirling, P.C. M.A. LL.D. F.R.S., Vice-President, in the Chair.

Professor Arthur Schuster, Langworthy Professor of Physics in the University of Manchester.

International Science.

The pursuit of science has always joined in sympathy men of different nationalities, and even before the days of rapid letter post and quick travelling, intercourse, especially by correspondence, exercised a considerable influence on scientific activity. Such intercourse was, however, of a personal and purely stimulating character, and only quite exceptionally was there any direct attempt to organise investigations which required a combination of workers in different localities. Within the last century, however, many problems became urgent which could not be solved without some international agreement, and special organisations came into life which have rendered a service the importance of which cannot be exaggerated.

At present we are confronted with a new difficulty. International combination has become so necessary, and organisations have in consequence increased to such an extent, that they begin to overlap, and there has been some danger of mutual interference. Fear has also been expressed that any attempt to advance knowledge by an organised combination of workers might discourage private efforts, and therefore do mischief rather than good. It must be acknowledged that this danger exists. The proper function of combination must be clearly separated from that of private enterprise, and some general regulating control is therefore called for. The time seems ripe for a general review of the situation.

We may distinguish between three types of international organisations. The first aims simply at collecting information, the second is intended to fix fundamental units or to initiate agreements on matters in which uniformity is desirable, while in the third type of organisation a more direct advance of knowledge is aimed at, and research is carried out according to a combined scheme. Generally an international association does not entirely fall within any single one of these divisions, but it is useful to draw the distinction and classify the associations according to the main object which they are intended to serve.

The best example of an organisation formed for the purpose of collecting information is furnished by the great undertaking initiated by our Royal Society and having for its object the systematic cata-

loguing of the scientific literature of the world both according to the subjects and authors. Twenty-nine countries (counting the four Australian Colonies separately) are actively participating in this work by furnishing slips containing the entries which form the basis of the catalogue. A still larger number of countries assist by subscribing to the annual volumes.

The subjects included in the catalogue are classified according to seventeen branches of science, as follows :—

(A) Mathematics ; (B) Mechanics ; (C) Physics ; (D) Chemistry ;

—	Sets	A	B	C	D	E	F	G	H	J	K	L	M	N	O	P	Q	R
Russia .	14	2	2	11	6	18	15	19	20	20	13	8	38	30	5	14	8	8
France .	27	4	5	11	17	4	3	10	7	5	6	15	13	12	7	3	18	16
Switzerland	7																	
Canada .	7																	
Holland .	5	1	2	1	3	1	2	1	2	3	1	4	3	3	1	1	2	3
Greece .	2																	
Hungary .	4																	
Norway .	3	..	..	1	1	..	1	.	..	..	.	..	2	4	1	1	2	..
India . .	29	5	4	7	5	2	5	2	3	4	2	5	14	5	2	..	4	6
United States .	62	11	14	17	14	10	11	8	12	10	7	9	12	10	3	3	7	9
Great Britain .	29	5	7	18	17	6	8	8	8	5	4	6	6	5	6	6	7	13
Austria .	4	1	2	4	2	1	4	3	5	6	2	4	4	5	1	3	..	1
Cape of Good Hope	6	..	..	..	2	..	..	2	2	..	2	..	1	..	..	..	..	..
Denmark .	6																	
Egypt . .	1																	
Finland .	1	1	2	2	2	1	1	2	3	1	1	1	2	2	1	1	2	1
Germany .	44	6	8	14	18	2	5	3	4	5	1	13	9	8	5	2	18	7
Italy . .	27																	
Japan . .	15																	
Mexico .	5																	
New South Wales .	2																	
Nova Scotia	1																	
Orange River .	1																	
Poland .	1																	
Portugal .	1																	
Queensland	2																	
South Australia	2																	
Sweden .	5																	
Victoria .	1																	
Western Australia	1																	
Total .	315	38	46	86	86	45	55	58	66	59	39	65	103	90	32	34	66	64
	Sets	A	B	C	D	E	F	G	H	J	K	L	M	N	O	P	Q	R

(E) Astronomy ; (F) Meteorology ; (G) Mineralogy ; (H) Geology ; (J) Geography ; (K) Palæontology ; (L) Biology ; (M) Botany ; (N) Zoology ; (O) Anatomy ; (P) Anthropology ; (Q) Physiology ; (R) Bacteriology.

Subscribers may either obtain complete sets or any of the separate volumes. The relative popularity of the different subjects is illustrated by the preceding table which gives in the different columns for each science the volumes approximately required by each country. The figures are, of course, subject to variations from year to year. The first column shows the number of complete sets subscribed for in addition to the separate volumes ; these presumably find their way into university or public libraries.

The popularity of the special botanical catalogue is remarkable.

We may obtain a rough idea of the scientific activity of different countries by comparing the number of slips received from them during a certain interval. The numbers given in the report published by the International Convention held in London last summer and referring to all slips received, are shown in the following table.

The total number up to March 1906, has increased to 700,000.

—	Slips Received.	Number of Journals.	Average Number of Slips per Journal.
Austria	13,186	535	25
Belgium	2,272	174	13
Canada	537	45	12
Denmark	2,584	40	64
Finland	1,828	33	55
France	60,401	930	65
Germany	213,545	1,397	153
Holland	9,861	70	141
Hungary	2,605	35	75
India and Ceylon	2,699	31	87
Italy	21,238	300	71
Japan	3,043	42	72
New South Wales	2,049	8	256
New Zealand	440	1	440
Norway	2,017	36	56
Poland	5,820	65	90
Russia	25,741	457	56
South Africa	1,872	15	125
South Australia	159	6	56
Sweden	1,639	63	31
Switzerland	5,140	126	41
United Kingdom	56,382	488	116
United States of America	66,071	588	112
Victoria (Australia)	2,858	23	124
Total	504,297	5,508	90

The catalogue begins with the year 1901, but some countries send in their slips rather earlier than others, so that the time interval covered by the investigations to which the table refers is not quite the same for all. Nevertheless, the numbers shown in the table possess a certain interest. I have given in the last two columns the number of journals which different countries take into account, and the ratio of the number of slips to the number of publications. Here again it is difficult to estimate accurately how much value is to be attached to the figures, as there is no uniformity of selection as to what should, and what should not be included in the catalogue. Journals which may only very seldom contain any paper which is to be included. may unduly diminish the numbers in the last column, which are also affected by the interpretation given as to what is purely technical, and therefore to be excluded. Nevertheless, the comparison between the United Kingdom and France gives the somewhat striking result that while France is slightly ahead in the number of separate entries it contributes to the catalogue, it takes account of nearly double the number of journals, and the ratio showing the number of entries per journal is therefore very small. In the case of Belgium and Canada, we find also a large number of publications as compared with the slips received.

Regard must, however, be had to the fact that in the subject catalogue the same paper may furnish several entries. Especially is this the case in biological subjects where several species may be described, for each of which a separate slip must be written out. Hence, in any country active chiefly in the discovery of new species the ratio given in the last column of the table would be abnormally large. This is probably the explanation of the figures given for New Zealand. In the opinion of the Director of the Central Bureau, the standards adopted by different countries are drawing nearer together as the work proceeds, and before long we may therefore expect to obtain valuable statistical information on the scientific activity in different countries. But this is only an incidental result of the undertaking. It may reasonably be argued that the scientific investigator ought not, before he begins a research, to trouble too much about what may have been done by others in the same direction, but there is no doubt that before publication he should have made himself acquainted with the literature of his subject. A well arranged catalogue then becomes a necessity, though its value as a means of helping students differs considerably in different subjects.

The governing body of the catalogue is an international council composed of one representative from each of the countries taking part in the scheme. This council has appointed an executive committee, of which Professor Armstrong is the chairman.

The Central Bureau for the publication of the Catalogue is in London under the direction of Dr. Henry Forster Morley, who has a

staff of thirteen workers under him. There are in addition nineteen experts or referees representing the different sciences. The annual office expenses, including salaries, amount to about 2200*l.*; while the expenditure on printing, binding and publication in the year ending March 1, 1905, amounted to nearly 4900*l.* The two items are just covered by the guarantees of the different countries which, as already mentioned, take the form of subscriptions for copies of the catalogue, so that it may be said that the Central Office is self-supporting. After so short a time of working, this success must be a source of considerable satisfaction to Professor Armstrong and those who have helped to initiate the work. But the expenses incurred in London only represent a fraction of the total cost of the work. Most of the countries establish regional bureaux which prepare the slips and forward them to London. This really constitutes the most serious part of the work. In Germany, for instance, the regional bureaux are under Professor Uhlworm, one of the university librarians, who is helped by six assistants and devotes his whole time to the work.

I pass on to an undertaking of a very different kind, but still one which must be included in the class which primarily aims at cataloguing. The accurate determination of the positions of the stars for a particular period is a work which must precede all exact measurements of their proper motions. Hence it constitutes a fundamental problem of Astronomy. The multitude of stars seen on a bright night is bewildering to the casual observer. They are described in poetical writings as innumerable, but when an actual count is made, it is found that their number is really moderate, and it is doubtful if more than two thousand stars have ever been visible to the naked eye at the same time. The use of the telescope considerably increases this number, according to the size of the object glass or reflecting mirror used. Thus, Argelander in his great star catalogue included nearly 324,200 stars which he observed through his telescope of four inches aperture. The advent of photography, and the manufacture of suitable lenses to be used in connection with photography, increased the astronomical output of a fine night to such an extent that it became possible to make a further and very substantial advance. The international Star Catalogue which is at present being constructed, owes its origin chiefly to the hard work of Admiral Mouchez, who was at the time Director of the Paris Observatory, and who became converted to the feasibility of the plan by the excellent results obtained by the brothers Henry, the pioneers in star photography. He was assisted by the energetic support of Sir David Gill, to whom the first suggestion was due. The programme of work was determined upon at an International Conference which met at Paris in the year 1887. Eighteen observatories were to take part in the work, the telescopes to be used were to have an aperture of thirteen inches, and such a focal length that a millimetre on the plate corresponded to one minute of arc.

Each observatory had a certain region of the sky assigned to it, and undertook to cover this region four times, twice with plates of short exposure, twice with plates of long exposure, and to measure all the stars appearing on the short exposure photographs. The long exposures were intended for reproduction in the form of charts, and are only taken by some of the observatories. As there are about 400 stars on each plate and it takes about 600 plates to cover the share of one observatory once, this means that each observatory has to measure nearly half a million star places, and that the complete catalogue will give the positions of nearly four and a half million stars. This includes all stars down to the eleventh magnitude.

The following is a list of observatories taking part in the work :—

For the Northern Hemisphere : Greenwich, Oxford, Paris, Bordeaux, Toulouse, Potsdam, Helsingfors, Rome, Catania, Algiers.

For the Southern Hemisphere : San Fernando, Tacubaya, Santiago de Chile, Cordoba, Cape of Good Hope, Perth (W. Australia), Sydney, Melbourne.

The work connected with the ultimate completion of the catalogue and especially the reproduction of the star maps requires considerable expenditure. Each country has to make its own arrangements, which in the British Empire usually means that each body concerned has to pay its own expenses. There was, however, in this case, some official help. The Astronomer Royal obtained a contribution of 5000*l*. from the Government for the reproduction of charts, and in the case of the Cape of Good Hope the necessary expenses have been met from Imperial Funds. Professor Turner, of Oxford, has obtained a grant of 1000*l*. from the Government grant of the Royal Society, and a further sum of 2000*l*. for publication from the Treasury and the University of Oxford jointly ; but the Australian Colonies are much hampered by the want of funds, and their work will be delayed in consequence. The four French observatories on the other hand are well supported. Each of them has received a Government contribution of 25,700*l*., making a total of well over 100,000*l*. More than half this goes towards the reproduction of the long exposure photographs as a series of charts, which, however, have proved to be so costly that they will probably never be completed. Indeed, if completed, their utility may to some extent be impaired by the difficulty of storing them in an accessible manner. Professor Turner calculates that the series of maps will form a pile of papers 30 feet high, weighing about two tons.

I now pass on to a few examples of undertakings which are intended to fix standards of measurement, or to establish a general agreement on matters in which uniformity is desirable. The foremost place in this division must be given to the Bureau International des Poids et Mesures, established in the year 1873, at Sèvres, near Paris. This bureau was the outcome of an international commission constituted in 1869, which had for its object the scientific construction of

a series of international metric standards. By a convention, entered into by the different countries at a diplomatic conference held at Paris in March and April 1875, means were created for carrying out the work of verifying standards under a new International Metric Committee, and for the purpose of enabling the Committee to execute their duties effectually, as well as of securing the future custody and preservation of new metric prototypes and instruments, the Permanent Metric Bureau was founded. The original cost of the Bureau was 20,000*l.*, and the annual budget was fixed at 3000*l.* for the period during which the prototypes were being prepared, after which time it was expected that the expenditure could be reduced to 2000*l.* In 1901, however, it reached 4000*l.*, the maximum to which by the terms of the convention the annual budget could be raised. Great Britain did not join the convention until 1884, when it declared its adhesion. A first payment of 1787*l.* was made as entrance fee, and the annual contribution now ranges between 200*l.* and 300*l.* Major MacMahon, to whom I owe the above details, is at present the British representative on the International Committee.

The work carried out at Sèvres is not confined to the reproduction of metric standards, but measurements of precision in various directions have been made with conspicuous success. Scientific thermometry owes much to the International Bureau, and in some respects it may be said that exact thermometry was created there. Professor Michelson's work in which the length of the metre was compared directly with the length of a wave of red light, is another classical investigation carried on in the laboratories of the International Bureau. More recently Mr. Guillaume examined the physical properties of alloys, notably those of nickel steel, and proved the possibility of manufacturing a material which shows no sensible expansion with rise of temperature. The importance of metallic rods the length of which does not depend on temperature is obvious, provided they prove to be of sufficient permanence.

It would lead me too far if I were to give an account of the Conference and Conventions which have led to a general agreement on the standards of electric measurements, but it is a satisfaction to know that these standards are essentially those proposed and first constructed by the British Association. The old British Association ohm no doubt was found to be wrong by more than 1 per cent, but it has remained the prototype of the present international unit, and in principle the old ohm, volt, and unit of current stand as they were given to us by the original Committee.*

* The original Committee was appointed in 1861, and consisted of: Professors A. Williamson, C. Wheatstone, W. Thomson (Lord Kelvin), W. H. Miller, Dr. A. Matthiessen, and Mr. F. Jenkins. In the following year, Messrs. C. Varley, Balfour Stewart, C. W. (Sir Charles) Siemens, Professor Clerk Maxwell, Dr. Joule, Dr. Esselbach, and Sir Charles Bright, were added to the Committee.

While in the case of scientific units complete agreement is absolutely essential, uniformity is desirable in other cases. There are matters of nomenclature in which confusion has arisen purely from want of general agreement. Thus the recent great improvement in the optical power of telescopes has led to the discovery of many details on the surface of the moon. Small craters or other distinctive features named by one observer were not correctly identified by another, so that at the present time the same name is applied to quite different things by different observers. It is quite clear that an international agreement in lunar nomenclature is called for.

There are other deficiencies of uniformity which perhaps appear trivial, but which yet lead to the waste of a good deal of time. Such, for instance, is the position of the index in scientific books. The index is placed sometimes at the beginning, sometimes at the end, and sometimes neither at the beginning nor at the end. Some books have no index, some have two—one for the subject matter and one for names of authors. The loss of time which arises from one's ignorance as to where to look for the index cannot be estimated simply by what is spent on the search, but must include the time necessary to regain the placidity of thought which is essential to scientific work.

We must now turn to the more serious aspect of those international associations which aim directly at an advance of knowledge. Mathematicians have drawn interesting conclusions from the contemplation of ideal beings who are confined to live on the surface and have no knowledge of anything that goes on outside the surface. Our Euclidean geometry would be unknown to them, and spiritualistic tricks could be performed by anyone possessing even to a minute extent the power of controlling a third dimension. It is, I think, worth while investigating the extent of the direct knowledge of a third dimension, which makes us so infinitely superior to the two-dimensional beings. We are able no doubt, through our eyes, to penetrate the depths of space, but we should be unable to interpret the impressions of our sight if we had not some tangible knowledge of three dimensions, and had not learned to bring the sense of sight and the sense of touch into harmony. But our sense of touch is confined to a very small distance from the ground on which we stand, and, independently of artificial means of raising ourselves above the surface of the earth, a layer six or seven feet thick represents the extent of our three-dimensional knowledge. Compared with the radius of the earth, the thickness of such a layer is small enough, for it would represent only the thickness of a sheet of paper on a sphere having a radius of 250 metres. Compared with the solar system, and even more so with stellar distance, a thickness of seven feet seems infinitesimal; yet the infinitesimal is essentially different from the zero, and even were our bodies much smaller than they are, we should continue to have the power to interpret three dimensions. These considerations show how important it is for us to increase our knowledge of the earth itself, and to extend

it as far as possible to the depth below our feet and the height above our heads.

In passing from the arbitrary units to which we refer our terrestrial measurements of length, to the scale on which we measure the dimensions of the solar system, and from them to stellar distances, the magnitude of the earth's radius or circumference forms an all important immediate quantity. One of the first acts of the French Academy of Sciences, founded in 1666, consisted in organising the work of accurately measuring the dimensions of the earth, and this at once enabled Newton to confirm his celebrated theory of universal gravitation. As improvements in the methods of measuring kept pace with the work actually accomplished, our knowledge steadily increased, but we are still improving on it. New problems have arisen requiring more minute study, and the measurement of the shape and size of the earth still remain a question of the first importance. The actual surveys and triangulation required for the purpose are of necessity left to the initiative of individual states or to the combination of the states primarily concerned, but the general discussion of results, as far as they apply to the earth as a whole, is entrusted to an International Geodetic Association, which at present consists of twenty-one states. These, together with their annual contributions to the general fund, are entered in the following table :—

	£		£
Belgium	80	Norway	40
Denmark	40	Austria	300
Germany	300	Portugal	80
France	300	Roumania	80
Greece	40	Russia	300
Great Britain	800	Sweden	40
Italy	300	Switzerland	40
Japan	300	Servia	40
Mexico	150	Spain	150
The Colonies of the Netherlands	40	Hungary	150
		United States of America	300

The Central Bureau of this Association is attached to the Royal Geodetic Institute of Potsdam, which is under the distinguished direction of Professor Helmert, who acts as secretary to the Association.

The question of measuring the size of the earth depends to a great extent on the measurement of arcs of meridian. As long as we were confined to Europe for the measurements of these arcs they remained necessarily short, but larger portions of our globe have become accessible to the theodolite, and there is especially one arc which is distinguished by the fact that it is the longest possible which can be traced along the land covering the earth's surface. It runs about 30° east of Greenwich, and a large portion of it passes through Africa. Owing to the great energy and enterprise of Sir David Gill, the work

of measuring this arc is well in hand, though at the present moment, want of funds threatens to endanger its completion. The Egyptian survey entrusted to Captain Lyons will no doubt receive continued support, and by an arrangement entered into between representatives of the German Government and Sir David Gill at a Conference held in Berlin, in 1896, Germany undertook to carry out the triangulation through her territory in South West Africa. I understand this work has been done and the triangulation of the Transvaal and the Orange River Colony is also complete. There is still a gap in the southern part of Rhodesia, but there is every hope that this will soon be bridged over. The British South African Company have spent 36,000*l.* on the work and thus have very materially assisted an important enterprise. When the African arc is complete it will be connected with the Russian and Roumanian arcs so as to form a continuous chain of 105° extending from 70° north to 35° south latitude. I have to point out, however, that in the opinion of those best able to judge, the completion of the South African arc is not the only undertaking to which this country is called upon to pay attention. The triangulation of our own island, excellent as it was when first made, has fallen below the accuracy required in modern geodetic work. Until our fundamental triangulation has been repeated the sums which at present are being spent on the detailed survey might find a better use.

The main result of the work has been that so far as present measurements allow us to judge, the surface of the ocean can be well represented by a surface of revolution, and it is not necessary to assume a more complicated shape. The mean radius of the earth is determined to about 100 metres, which means a possibility of doubt amounting to about one part in 60,000.

Geodetic work is, however, not confined to measurements of length, for important information may be derived from an exact knowledge of the acceleration of gravity over its surface. The introduction of the pendulum of short length intended for relative and not for absolute measurement has greatly facilitated this work, and it is hoped that these pendulum observations may be carried out over still more extended regions. India is setting a good example. It has measured two arcs of meridian, and the gravitational work carried out by Captain Burrard, and recently published by the Royal Society, is of primary importance. But, otherwise, British Colonies require encouragement to do more. I am assured that measurements of the gravitational constant in Canada would be of the greatest importance.

The bearing of such work on our knowledge of the earth may perhaps be illustrated by one example. It has often been a matter of wonder how mountain chains such as the Himalayas could rest on the lower strata of the earth without crushing them and forcing them in by the pure power of their weight, and the most plausible theory to account for this was found in the idea first suggested by Pratt that

the mountain chains must not be compared with a large weight resting on an understructure, but rather with a lighter body partially immersed in a heavier one. Mountains, according to this theory, float in the body of the earth very much like icebergs float in water. The truth of this theory can only be tested by accurate measurement of the gravitational force from which information may be derived on the distribution of density in the earth's strata near the surface. On the whole, the measurements so far available have confirmed Pratt's hypothesis.

More recently, another problem has occupied the attention of the International Geodetic Association, and owing to its immediate interest, has absorbed the greater portion of its funds. The astronomical world was surprised by the announcement of Professor Chandler that he was able to demonstrate from existing observations that the earth's pole describes a closed curve taking about fourteen months to complete a revolution. The possibility of a periodic shift of the earth's axis was foreseen by Euler, who calculated the time of revolution to be ten months, but observations did not show a sensible period of that duration. No one apparently before Chandler tried to see whether another period beyond a small annual one existed. The discrepancy between the calculated ten and the observed fourteen months was cleared up by Professor Newcomb, who pointed out that Euler's calculation was based on the supposition that the earth is an absolutely rigid body. Any yielding would increase the length of the period, in fact the earth must be more rigid than steel in order that the period should be as short as fourteen months. This shows how indirect information on the physical properties of the earth may be obtained sometimes in an unexpected manner, the periodic revolution of the pole leading to an estimate of the average rigidity of the interior of the earth. The total displacement of the pole of the earth from its average position is small, never amounting to more than eight metres. The accuracy with which that displacement can be measured is a testimony to the excellence of our astronomical observations. It is a type of work in which co-operation is absolutely necessary. The subject has received additional interest through the suggestion made by Prof. Milne, in his recent Bakerian lecture, that seismic disturbances may be caused by the changes in the position of the earth's axis. Considering that the distortions in the earth are sufficient to increase the periodic revolution of the pole from ten to fourteen months, this suggestion is well worth investigation, and the 300*l.* per annum spent by this country in support of the work of the Geodetic Association will be well employed if it allows the vagaries of our pole to be more closely studied and all the dimensional quantities of the surface of the earth to become more accurately known.

The contributions received by the Central Bureau of this Association from the participating states amount to about 3000*l.*, and there is a balance which at the end of 1904 amounted to over 5000*l.* The expenditure during 1905 was nearly 5000*l.*, reducing the balance

by 2000*l*. The principal items of the expenditure were formed by contributions towards the maintenance of six stations in the Northern and two stations in the Southern Hemisphere for carrying out the observations relating to the changes of the position of the earth's axis. The whole cost of this service is about 4450*l*. The honorarium of the Secretary is 250*l*., which, together with the cost of printing, postage, and a small item for grants towards special scientific work, makes up the expenditure. No charges are made for office expenses, which are defrayed by the Prussian Government.

The geodetic work indirectly gives us valuable, though only partial, information on the interior of the earth, but it confines itself in the main to the surface of the globe; the investigation of the atmosphere carries us beyond.

In an address delivered to the British Association at its Belfast meeting, in 1902, I expressed the opinion that meteorology might be advanced more rapidly if all routine observations were stopped for a period of five years, the energy of observers being concentrated on the discussion of the results already obtained. I am glad to say that meteorologists have taken seriously a remark, the echoes of which still reach me from distant parts of the earth. They disagree with me, but their disagreement is of the apologetic kind. I do not wish to retract or to weaken my previous statement, but merely now qualify it to the extent that it is only to be applied to two dimensional meteorology. There is a three dimensional meteorology as far removed from the one that confines itself to the surface of the earth as three dimensional space is from a flat area. Three dimensional meteorology is a new science, which at present requires the establishment of new facts before their discussion can properly begin. The extension of our range of observations by kites and balloons is of comparatively recent origin. Mr. Archibald in this country, was one of the pioneers of meteorological investigation by means of instruments attached to kites. In the United States, Mr. Rotch having established a separate observatory, succeeded in convincing scientific men of the great value of the results which could be obtained. Mr. L. Teisserenc de Bort, who established and maintained an observatory for dynamic meteorology at Trappes, near Paris, rendered similar services with regard to "pilot" or manned balloons carrying autographical instruments. The aeronautical department of the Royal Prussian Meteorological Institute, with Dr. Assmann at its head, under the direction of Professor von Bezold, also made a number of important contributions in the early stages of the work. Professor Hergesell, of Strasburg, similarly made numerous experiments, and chiefly through the efforts of those whose names have been mentioned, and more especially Professor Hergesell, an international agreement has been secured by means of which kite and balloon ascents are made in several countries on the first Thursday in each month, and on three consecutive days during two months of the year. A large station for

aeronautical work was recently established at Lindenberg, near Berlin, where kites or balloons are sent up daily for the purpose of securing meteorological records. The greatest height yet reached was during the ascent of the 25th of November, 1905, when by means of several kites sent one after another on the same wire, the upper one rose to an altitude of 6430 metres, almost exactly four miles. Owing to want of funds this country could, until recently, only participate in this work through the individual efforts of Mr. Dines, who received, however, some assistance from the British Association and the Royal Meteorological Society.

The reconstruction of the Meteorological Office has made it possible now for Mr. Dines's work to be continued as part of the regular work of the office, and further stations are being established. Mr. Cave carries out regular ascents at his own expense at Ditcham Park, and through the co-operation of the Royal Meteorological Society and the University of Manchester, assisted by a contribution for apparatus from the Royal Society Government Grant Fund, a regular kite station is being established on the Derbyshire moors.

The International Committee which collates the observations is a commission appointed by a union voluntarily formed between the Directors of Meteorological Observatories and Institutes of countries in which regular observations are taken. The meeting of Directors discusses schemes of observations, and encourages uniformity.

If I mention a few of the difficulties which stand in the way of a homogeneous system extending over Europe, I do it in the hope that it may perhaps ultimately assist in removing some of them. It is obviously desirable that the charts which are intended to show the distribution of pressure and temperature should be derived from observations made at the same hour. Germany observes at eight o'clock of Central European time, and France observes simultaneously (or nearly so) by choosing seven o'clock Paris time for its readings. We observe at eight o'clock Greenwich time, which is an hour later. It is the great desire of Continental meteorologists that our standard hour should be seven o'clock, and what prevents it from being so? Chiefly and absolutely the additional cost which the Post Office must claim for the transmission of telegrams; because messages transmitted before eight o'clock are subject to an additional charge of one shilling which may be claimed by the postmaster, the claim being possibly increased to two shillings when the postmaster and telegraphist are different persons. This is prohibitive, but it does not exhaust the inconvenience of the additional charge. For the purpose of weather forecasting it is clearly necessary that telegrams should be received as early as possible by the Meteorological Office. But the eight o'clock rule delays telegrams from some Irish stations, because eight o'clock by Dublin time is 8.25 by Greenwich time, and therefore Irish telegrams may have to wait until nearly half-past eight if they are to be transmitted without extra charge.

While the international organisation of meteorology is well on its way, though difficulties such as those I have mentioned may temporarily retard it, another question not altogether disconnected with it has been raised by Sir John Eliot. This is the establishment of an institution devoted to the collective study of meteorological problems affecting all parts of the British Dominions. It is true, not only in this but also in other matters, that in order to take our proper position in international work it is necessary that we should set our own house in order, and we must give Sir John Eliot's proposals our hearty support. If I do not enter further into this question, it is because I am now dealing more especially with problems which go beyond the limits of the Empire. I assume the existence of a national organisation, but lay stress on the insufficiency of this limitation.

The importance of the subject, however, may be my justification, if I direct attention for a moment to the meteorological question as it presents itself in India. We all know and realise the vital importance of the rainy season, and the benefit which the native population would derive if it were possible to predict, even if only imperfectly, the setting in of the monsoon. It appears that Dr. Walker, the present Director of Observatories in India, recently obtained very encouraging results in this respect. According to his investigations, a forecast of the monsoon may be derived from a knowledge of the weather during preceding months in different parts of the world. Thus a heavy rainfall in Zanzibar in May is followed by a weak monsoon, while a pressure deficiency in Siberia during the month of March indicates a probable deficiency of rain in India during the following August. I need not insist on the importance of these results, which at present are purely empirical and require further confirmation, but it is quite clear that for the successful prosecution of these inquiries political boundaries must be disregarded, and a system of intercommunication organised between the countries chiefly concerned. Dr. Walker informs me that he has successfully arranged for telegraphic reports to be sent to him at the beginning of June from six different stations in Siberia. It is hoped that this co-operation, which was unavoidably discontinued during the late war, may now be re-established.

The course of international organisations does not always run smoothly. The efforts made toward co-operation in earthquake records have unfortunately led to differences of opinion, which have hitherto prevented a truly international system being formed; and if I give a short historical account of the circumstances which have led up to these differences, it is only in the hope that this may help to remove them. The scientific investigation of earthquakes may be said to have begun when British professors of physics, engineering, and geology, were appointed at the Imperial College of Engineering in Tokio. Some of them on returning home succeeded in interesting

the British Association in the subject. Ever since 1880 that Association has been an active supporter of seismic investigations. The much disturbed region of the Japanese islands was naturally the first to be studied, but, in 1895, Professor Milne, as one of the secretaries of the Committee, issued a circular calling attention to the desirability of observing waves which have travelled great distances, and some months later, Dr. E. v. Rebeur-Paschwitz, of Strasburg, drew up suggestions for the establishment of an international system of earthquake stations. To this scheme Professor Milne and other members of the British Association Committee gave their approval. The co-operation which thus seemed so happily inaugurated was broken by the unfortunate death of its originator. Circumstances then arose which compelled the British Association Committee to go its own way. Under its direction a system was established which now includes about forty stations distributed all over the world. But the needs of different countries are not, and were not meant to be satisfied by this organisation.

There is always a certain number of earthquakes having purely local importance and requiring discussion from a purely local point of view. For the purpose of such discussion relating to the disturbances which chiefly affect Central Europe, the Union (so-called Kartell) of the Academies of Vienna, Munich, Leipzig, and Göttingen formed a committee and did excellent work. In the meantime Professor Gerland, who had succeeded Dr. Rebeur-Paschwitz, at Strasburg, had personally invited a number of friends interested in the subject to a conference at Strasburg with the object of forming an international association. This was followed in 1903 by a formal conference called by the German Government, at which Great Britain was represented by Sir George Darwin and Professor Milne. This conference drew up a scheme for an international association, and a large number of countries, including Russia and Japan, joined. Strasburg was selected as the seat of the Central Bureau. The matter came up for discussion at the meeting of the International Association of Academies, which was held in London in 1904, and a committee was appointed for the purpose of suggesting such modifications in the constitution of the seismic organisation as might bring it into harmony with the views of the Associated Academies. This committee, over which I had the honour to preside, met at Frankfort, and recommended a number of important changes which were unanimously accepted by the second seismic conference held last summer in Berlin. In consequence of this acceptance, it appears that Italy and the United States joined the seismic association, while England declared its willingness to join under certain conditions, of which the simultaneous adhesion of France was one. The following summary of the States which have joined and their population, is copied from the official report of the last meeting at Berlin :—

Country.	Population.	Contribution. £
German Empire	60,000,000	160
Belgium	7,000,000	40
Bulgaria	3,700,000	20
Chili	3,000,000	20
Congo State	19,000,000	80
Spain	19,000,000	80
United States of America	76,000,000	160
Greece	2,500,000	20
Hungary	19,250,000	80
Japan	48,000,000	160
Italy	33,000,000	160
Mexico	13,600,000	80
Norway	2,300,000	20
The Colonies of the Netherlands	5,500,000	40
Portugal	5,400,000	40
Roumania	6,300,000	40
Russia	129,000,000	160
Switzerland	3,300,000	20

It was decided at the Berlin meeting that Professor Kövesligethy, of Budapest, should be Secretary, and Professor Palazzo, of Rome, the Vice-President of the International Seismic Association. Prof. Gerland had already previously been designated as Director of the Central Bureau. The office of President of the Association was left vacant until the final decision of Great Britain as to its adhesion had been settled. There the matter stands for the present.

The disastrous results of recent earthquakes and volcanic eruptions have directed increased attention to the subject. Its thorough investigation is indeed likely to yield important information on the interior constitution of the earth. A hearty co-operation to obtain and circulate the material for a detailed discussion cannot fail to bear fruit, and even though there may be legitimate grounds for dissatisfaction at the manner in which a particular scheme has been organised, I must express my own opinion that at the present moment the permanent interests of this country would be best secured by our joining the association, and helping to direct its work in a manner which would assist rather than hamper the present organisation of the British Association.

I do not like to conclude without mentioning a newly established organisation, which has its central bureau in my own laboratory at the University of Manchester. This is a union for the observation of solar phenomena. Called into being chiefly by the energy of Professor Hale, this association is perhaps unique in two respects. It aims more directly at conducting research work than is the case with other unions, and in so far may run the danger of hampering private efforts. This danger has, I believe, been well guarded against by the constitution adopted at the first meeting of the Conference, held last September at Oxford. The second peculiarity referred to is that it works a central bureau, a computing bureau (under the direction of

Professor Turner), and is going to publish Transactions without any funds beyond those doled out to it by charity. Its vitality will, I hope, help it to overcome its initial troubles. Its ambitious programme includes a definite agreement on the standard of wave-length and investigations on the permanence or variability of solar radiation.

This latter question is of considerable interest to meteorologists, and comes therefore within the purview of the Directors of Meteorological Observatories, who have also, under the presidency of Sir Norman Lockyer, established a commission charged with its discussion. An arrangement has been made securing co-operation between the two bodies, the Solar Union leaving out of its programme the difficult question of the relationship between sunspot variability and meteorological phenomena.

Although an unnecessary overlapping of two separate enterprises has in this instance been avoided, such overlapping constitutes a certain danger for the future, as the problems of geo-physics—for the investigation of which international associations are specially marked out—are so intimately connected with each other, that a homogeneous treatment would seem to require a central body supervising to some extent the separate associations. Such a central body may be found in the International Association of Academies, which promises to play so important a part in scientific history that a short account of its early history may be of interest. The Kartell of some of the German Academies and that of Vienna has already been referred to. In discussing the utility of its deliberations Professor Felix Klein, of Göttingen, first mentioned to me the idea, that an association of a similar nature would be likely to prove of still greater value, if formed between the scientific and literary academies all over the world. In consequence of this conversation I tried to interest the Royal Society in the subject; and in order to obtain further information Professor Armstrong and myself attended privately, though with the knowledge and consent of the Council of the Royal Society, the meeting of the Kartell which was held at Leipzig in the year 1897. In the following year the two secretaries of the Royal Society, Sir Michael Foster and Sir Arthur Rücker, together with Professor Armstrong and myself, attended the Kartell which then met at Göttingen. The secretaries were impressed by the great possibilities of the scheme, and the Council took the initiative and approached the Academies of Paris and St. Petersburg, which both returned favourable answers.

In consequence of the correspondence between these learned societies, the Royal Academy of Berlin in conjunction with the Royal Society of London, issued invitations for a general conference to be held at Wiesbaden on the 9th and 10th of October, in the year 1899.

The following were represented at this meeting, at which the statutes of the new association were agreed upon :—

The Royal Prussian Academy of Sciences of Berlin.
The Royal Academy of Sciences of Göttingen.
The Saxon Academy of Sciences of Leipzig.
The Royal Society of London.
The Royal Bavarian Academy of Science of Munich.
The Academy of Sciences of Paris.
The Imperial Academy of Science of St. Petersburg.
The National Academy of Science of Washington.
The Imperial Academy of Sciences of Vienna.

The unanimity of the meeting may be judged from the fact, that a working constitution, which subsequent experience proved to be eminently effective, was finally arrived at on the second day. Many distinguished men took part in the discussions, amongst them Professor Simon Newcomb and the late Professor Virchow may be specially mentioned.

Although the Berlin Academy had never joined the German Kartell, the first idea of a wider association seems to be due to a distinguished member of that body, the historian Mommsen, who though of advanced age, was able to be present at the first regular meeting of the Association, which was held at Paris on April 16–20, 1901. In addition to the societies which took part in its foundation, the following form part of the Association and were represented at Paris :—

The Royal Academy of Sciences of Amsterdam.
The Royal Belgium Academy of Sciences, Arts and Letters.
The Hungarian Academy of Sciences.
The Academy of Sciences of Christiania.
The Academy of Sciences of Copenhagen.
The Academy "des Inscriptions et Belles Lettres" of the Institut de France.
The Academy of "Sciences, morales et politiques" of the Institut de France.
The Royal Society "dei Lincei" of Rome.
The Royal Swedish Academy of Sciences.

This meeting is not likely to pass out of the memory of those who took part in it. Its importance was enhanced by the social functions which were held in connection with it, and which included a luncheon given by President Loubet, at the Elysée, a banquet given by the Conseil Municipal, and a special performance at the Théâtre Français. The subsequent triennial meeting of the Academy which was held in 1904, passed off not less brilliantly. The representatives of the learned societies were received by their Majesties at Windsor, and the Lord Mayor invited them to dinner at the Mansion House. Social engagements, though welcome as marking the importance of the occa-

sion, are not allowed to interfere with the very substantial work which is being done at these meetings. The list of subjects included in the discussion of the London assembly gives an idea of the activity of the association which does not stop at the conclusion of the meetings, but is kept alive by the work of its members. A permanent committee was charged with the investigation of the functions of the brain, and others were appointed to deal with questions of atmospheric electricity and of the measurement of magnetic elements at sea. An important proposal to carry out an exact magnetic survey along a complete circle of latitude is under discussion. The section of letters dealt with the mutual arrangements between libraries regarding the interchange of manuscripts, approved the intended edition of the Mahabharata, and considered a proposal to construct a new Thesaurus of Ancient Greek. The association also took cognisance of and received reports on independent international undertakings, such as the catalogue of scientific literature, the Geodetic Association, and the Geological Congress.

The association meets every three years. To these meetings each constituent academy may send as many delegates as may be found convenient. For the discussion of special questions the assembly divides itself into a scientific section and a literary section.

In each of these sections, as well as in the plenary meetings comprising both sections, each academy has only one vote. At each triennial assembly, the next meeting place is chosen. In the intervals between the meetings the affairs of the Association are placed in the hands of a council on which each academy is represented by two members or one, according as it comprises both a literary and scientific section or only one of them. The resolutions passed by the Association are not binding on the constituent academies, who maintain their liberty of adopting or rejecting them.

The Association of Academies suffers unavoidably from a certain want of homogeneity, owing to differences in the constitution of its component bodies. Most Continental academies contain both literary and scientific sections, and at the organising meeting held at Wiesbaden, marked attention was drawn to the fact that there was no body in England that could be considered as representative of literary studies. If matters had been left as they stood then, this country would have been altogether unrepresentative as regards half the activity of the Association. Efforts were made, in consequence, to take a more liberal view of the branches of knowledge coming within the range of the Royal Society and to include literary subjects. Very unfortunately, in my opinion, these efforts failed, and a charter was granted to the British Academy, which has now been included as a separate body among the list of academies forming part of the Association. While in this respect we have been at a certain disadvantage, the constitution of the Royal Society has the great advantage of being truly representative of the Empire. In France, on the other hand, no

one can belong to the Academy of Sciences who is not domiciled in Paris. Similarly although Germany possesses four Royal Academies (Berlin, Göttingen, Leipzig, Munich), each of them is confined as regards ordinary members to its own locality ; so that a Professor of the Universities of Bonn or Heidelberg, however eminent he may be, could not become a member of any of these Academies. Neither in France nor in Germany can the Academy therefore be called truly representative. The disadvantages which may arise from this defect have been minimised by adopting a rule that the International Association of Academies may appoint Committees for the discussion of special questions, and that members of these Committees need not be members of any of the constituent academies. This to a large degree obviates what would otherwise be a considerable difficulty. Nevertheless I believe that the circumstances to which I have drawn attention, form the only impediment in the way of handing over to the Association of Academies the ultimate control of every new international undertaking and even the charge of some of those already established. It is highly desirable that we should work towards this end. An energetic enthusiast may easily start a new enterprise, and governments are appealed to from different sides for help and support. There ought to be some authoritative body to whom the governments could apply for advice. Overlapping and waste would be thus avoided.

It is not my desire to disguise the difficulties which have sometimes been encountered in providing for joint undertakings on a large scale. Whether national or international, combined work between men of different temperaments always requires some suppression of personality. Even stronger feelings may be involved when a central office or bureau has to be selected which specially distinguishes one locality. The advantage gained by the locality is often one of appearance rather than of reality, for these central offices should be the servants rather than the masters of the undertaking. In order to prevent national feeling being aroused by any preference given to one nation, it has been customary to select a president belonging to a different country from that of the director of the Central Bureau ; there are also a vice-president and a secretary—all belonging to different nations. It is thought that such a distribution of office may assist in preserving harmony. I believe that this is the case, but sometimes at the risk of impaired efficiency. It cannot be denied, however, that the seat of the central office of an important undertaking confers a certain dignity, and it is quite natural that a country should feel some pride in the distinction.

England as a whole has not done so badly. We should not forget that in a great portion of the world, all clocks strike the same minutes and seconds. Before long all civilised countries (except Ireland) will have adopted the Greenwich meridian for their standard of time, and we may rightly therefore call Greenwich the central bureau of universal time. The offices of the International Catalogue and both the central

and computing bureaux of the Solar Union are situated in this country, and if we have secured an even larger share of the onerous but honourable duties incumbent on such offices, the fault is our own. The questions which at the present moment more especially require combined treatment are those of Geo-Physics, a subject for which very inadequate provision has been made in England. Our earthquake observations almost entirely depend on the self-devotion of one man, and the Meteorological Office, which might reasonably be expected to take charge of certain portions of the work such as atmospheric electricity, being kept in a state of chronic poverty, must restrict its activity to work of the most pressing necessity. Germany, on the other hand, having a large number of well equipped stations for geodetic, magnetic, and aeronautic work, naturally reaps the reward when the offices of an International undertaking have to be chosen which shall be attached to flourishing institutions in charge of men possessing the leisure and qualifications for the work.

No serious advance will be made in our own country in this respect until our universities pay more attention to the subject of terrestrial physics. This would involve the establishment by the universities of separate laboratories or institutions, to which their present funds could not be applied. The matter wants consideration in detail and should be carried out according to a homogeneous scheme which would prevent wasteful repetition in different places. But I feel certain that until we have trained up a number of students who possess an adequate knowledge of questions of meteorology, geodetics, terrestrial magnetism, and seismology, the position which this country will take in international organisation cannot be a leading one, though it may be, and, indeed, owing to private efforts, is at the present moment, one of which we need not be ashamed.

Finally I must lay stress on one aspect of the question which I hope may induce us to attach still greater importance to international undertakings. The co-operation of different nations in the joint investigation of the constitution of the terrestrial globe, of the phenomena which take place at its surface, and of the celestial bodies which shine equally upon all, directs attention to our common interests and exposes the artificial nature of political boundaries. The meetings in common discussion of earnest workers in the fields of knowledge tend to obliterate the superficial distinctions of manner and outward bearing which so often get exaggerated until they are mistaken for deep-seated national characteristics.

I am afraid I have only given a very inadequate account of the serious interests which are already involved in international scientific investigations. But if I may point once more to Indian meteorology and insist on the vital importance of an effective study of the conditions which rule the monsoon, everyone will I think, realise how impossible it is to separate scientific from national interests. The solution of this particular problem requires an intimate co-operation with Central Asia

and Siberia—a co-operation which has been easily secured. I do not wish to exaggerate the civilising value of scientific investigation, but the great problems of creation link all humanity together, and it may yet come to pass that when diplomacy fails—and it often comes perilously near failure—it will fall to the men of science and learning to preserve the peace of the world.

[A. S.]

WEEKLY EVENING MEETING,

Friday, May 25, 1906.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Leonard Hill, Esq., M.B. F.R.S.

Compressed Air and its Physiological Effects.

Compressed air is used in all the great subaqueous works of to-day, in tunnelling, pier sinking, bridge building, diving, etc. All such works are limited to a certain depth by the pathological effects produced on the workers.

The naked diver preceded the diver who uses compressed air. The body of the naked diver is pressed upon by the water, equally and in all its parts, by a pressure equal to + 1 atm. (15 lb. per sq. in.) for every $33\frac{1}{2}$ ft. (10·3 m.) of depth. He fills his lungs before, and holds his breath during, the dive. The air in his lungs must be compressed to half its volume at $33\frac{1}{2}$ ft. (2 atm. abs.), to one-third at 67 ft. (3 atm. abs.), to a quarter at $100\frac{1}{2}$ ft. (4 atm. abs.), and so on.* The depths attained are usually not greater than 60–70 ft.

The duration of his stay under water is limited by the oxygen-carrying power of his blood. This may become greater by practice. Diving birds have double the normal volume of blood (Bohr). The diver who uses gear, or the caisson worker, is surrounded with compressed air and breathes freely in it. The body of either is pressed upon by the air, and the air pressure must always be just greater than that of the water to keep the latter out of the helmet, bell, or caisson. Whether it be air or water that *uniformly* presses upon the body the tissue fluids transmit the pressure equally throughout the body, and thus although it is computed that + 1 atm. means an additional total pressure of 15,000 to 20,000 kilograms (40,000 lb.) on the body of a man, no mechanical effect is produced. Living matter is a colloidal solution, containing about 80 per cent. of water, and is practically incompressible.

The theory, put forward by many medical writers, that congestion

* The squid eating whale, according to the Prince of Monaco, finds its feeding grounds at depths of 4000 ft. Is the volume of air in its lungs compressed to the 100th part?

of the blood in the deeper parts of the body can be induced by variations of atmospheric pressure, is contrary to physical principles, and is quite untenable.

I can refute it by the following experiment: A frog's web is stretched over the glass window of a pressure chamber, and is illuminated by the arc light, so that the circulation of the blood is projected on the screen. The circulation remains unchanged when the pressure is raised to 20 atm. Manometric records of blood pressure in mammals also show no noteworthy change.

That mere mechanical pressure uniformly applied is of little importance to living matter is shown by the existence of life in the greatest depths yet sounded, where the superincumbent pressure may equal 2 to 3 and even 5 miles of water.

The use of compressed air for submarine work was a matter of slow development, owing, not to lack of invention, but to want of efficient air pumps and flexible tubes. The oldest invention is that of a pipe conveying air from the surface to the mouth of the diver. Such a device cannot be used at any depth, because the body is pressed upon by the water plus the atmospheric pressure, while the lungs are exposed to the atmospheric pressure alone. The pressure of the water on the body makes breathing difficult and congests the blood within the lungs. The cupping glass demonstrates the congestive effect produced by lessening the atmospheric pressure at one part of the body only.

The same bad conditions pertain to the water-tight metal helmet, combined with leather dress, suggested by Borellus (17th cent.), the diver being supposed to live on the air in the helmet.

Bernouilli formulated the correct theory that the diver must either be supplied with air at the pressure of the water surrounding him, or that Borellus' helmet must be made of leather so that the air within can be compressed by the water. The Venetians (17th cent.) pumped air into a diver's helmet with a bellows, and thus forestalled by two centuries the modern diving dress of Siebe, Gorman and Co.

Any one who pushed an inverted glass under water and saw it did not fill, would conceive the idea of a diving-bell.

Sinclair (1665) fashioned a simple wooden bell to recover treasure from an Armada ship off Mull. At 33½ ft. the air in such a bell is compressed to half its volume, and this, together with lack of ventilation, rendered such a bell of little use.

Halley, the astronomer, used a pipe and bellows for shallow work, while for deep work, when his bellows failed, he sank a cask full of air to a deeper level than the bell. From the cask to the bell passed a tube, and the water entering the cask through a hole displaced the air into the bell. He descended to 9 to 10 fathoms with four others, and used up 7 to 8 barrels of air.

With the building of efficient air-pumps, Smeaton (1778) applied the bell to the important use of building the piles of bridges. Triger

(1839) applied it to the sinking of excavation through quicksands, and the bell became evolved into the modern caisson—a steel chamber provided with a cutting edge below, and an air-lock above for allowing the men to enter and leave without raising the bell. Finally the caisson was applied to the purpose of horizontally tunnelling under rivers. To effect this a steel shield provided with cutting edge, is driven forward by hydraulic jacks. Screens are placed in the shield to allow excavation of the soil in front of it. As fast as the shield is driven forward, segments of the iron tunnel are built into place. Water is kept out of the work by the use of compressed air. On entering, the men are "compressed" in the air-lock, i.e. the air-pressure is raised to that in the tunnel, and on leaving the tunnel they are "decompressed," i.e. the air-pressure is lowered in the lock down to the normal, so that the outer door of the lock may be opened.

A diver is "compressed" on descending into the water, as the pressure of his air-pump always keeps up to that of the water. On coming up he is "decompressed."

The workers in compressed air from first to last have suffered from illness and loss of life. The higher the pressure, the greater the loss. Thus there occurred at :—

Douchy mines (shaft sinking) ..	63 cases (Pol and Watelle).
Kehler bridge (Rhine)	133 cases (François).
Adour bridge	90 per cent. of workers (Limousin).
St. Louis bridge (Mississippi) ..	119 cases out of 352 workers; 50 cases of paralysis; 14 deaths (Eads, Jaminet).
Brooklyn bridge	110 cases in 4 months and 3 deaths (Smith).
Toulon dry dock	43 cases in 3 weeks and 2 deaths.
Cubsac bridge	104 cases and 3 deaths (Gerard).
Eider bridge	38 cases and 2 deaths (v. Haller).
Nussdorf works (Danube) ..	320 cases among 675 workers (v. Schrötter).
Felesti bridge (Danube)	55 cases and 5 deaths among 154 workers (Tinc).
Hudson tunnel	1 man died a month among 50 workers, until the conditions were altered (E. W. Moir).

At New York, Mr. E. W. Moir tells me, 8 men have died in the last 6 months, and there have been many severe cases besides.

Many cases of illness and death occur also among deep-sea divers. Catsaras (1890) published accounts of 70 cases collected from the Greek sponge divers. He averaged the deaths as 10 per annum in the sponge fisheries of Hydra. Compressed air sickness is characterised by its protean symptoms. Catsaras records cases of loss of speech, blindness, deafness, transitory madness, vertigo, loss of consciousness, subcutaneous emphysema, spinal paralysis, etc.

V. Schrötter, at Nussdorf, observed 68 cases of ear trouble, 105 of pain in the muscles, 60 of pain in the joints, 10 of girdle pains, 17 of partial paralysis, 26 of paralysis of the lower half of the body, 14 of vertigo and noises in the ears, 2 of sudden deafness, 1 of loss of speech, 13 of asphyxial phenomena.

None of the symptoms, with the exception of some slight ear trouble, ever occur while the men are under pressure. Mules lived about a year in the Hudson tunnel, and were healthy enough to kick and bite at all comers (Moir). The illness comes on after decompression, usually within a few minutes to half an hour, sometimes even later. The trouble in the ear, which occurs during compression, is due to the inequality of air-pressure on either side of the drum of the ear. It is relieved at once by opening the Eustachian tubes by swallowing, or, if this is not enough, by a forced expiratory effort with the nose and mouth shut.

Many and conflicting were the theories of compressed air illness, and in the directions given to avoid it. Some medical men (Pol and Watelle) recommended slow, and others, like Foley, rapid decompression. All was made clear by a remarkable series of experiments carried out by Paul Bert on animals between 1870–1880. By these experiments he not only proved the cause, but found the means of prevention.

Bert showed that nitrogen gas is dissolved by the blood and body fluids in proportion to the pressure of the air, and that the gas bubbles off in the blood, when an animal or man is rapidly decompressed. The bubbles may block up the capillaries in one or other part of the body, and by cutting off the part from blood supply, produce one or other of the symptoms mentioned above. The illness is prevented by making the period of "decompression" sufficiently slow, i.e. by allowing time for the dissolved nitrogen to escape from the lungs.*

Bert's experimental results have been confirmed and extended by B. Blanchard and P. Regnard, Catsaras, Philippon, Layet and Hersent, H. v. Schrötter, R. Haller and W. Mager, and by myself and my colleagues, J. J. R. Macleod, C. Ham, and M. Greenwood. The whole matter in consequence is placed on a sure footing.

Exposed to 1 atm. at body temperature, blood dissolves just about 1 per cent. N_2, to 2 atm. 2 per cent., to 3 atm. 3 per cent., and so on. The tissue fluids take up the dissolved gas from the blood, and with

* Boyle, 200 years previously, had shown that gas bubbles appeared in the humours of the eye of a viper, when submitted to a rapid evacuation under the air pump. Hoppe Seyler, repeating Boyle's experiment, found gas bubbles collected in the veins of a mammal, which was very rapidly lowered to 70–80 mm. Hg.; the bubbles caused convulsions which might be stayed by the introduction of hydrogen at atm. pressure. The asphyxial convulsions which arose from want of oxygen occurred 2 minutes later. Rapid evacuation to 75 mm. Hg. in an atm. of oxygen caused convulsions, no less, owing to the setting free of nitrogen bubbles.

time the whole body becomes saturated, according to Dalton's law. From his analyses Bert concluded that the absorption of N_2 lagged behind the requirements of Dalton's law. This was due to his taking too high a figure for the coefficient of absorption of N_2 in blood. The saturation of the body fluids must take time, since the blood forms but 5 per cent. of the whole body weight (Haldane), and it is the blood alone that comes in direct contact in the lungs with the increased atmospheric pressure. Probably about 5 litres of blood circulate through the lungs per minute, and this blood conveys the absorbed nitrogen to the 30–40 litres of water which are in the body. The arterial blood saturated in the lungs yields the nitrogen to the tissues, and returns to be saturated again in the lungs. Those tissues, which are plentifully supplied with blood, will become saturated rapidly, while less vascular areas, and parts in a state of vaso-constriction, will saturate very slowly.

C. Ham and I exposed rats to 10–20 atm., killed them by instant decompression, and then mincing their bodies under water, collected and analysed the gas set free therein. We obtained in this gas CO_2 6·7—16 per cent., O_2 2·1—8·7 per cent., N_2 80—87 per cent., and a volume of N_2 even greater than that calculated by Dalton's law. The excess we found was due to gas swallowed while under pressure. Most of the gas, set free on decompression from such high pressures, is free in the peritoneal cavity and alimentary canal.

M. Greenwood and I have tested upon ourselves the rate of saturation, using the urine as a test fluid. We were compressed in a large boiler, placed at our disposal by Messrs. Siebe, Gorman and Co. The chamber was fitted with electric light and telephone, and taps for slow decompression. The pressure was raised by means of a diving pump driven by a gas engine. We drank a quart of water before entering, and collected samples of urine at varying pressures and times. The urine, collected in sealed bulbs, was evacuated by my blood gas pump. We found the urine secreted in the next 10 minutes after reaching any given pressure is saturated with N_2 at that pressure. Oxygen as well as nitrogen is dissolved, but this is of no importance, because the oxygen chemically combines with the blood and tissues on decompression.

To demonstrate the bubbling off of nitrogen on rapid decompression, I have spread the web of frog's foot or wing of bat over the glass window of a pressure chamber. The circulation of the blood is projected on a screen with aid of microscope and arc light. We can thus observe the circulation under 20 atm. of air, and watch the bubbles forming in the capillaries on rapid decompression. The compression diminishes the size and finally drives the bubbles again into solution.

When the larger mammals are exposed to high pressures, such as 8 atm., for an hour or so, and are then rapidly decompressed, they usually die in a few minutes. Small mammals, such as mice and rats,

may escape, owing to the small bulk of body, and great rapidity of circulation, this being sufficiently rapid to clear out the nitrogen. Young animals also escape. After the decompression, the animals show signs of sensations in the limbs, which they lick or bite at, paralysis in the limbs follows, and then the animals fall over and become unconscious. Noise of gas bubbles gurgling in the heart may be heard. Respiration becomes embarrassed, and the animals die. On dissection, the peritoneal cavity may be found distended with gas, or the stomach, and bubbles of gas may be seen in the intestine. A part of this gas arises from the fermentative processes of digestion, and from air swallowed during compression. The veins of the portal system, the venæ cavæ, etc., are seen to contain chains of bubbles: the heart is full of froth. Small hæmorrhages may be present in the lungs. The edges of the lobes of the lung are emphysematous, blown out by the rapid decompression. The fat everywhere is full of small bubbles, so too are the connective tissues. Bubbles are seen in the joints, and may appear in the aqueous humour of the eye. On opening the skull, bubbles are seen in the veins of the brain. The bubbles are not restricted to the veins, but may also be seen in the arteries. The coronary vessels of the heart often show chains of bubbles. On microscopic examination, the bubbles are seen in the capillaries; here and there they run together and form larger bubbles, sometimes rupturing the walls of the vessel, and compressing the surrounding tissues. The bubbles appear in the lymph spaces and lymphatics equally with the blood system. I have never seen them actually within the substance of a cell.

The gas set free in the heart can be collected and analysed; about 80 per cent. of it is found to be nitrogen (Bert, v. Schrötter, Hill, and Macleod). Catsaras lowered dogs in a diving dress to depths of 43·7 m., and after about an hour rapidly drew them to the surface. He found bubbles set free in these dogs just as in those exposed in a pressure chamber. In one dog which escaped without any severe symptoms, gas bubbles were found in the veins six hours later. This shows how long it may take for nitrogen gas once set free as bubbles to escape from the lungs, and explains why caisson workers may suddenly be seized some hours or more after leaving the works. In such cases the bubbles may be swept from the abdominal veins—where they do no harm—into the heart, and impede the action of this organ, or they may penetrate the pulmonary circulation, and enter the arterial system, and block up, perchance, the coronary arteries, or others in the brain or spinal cord.

V. Schrötter placed an animal for 1 hour in 4·5 atm., and then decompressing it in 30 seconds, watched the appearance of bubbles in the mesenteric vessels. Spasms occurred in 5 minutes (provoked by bubbles in the spinal cord) ; visible bubbles appeared in the veins 2 minutes later. The blood is a colloidal solution, and it takes time for the nitrogen to come out of solution and for the small bubbles to

run together to form visible bubbles. We find colloidal solutions form super-saturated solutions after exposure to high pressures. After decompression the gas does not come off until the solutions are shaken. I surmise that the gas molecules are tacked on to the colloidal particles, and that skins of proteid antagonise the union of the gas molecules with each other.

The gas bubbles tend to collect in the veins, as the blood travels quickly through the arteries and slowly in the veins. The higher blood pressure in the arteries, I think, cannot appreciably influence this. It is only when the gas in the veins becomes sufficient in amount to produce foam in the heart, or when gas bubbles block up arteries of vital import, that grave symptoms arise. The place where bubbles in the arteries must always produce serious results is the central nervous system. In the liver, kidneys, muscles, fat, etc., bubbles may embolise small arteries and produce no grave effect, but in the spinal cord the interruption of the blood supply to any group of cells or tract of fibres, is evidenced at once by pain and anæsthesia, spasm and paralysis. In the medulla oblongata arrest of the circulation will stop respiration, and bubbles lodging there may produce immediate death. Lodging in the arteries of the great brain, bubbles may produce hemiplegia, aphasia, blindness, or mental disturbance.

The explanation of the idiosyncrasy of different animals in this respect is not altogether clear. Small animals, such as rats and mice, and young animals generally, escape owing to the rapidity of respiration and circulation, which frees their bodies from nitrogen. But among larger animals and among men some are affected and others not. We can look for an explanation in the varying state of that colloidal solution, the blood, in the varying vigour of the circulation and respiration and the effect of fatigue, in vaso-motor changes which alter the relative volume of circulating blood in viscera and muscles, and possibly to a minor degree in the fermentative processes going on in the alimentary tract. The young man who is in perfect health, with powerful heart and deep respiration, can expel the dissolved nitrogen from his lungs far more rapidly than the old, the intemperate, or one who is over-fatigued by excessive labour. The records of caisson works seem to show that men under 20–25 years escape ; that long shifts increase the number of cases ; that men who work the air-locks, passing material through, and undergoing frequent and short lasting compression and decompression, are not affected. The longer the shift the more complete the saturation of the body ; the higher the pressure the greater the risks and the graver the symptoms. The records show that practically no cases occur with a pressure below $1\frac{1}{2}$ atm., even though the decompression period be made only a minute or two.

At the Rotherhithe tunnel, now building, the decompression period is 3 minutes, and the pressure + 22 lb. No cases of any

gravity have yet occurred. Nevertheless we have proved that the workers have excess of nitrogen in their bodies after decompression. We have given them a quart of beer to drink in the tunnel 30 minutes before decompression to provoke diuresis, and have made them empty their bladders just before, and again 10 minutes after, decompression. Their urine yielded more than the normal volume of N_2 (1·6 per cent. in place of 1·2 per cent.). The urine, passed immediately after their decompression, obviously effervesced.

The saturation of the body fluids with N_2 probably follows a curve, at first steep in ascent and then slowing off. On decompression the curve of desaturation is probably the reverse—at first steep in descent and then slowing off. Thus we find our urine not saturated with N_2 when we reach say + 3 atm., but becoming saturated in the next 10 minutes. Again, we find on examining, that even after allowing 20 minutes per atm. for decompression, there is more N_2 than normal in our urine. Thus the urine secreted in the next 10 minutes following decompression may contain 1·6 to 2 per cent. in place of the normal 1·2 per cent.

The records of caisson works seem to show that bad ventilation of the caissons increases the cases of illness. E. W. Moir, in particular, has laid great stress on this, and has attributed caisson illness to excess of carbon dioxide in the air breathed. Snell, the medical officer at the Blackwall tunnel, accepted Moir's views, and said that to avoid caisson illness it is necessary to keep the CO_2 in the air under 0·1 per cent., and this is actually being done now at the Rotherhithe tunnel. To maintain so perfect a ventilation as that means an enormous supply of compressed air, and entails great expense. The vast hall at Rotherhithe full of compressor engines is a most impressive sight.

The work of modern physiologists is opposed to this view (Haldane). Mr. Ham and I have exposed animals in compressed air to a partial pressure of CO_2, equal to 10 per cent. of an atm. Mr. Greenwood and I frequently have no ventilation going in our chamber, because we dislike the noise of the pump, and are decompressed from air containing as much CO_2 as 2 per cent. of an atm. I have breathed comfortably for half an hour in Bamberger and Bock's life-saving dress, wherein the CO_2 rose to 3 per cent. No harm can result from the breathing of percentages of CO_2 far higher than Snell's figure of 0·1 per cent.

Other sources of the ill results of bad ventilation must be looked to. First and foremost is the possible pressure of carbon monoxide (CO), in caissons where flare-lights, furnaces, and blasting charges are used, or where low-flash oils are employed to lubricate the compressors. CO has an affinity for hæmoglobin 130 times as great as O_2, and a very small percentage may have a marked effect. This effect may not be manifest in the caisson where the partial pressure of O_2 is high, but may first become manifest on decompression. For Haldane has

shown that animals, whose hæmoglobin is saturated with CO, can live in 2 atm. of pure oxygen. So men with blood one third saturated with CO would be unaffected in $\frac{2}{3}$ atm. of oxygen, i.e. about 3 atm. of air, but would become affected on decompression. If during and after decompression the O_2 supply is diminished by CO, and the heart enfeebled thereby, less N_2 will be given off, and the men may be affected both by want of O_2 and by N_2 bubbles. The immediate success of recompression, which E. Moir has had in recent cases at the New York tunnels, suggests to me that CO may contribute to the numerous accidents there, especially as, Mr. Moir tells me, 20 minutes are allowed for decompression. He says that the men who use the diving-bells at the Dover works are free from illness, in contrast to caisson workers working at the same pressure. Here again the purity of the air from CO is possibly one of the causes of the Dover immunity.

The excessive heat and saturation of the air with moisture of caisson works induces fatigue. The heat-regulating mechanism, the heart and respiration, is sorely tried in keeping the body temperature normal, while a man is doing heavy work at 80° F., and in air completely saturated. The loss of sweat is enormous, but this by drying the muscles may be of advantage by lessening the total volume of body water and so of absorbed nitrogen.

Any cause, then, which depresses the vigour of the workman increases his risk from decompression. Hence the beneficial effects of short shifts and ample ventilation.

The men chosen for high-pressure work should be young, spare, and wiry, and in perfect health. The man of spare habit has less water in his body to take up N_2. The man with powerful heart and ample respiration can get rid of the dissolved N_2 most readily. Some men are more fitted than others, as experience shows, but no immunity to caisson illness can be established by habitual work in compressed air.

The experimental results obtained by rapid decompression have unfortunately been amply confirmed on man by the results of explosion of caissons, or the air pipes of divers, on more than one occasion. I recall the case of three workers who were sinking a well in a brewery and working at + 2·5 atm., when the air pipe burst. The men were found dead. On dissection, bubbles of gas were visible in all parts of the connective tissues and blood vessels.

The post-mortem examination of fatal cases of caisson and divers' paralysis are now fairly numerous (v. Leyden, Van Rensselaer, Nikiforoff, Sharpless, v. Schrötter, etc.). They show lesions in the spinal cord, areas of degeneration and actual destruction brought about by bubbles here or there blocking capillaries and cutting off the blood supply. The figures of such lesions shown on the screen make evident the terrible risks run by compressed air workers.

The Prevention of Compressed Air Illness.

Bert, from his experiments on animals, concluded that all trouble could be avoided by extending the decompression period to 30 minutes for 2–3 atm., 60 minutes for 3–4 atm.

This ruling of Bert has never been carried out at any of the English or American works, a minute or two (at most 15 minutes) being the usual period allowed for "leaking out." V. Schrötter, from his experiments, concluded that 20 minutes per atm. was a safe period, and I have found this to be uniformly safe for a large number of animals which I had exposed to saturation for some hours at a time.

On the Continent the decompression period has been made nearer to that demanded by Bert, and yet severe and even fatal cases have not been avoided. Thus at the Limfjord works two deaths occurred, the pressure being + 3·5 atm., and the decompression period 45 minutes. At Cubsac there were sixteen severe cases and three deaths, the pressure being + 2 to + 3 atm., and the decompression period 25–30 minutes; at Nussdorf two deaths, the pressure being + 2·3 atm., and decompression period 25–30 minutes.

In discussing these cases we are uncertain whether the men obeyed the rules, or were in truth more rapidly decompressed. Snell suggests the men disobeyed the rules at Blackwall. The decompression rate, too, may not have been uniform. It is especially of importance not to hurry the last atmosphere, because any gas free in the blood doubles its volume on dropping from 2 to 1 atm. (Haldane). This is just the period which the men might think it safe to hurry. I think it important that all parts of the body should be moved in turn and often during decompression, so as to drive the blood back to the heart, and increase the rate of circulation and depth of respiration.

Can Work Safely be Extended to Greater Depths?

In caisson works the sickness has been so great at the higher pressures that no engineer has dared to use more than + 3·5 atm. (115 ft.). Divers do more than this; the best pearlers and sponge divers reach 140 ft. Lambert salved 100,000*l.* at a depth of 160 ft. Erostabe salved bullion at 171 ft. These men stayed but 20 minutes below at a time, and took 20 minutes to ascend. Lambert was stricken slightly with paralysis after his last ascent, when he had stayed down longer looking for the last box of gold.

Hersent experimentally compressed a workman to just over 6 atm. (76·8 lb.) for 1 hour, giving him 2 hours 25 minutes for decompression. No ill result followed. I have been compressed to the same pressure, while my colleague, Mr. Greenwood, has reached a pressure of over 7 atm. (92 lb.), corresponding to a depth of 210 ft.

Our object has been to prove that such high pressures can be endured safely if the decompression period be made long enough, to study on ourselves the rate at which the body absorbs nitrogen on compression and gives it up on decompression, the effect of compressed air on the respiratory exchange and general metabolism of the body, and the effect on the sensations and mental state. We find the voice becomes high pitched and nasal in tone, and loses its individual character. The pitch rises with the pressure. The fine vibrations of the lips which cause whistling and whispering cannot be produced in the dense air at 4 atm. The mechanism of respiration and the output of CO_2 as far as we have tried them are unaltered,* and no noteworthy change occurs in the circulation. We had no sense of the pressure and could not estimate its height. Mr. Greenwood after decompression from 92 lb. suffered from pains in the forearms, which were of some severity and lasted a few minutes. On one occasion I had some small patches of ecchymoses in the subcutaneous fat of the chest. Otherwise, except nervousness, we have endured no symptoms of any note.

The Relief of Compressed Air Illness.

Recompression was a method which naturally suggested itself to the earliest workers in caissons (Pol and Watelle, 1854). Bert showed the value of it experimentally, and v. Schrötter, and Macleod and myself have demonstrated the same. In the frog's web, as I have shown you, the bubbles go into solution on recompression, and the circulation may even recommence. If bubbles have formed in the nervous system, the recompression must be carried out quickly, for anæmia lasting some minutes will produce death of the nerve cell. In the case of bubbles embarrassing the heart, recompression may immediately and entirely relieve the symptoms.

Smith, at Brooklyn, and E. W. Moir, at the Hudson tunnel, introduced a recompression chamber, or "medical lock," for the workmen, and Moir has seen many men entirely restored from coma, etc., by recompression followed by slow decompression. A medical lock is now the rule at all works. The men should live in barracks at the works, so that the medical lock may be always at hand, for they often are not affected till some half hour or so after leaving the caisson. If the recompression is delayed it can do little good.

The safety of divers and caisson workers can only be assured by increasing the period of decompression. For pressures above +1·5 atm. the locking out period should be extended. For pressures of

* To study the respiratory exchange we have used the ingenious method of Geppert and Zuntry, and Haldane's method for measuring the tension of CO_2 in the lungs. The CO_2 tension regulates the ventilation of the lungs, and we find this regulation continues to act up to 6 atm. There is the same partial pressure of CO_2 in the lungs at 6 atm. as at 1 atm.

+ 2–3 atm. I should be sorry to be decompressed at any rate quicker than 20 minutes per atm. The lock should be made a comfortable room, warmed and lighted. The men (during decompression) should carry out muscular movements of all parts of the body, so as to hasten the circulation and carry the blood quickly through the lungs.

A band of men trained in patience to be so decompressed, might carry out tunnelling operations at a depth hitherto not dared by the engineer. For deep-sea divers I have designed a bell which the diver can enter at the bottom of the sea, and in which he can be slowly and safely decompressed after the bell has been raised on deck. With such an apparatus, I believe it will be possible to salvage wrecks at depths of 200 ft.

I now turn to the consideration of the influence of high pressures of oxygen on life. Contrary to preconceived opinion, we find the rate of combustion of an animal cannot be accelerated, as that of a fire can be, by increasing the supply of oxygen. The living tissues set their own rate of metabolism, and neither the inhalation of oxygen nor exposure to compressed air can be used as a therapeutic agent to increase the normal rate of tissue change. It is only cases where the oxygen supply is deficient—such as severe anæmia, carbon monoxide poisoning, congenital disease of the heart, pneumonia, etc.—where the tissues, and in particular the heart itself, are suffering from oxygen hunger; it is only these cases that can be benefited by oxygen inhalations.

It is a remarkable thing that the continued action of oxygen at a pressure of 1 atm. and over acts as a poison (Bert, Lorrain Smith, L. Hill and Macleod). It produces inflammation of the lungs, depresses the respiratory exchange, and lowers the body temperature; at pressure of 2–3 atm. it quickly produces general epileptiform convulsions, and these are followed by a gasping type of breathing, coma, and death. All terrestrial animals, as far as I have tried them, are instantly thrown into convulsions and killed by exposure to 50–60 atm. of oxygen.

On the other hand, the excised hearts, muscles, and nerves of frogs, I find, survive the exposure to such a pressure for an hour, although with somewhat abated vigour. The swim-bladder of the fish must be immune to oxygen poisoning, for in a fish, caught at a depth of 4500 ft., whose bladder contained almost pure oxygen, the tension at which this gas was secreted was nearly 135 atm.!

The lungs and the nerve cells seem to be the especial points of attack by high pressures of oxygen. It requires, I find, an exposure for about 24 hours at 8 atm. of air (the oxygen pressure = 167 per cent. of an atm.) to produce marked pulmonary congestion; no such result follows if the oxygen in the air be halved by the addition of N_2. Owing to the danger of oxygen poisoning it will not be advisable for men to work for long periods at high pressures. Oxygen at a tension of 180 per cent. of an atm. kills mice in 24 hours, while

at 300 per cent. of an atm. it produces pneumonia in about 5 hours (Lorrain Smith, Hill, and Macleod). Bert showed that inhalation of oxygen will hasten the clearance of dissolved nitrogen. V. Schrötter has recommended deep-sea divers to carry a small cylinder of oxygen and to breathe it for a few minutes before ascending. Now I have found that a cat becomes convulsed in oxygen at 50 lb. in as short a time as 6 minutes. As the size of the animal seems to make no difference to the time of onset, it does not seem safe to employ the suggestion of v. Schrötter. The idea of a diver convulsed at the bottom of the sea is one too horrible to contemplate. Any diving apparatus, such as the Fleuss, which is fitted with an oxygen cylinder in place of air pump and pipe, is obviously not safe to use for more than a few minutes at any pressures above 1 atm.

[L. H.]

WEEKLY EVENING MEETING,

Friday, June 1, 1906.

LUDWIG MOND, Esq., Ph.D. D.Sc. F.R.S., Vice-President, in the Chair.

PROFESSOR H. MOISSAN, D.C.L. D.Sc. For.Mem.R.S. Membre de l'Institut, *Hon.Mem.R.I.*, Professeur de Chimie Minérale à l'Université de Paris.

L'Ébullition des Corps Simples.

JE vous entretiendrai ce soir de la distillation des corps simples, et je suis heureux de traiter cette question dans l'amphithéâtre de la Royal Institution. C'est qu'en effet, nous sommes ici au berceau même de la question. Nous en trouvons le point de départ dans le premier volume du journal de la Royal Institution publié en 1802.*

Il y a un peu plus d'un siècle que Sir Humphry Davy, en approchant l'un de l'autre les charbons qui servaient de conducteurs à un courant intense de la pile à un liquide obtint, le premier, l'arc électrique qui est devenu aujourd'hui d'un usage courant. Au moyen de cet arc et de la température élevée qu'il développe, il fit un certain nombre d'expériences qu'il a rapidement mentionées sur la fusion du platine, du quartz, du saphir, de la chaux et sur la volatilisation de l'or.

Mais à cette époque qui n'est pourtant pas bien éloignée de nous, ces expériences étaient difficiles à réaliser. Il a fallu attendre le perfectionnement de la machine dynamo-électrique pour que le savant et l'industriel puissent enfin manier avec facilité et d'une façon continue des courants intenses et des arcs puissants.

Il va de soi que, pour obtenir une notable élévation de température au moyen de l'arc électrique, il était utile de l'enfermer dans un four. C'est ce qui a été fait, dans le laboratoire, par Siemens et Huntington † et, dans l'industrie, par les frères Cowles. De même, Sir James Dewar,‡ pour étudier les spectres de certains métaux, avait enfermé l'arc électrique dans un four en carbonate de chaux. Après tous ces auteurs, ainsi que Deville et Debray l'avaient réalisé pour le chalumeau oxhydrique, nous avons disposé un arc électrique que nous avons cherché à rendre aussi puissant que possible dans une petite cavité de

* *Humphry Davy.* An Account of the Experiments on Galvanic Electricity. Roy. Inst. Journ., t. 1802, p. 165.

† *Siemens et Huntington.* Chemical News, t. 46, 1882, p. 163, et Ann. Ch. Ph. (5e Série), t. 30, 1883, p. 465.

‡ *Sir James Dewar.* Proc. Roy. Inst., vol. 9, p. 212.

chaux vive, où se trouvait la substance sur laquelle on voulait faire agir une température élevée.

Notre modèle de four électrique n'a donc pour lui que son extrême simplicité. Il permet de répéter facilement les expériences et de suivre une réaction à une température de plus en plus élevée. Enfin, il sépare complètement l'action calorifique de l'action électrique.

Notre four électrique se compose de deux blocs superposés de carbonate de chaux. Au milieu du bloc inférieur, se trouve une cavité dans laquelle le creuset sera disposé. Deux rainures permettent le passage des électrodes dont le diamètre variera avec l'intensité du courant. Le couvercle formera, au-dessus de l'arc, une cavité ellipsoïdale pour réfléchir la chaleur sur le creuset. En réalité, nous avons un arc intense enfermé dans une petite cavité au-dessus d'un creuset de charbon. L'arc est donc complètement séparé de la matière sur laquelle il doit réagir. Deux glissières rendent très facile le maniement des électrodes et permettent, par la diminution ou par l'accroissement de l'arc, de règler l'intensité du courant qui est mesurée par un ampèremètre placé sur le circuit.

Aussitôt que l'arc est établi à l'intérieur du four, des gaz se dégagent en abondance. Leur analyse permet de reconnaître qu'ils sont formés, en grande partie, d'hydrogène et d'oxyde de carbone. Ce fait est important, parce que ces gaz forment un milieu réducteur, dans lequel un grand nombre d'expériences nouvelles seront possibles.

Lorsque l'on utilisait en effet la combustion de l'hydrogène au moyen de l'oxygène avec le chalumeau oxhydrique, on produisait une atmosphère de vapeur d'eau et l'on formait ainsi un milieu oxydant qui ne permettait pas d'obtenir des métaux plus ou moins oxydables.

Cette atmosphère réductrice qui jaillit sans cesse du four empêche aussi l'oxygène et l'azote de l'air d'être en contact avec les vapeurs métalliques et de donner des réactions secondaires. Malgré la belle découverte de Lord Rayleigh et de Sir William Ramsay, nous n'avons pas encore assez d'argon pour opérer dans ce gaz véritablement inerte.

Nous avons fait, au moyen de l'appareil que nous venons de décrire en quelques mots, un très grand nombre d'expériences dont quelques unes ont eu une application industrielle. Nous citerons les préparations du carbure de calcium, du ferro-chrome et d'autres alliages difficilement fusibles.

Nos expériences ont été répétées par de nombreux savants aussi bien dans les laboratoires que dans l'industrie ; mais les températures atteintes n'ont pas encore été mesurées.

Nous pouvons déterminer les températures élevées d'une façon suffisamment exacte jusqu'au point de fusion du platine vers 1775° C. Mais au delà, les mesures font défaut. Tout ce que nous savons, et nous reviendrons tout à l'heure sur ce point, c'est que la température du charbon dans le cratère de l'arc électrique est voisine, d'après M. Violle, de 3500° C. Cependant nos expériences sont comparables entre elles. Si nous employons en effet le même courant, des électrodes de même diamètre et la même cavité à chauffer, nous pouvons,

avec une intensité électrique déterminée, reproduire des conditions expérimentales identiques après un temps donné. C'est ce qui nous a permis de comparer l'ébullition d'un certain nombre de corps simples ou composés.

Nous avons démontré tout d'abord que les composés, stables à 1800° C., sont ou volatilisés ou dissociés au four électrique. Comme tous les corps composés sont dissociables lorsque la température est suffisamment élevée, il en résulte que la question de savoir s'il existe des corps véritablement réfractaires était ramenée à celle de la fusion et de l'ébullition de nos différents éléments. Nous devions donc en faire l'étude séparée.

Distillation du Cuivre.

La volatilisation de petites quantités de cuivre par la chaleur de l'arc électrique à la pression atmosphérique a déjà été obtenue par Despretz en 1859 * puis par Siemens et Huntington † en 1882. En 1905, MM. Krafft et Bergfeld ‡ ont démontré que le cuivre entrait en ébullition dans le tube de Crookes à 1600° C. Enfin M. Féry § en utilisant notre four électrique a indiqué, au moyen de sa lunette pyrométrique, que le point d'ébullition du cuivre était voisin de 2100° C., tandis que MM. Krafft et Bergfeld, d'après les points d'évaporation et d'ébullition du métal dans le vide, donnent le chiffre de 2140° C.

L'expérience de la distillation de cuivre, à la pression atmosphérique, a été réalisée de la façon suivante :

Nous avons placé dans le creuset de notre four électrique 300 gr. de cuivre pur coupés en fragments cylindriques de 2 c³ environ de volume. Un tube de cuivre traversé par un rapide courant d'eau froide, ainsi que Deville l'avait fait dans ses expériences sur la dissociation, passait au milieu du four au-dessus du creuset et de l'arc. Il permettait de condenser rapidement une partie des vapeurs. Enfin, lorsque nous voulions recueillir une plus grande quantité de ces vapeurs, on perçait le couvercle du four et l'on disposait au-dessus de cette ouverture, une cloche cylindrique en verre mince (Fig. 1).

Nous avons cherché à rendre le courant aussi constant que possible, dans nos expériences, en écartant plus ou moins les électrodes, au fur et à mesure que l'atmosphère intérieure du four devenait plus conductrice par suite de l'abondante formation des vapeurs métalliques. Les électrodes cylindriques étaient terminées par des cônes de façon à donner de la fixité à l'arc et nous utilisions le courant alternatif. Dans ces conditions, les expériences d'une même série sont comparables entre elles.

* *Despretz.* Expériences sur quelques métaux et sur quelques gaz. Comptes Rendus, t. 48, 1859, p. 352.

† *Siemens et Huntington.* On the Electric Furnace. Chemical News, t. 46, 1882, p. 163.

‡ *Krafft et Bergfeld.* Berichte Deut. Chem. Gesell., t. xxxviii. 1905, p. 254.

§ *Féry.* Détermination des points d'ébullition du cuivre et du zinc, Ann. de Ch. et de Ph. (8ᵉ série), t. xxviii. 1903, p. 428.

Dans la première expérience, nous avons chauffé 300 gr. de cuivre pendant 5 minutes avec un courant de 300 ampères sous 110 volts. On voit nettement le métal fondre, puis après 3 minutes, entrer en ébullition. Le tube froid se recouvre de métal et la cloche d'un mélange de globules métalliques et d'oxyde. Le culot métallique, après l'expérience, ne pesait plus que 250 gr. Après 5 minutes de chauffe, nous avons distillé 50 gr. de cuivre.

Une deuxième expérience faite avec le même poids de cuivre et la

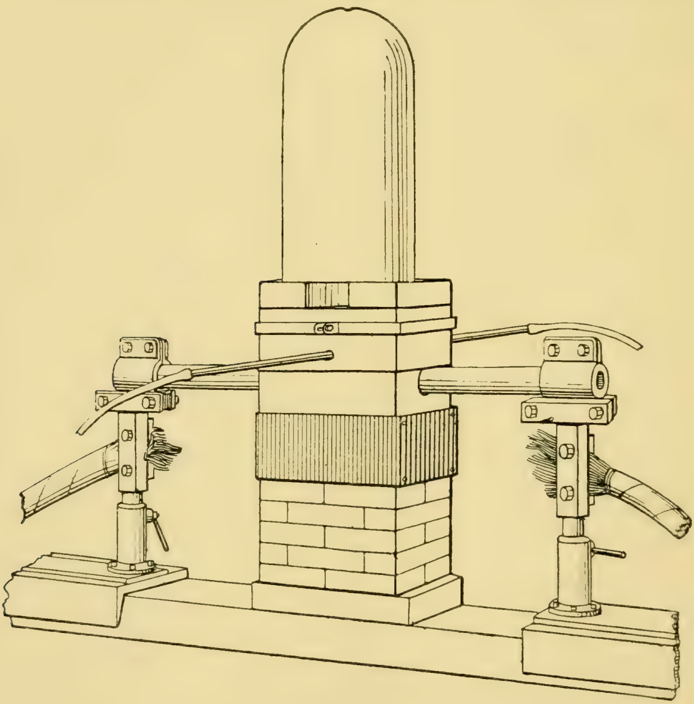


Fig. 1.

même intensité de courant, ne nous a donné, après 6 minutes, qu'un résidu de 140 gr. ; ce qui nous indique une volatilisation de 160 gr. de métal.

Enfin une troisième expérience, d'une durée de 8 minutes, nous a fourni, en partant du même poids de cuivre et d'une même densité de courant, un résidu de 67 gr., c'est à dire une volatilisation de 233 gr. de métal.

Si l'on examine le dépôt condensé sur le tube froid, on remarque que, en particulier dans la dernière expérience, il est formé d'un feutrage de filaments de cuivre de 5 mm. à 7 mm. d'épaisseur.

L'aspect de ces derniers rappelle celui de l'argent filiforme. Ce feutrage présente, à la loupe, des ramifications légères, brillantes et irisées, qui donnent à la masse, l'aspect du velours. Certains de ces filaments ont l'aspect de feuilles de fougère dont les frondes seraient arrondies. Leur couleur varie du rouge au jaune et présente de beaux phénomènes d'irisation.

La densité de ce cuivre distillé lorsqu'on l'a traité par l'acide acétique pour enlever une petite quantité de chaux qui le souille est de 8,16. Cette densité est plus faible que celle du cuivre fondu, ce qui tient à l'occlusion d'une certaine quantité de gaz. D'après Kalbaum, Roth et Siedler la densité du cuivre distillé est de 8,932.* Notre échantillon donne à l'analyse : cuivre 99,76 et ne renferme comme impureté qu'une petite quantité de chaux et de graphite. La surface de cette masse poreuse s'altère plus rapidement à l'air humide que la surface polie du cuivre fondu. Mais ses propriétés chimiques, vis à vis du chlore, de l'acide chlorhydrique, de l'hydrogène sulfuré ou de l'acide sulfurique sont identiques à celles de la limaille de cuivre. Dès que ce métal est traité par l'acide azotique étendu, les irisations superficielles disparaissent aussitôt et la couleur rouge du cuivre apparaît.

La matière pulvérulente, qui s'est déposée en abondance sur la cloche de verre, est formée surtout d'oxyde de cuivre, de chaux vive et de sphérules noires. Par conséquent, la vapeur de cuivre au contact de l'air a brûlé rapidement en fournissant de petits globules d'oxyde noir dont quelques uns renferment encore au centre une petite sphère de métal rouge.

Enfin, entre la surface du four et celle du couvercle, ainsi que sur les électrodes, on rencontre un grand nombre de petites gouttelettes de cuivre métallique, d'un rouge plus ou moins foncé. On trouve parfois quelques petites aiguilles déliées de cuivre ou de petits cristaux sans forme bien nette.

Le métal qui restait dans le creuset a fixé une très petite quantité de fer, de chaux et d'alumine, provenant des impuretés des électrodes. Mais, ce qui est beaucoup plus important, ce métal contient du graphite.

Lorsque le courant vient d'être arrêté et que le cuivre est en pleine ébullition, si l'on sort le creuset du four et qu'on le laisse refroidir lentement, on voit bientôt apparaître une croûte qui flotte sur le cuivre en fusion. Dès que la solidification commence sur le pourtour, on voit nettement de petits cristaux de graphite sortir du métal, puis la masse en fusion se boursoufle et il se produit un abondant dégagement gazeux. Différents observateurs ont déjà appelé l'attention sur cette solubilité des gaz dans le cuivre liquide.†

* *Kalbaum, Roth und Siedler.* Uber Metalldestillation und über destillierte Metalle. Zeitschrift für anorganische Chemie, t. 29, 1902, p. 177.

† *Caron.* De l'absorption de l'hydrogène et de l'oxyde de carbone par le cuivre en fusion. Comptes Rendus, t. 63, 1866, p. 1129. *Hampe.* Chem. Central Bl., t. 5. p. 104.

Lorsque le culot métallique est complètement refroidi, si l'on a évité un accès trop rapide de l'air, on voit qu'il est recouvert d'une couche onctueuse de graphite comme ces fontes manganésées produites par un haut fourneau marchant en allure trop chaude.

Ce graphite, soit amorphe, soit cristallisé, séparé par un traitement à l'acide azotique étendu, puis lavé et séché, a une densité de 2,12. Sa température d'inflammation dans l'oxygène est de 680° C. Il renferme comme impuretés du silicium, du fer et du magnésium qui proviennent du creuset et des électrodes : carbone 96,25 ; cendres, 3,36 ; hydrogène, 0,21. Les cristaux de graphite et les impuretés se sont concentrés entre les joints des cellules du métal, ainsi que M. Osmond l'a déjà observé dans des circonstances analogues.

En sciant ce culot de cuivre, puis en polissant sa section et en l'examinant au microscope, on reconnaît qu'il renferme un grand nombre de petites cavités sphériques. Chacune de ces géodes est tapissée de cristaux noirs brillants qui, séparés du métal par l'acide azotique, fournissent de l'oxyde graphitique en les traitant par le mélange d'acide azotique fumant et de chlorate de potassium. A sa température d'ébullition, le cuivre dissout donc le carbone et l'abandonne par le refroidissement sous forme de graphite.

Si l'on refroidit brusquement le culot de cuivre, encore à sa température d'ébullition, dans de l'eau froide, le métal au moment de sa solidification, roche avec vivacité et l'intérieur du culot présente alors de grandes cavités remplies de graphite. La teneur en graphite était au milieu du culot métallique de 1,62 pour 100 et sur le bord de 1,58.

Nous ajouterons qu'en augmentant l'intensité du courant jusqu'à 800 amp. sous 110 volts et en prenant un creuset qui puisse contenir 8 à 10 kg. de cuivre, il est facile d'en distiller plusieurs kilogrammes en quelques minutes.

Distillation de l'Or.

Pendant longtemps, l'or a été regardé comme un métal difficilement volatil que l'on ne pouvait amener à l'état de vapeur que sous l'action de l'étincelle d'une forte batterie électrique. Cependant Hare, en 1802, avait volatilisé une petite quantité d'or au moyen du chalumeau à oxygène et à hydrogène.

En 1893, nous avons démontré que l'or entrait en ébullition avec rapidité au four électrique et qu'il était facile de distiller 40 gr. d'or dans l'espace de quelques minutes.* Depuis nos premières expériences, Schuller † puis Krafft et Bergfeld ‡ ont établi que, dans le vide,

* *Henri Moissan.* Étude de quelques phénomènes nouveaux de fusion et de volatilisation produits au moyen de la chaleur de l'arc électrique. Comptes Rendus, t. 116, 1893, p. 1429.

† *Schuller.* Distillationem in luftleeren Quarzgefässen. Z. anorg. Chem., t. 37, 1903, p. 69.

‡ *Krafft et Bergfeld.* Uber tiefste Verdampfungstemperaturen von Metallen in Vacuum des Kathodenlichts. Ber. Deut. Chem. Gesell., t. 38, 1905, p. 254

l'or, renfermé dans un tube de quartz fondu, commence à se volatiliser vers 1070° C.

Nous avons chauffé, dans un creuset, 150 gr. d'or pur pendant 5 minutes 30 secondes, avec un courant de 500 ampères sous 110 volts et nous avons distillé ainsi 10 gr. de métal.

Dans une deuxième expérience faite avec le même poids de métal et la même intensité de courant, mais dans laquelle la durée de l'expérience était de 6 minutes 30 secondes, nous avons distillé 20 gr. de métal.

L'or, qui restait dans le creuset après refroidissement, ne renfermait pas de calcium ; il titrait 99,98 d'or. Sa surface extérieure présentait quelques cavités provenant des bulles gazeuses qui se dégagent au moment de la solidification. Cette surface métallique était recouverte, sur certaines de ses parties, d'une voile noir formé de cristaux enchevêtrés. Les géodes présentaient une cristallisation confuse de l'or sous forme d'arborescences se coupant à angles droits. Tout autour du creuset, se trouvaient de petites gouttelettes métalliques jaunes provenant de la condensation des vapeurs du métal. La chaux fondue, qui était voisine du creuset, était colorée en jaune très pâle, mais ne renfermait que des traces d'or ; il en est de même des cristaux de chaux qui se forment à une certaine distance. L'or distille avant le point d'ébullition de la chaux. C'est ainsi qu'un fragment de chaux fondue, voisin du creuset, est presque blanc, à peine teinté de jaune, tandis que la chaux frittée, qui se trouve près des électrodes et sur laquelle de très petits globules d'or se sont déposés est plus colorée.

Sur le couvercle du four, ainsi que sur les électrodes, on rencontre une grande quantité de gouttelettes d'or. Lorsque ces gouttelettes métalliques sont un peu éloignées du creuset et se trouvent sur la chaux du four, elles sont entourées d'une auréole rouge qui se dégrade en une belle teinte d'un pourpre foncé.

Le tube de cuivre, traversé par un courant d'eau froide, qui est disposé au-dessus du creuset, est recouvert d'un feutrage jaune foncé à reflets pourpres. Examiné à la loupe, il est formé de légères ramifications jaunes et brillantes rappelant l'aspect du cuivre que nous avons décrit précédemment.

Parfois on recueille de l'or filiforme, variété qui a déjà été obtenue par Margottet,* par M. Liversidge † et que M. Ditte ‡ a préparée au-dessous du point de fusion de l'or, en chauffant une lame de ce métal avec un mélange de chlorure et de pyrosulfate de sodium. La hauteur de ces filaments varie avec l'épaisseur de la couche d'or condensée sur le tube froid. On rencontre aussi, dans les parties les plus

* *Margottet.* Recherches sur les sulfures, séléniures et les tellurures métalliques. Ann. Ecole normale, 2e série, t. 8, 1879, p. 247.

† *Liversidge.* On the origin of mass gold. Journal Roy. Soc. of N. S. Wales, vol. 27, 1893, p. 287.

‡ *Ditte.* Sur la cristallisation de l'or. Comptes Rendus, t. 131, 1900, p. 143.

voisines de ce tube froid, de très petits cristaux jaunes brillants et d'apparence cubique. Cet or est accompagné d'une petite quantité de chaux distillée et de graphite.

Ce mélange, débarrassé de la chaux par un traitement à l'acide acétique étendu, renferme de l'or tellement divisé que cette poussière reste en suspension dans l'eau, en lui donnant, par transparence, une coloration verte.

Enfin, en recueillant la vapeur d'or condensée sur une cloche en verre mince, nous avons obtenu un dépôt d'une belle couleur pourpre formé d'un mélange de chaux et d'or distillé.

Ces expériences de la distillation de l'or ont été répétées dans un tube de charbon en plaçant le métal dans une nacelle de graphite. De même que dans l'expérience précédente, on voit nettement le métal, fondre sous l'action de l'arc électrique, puis après une minute 30 secondes entrer en ébullition. La vapeur, qui s'élève de la nacelle rencontre la partie supérieure du tube qui est fortement chauffée, elle reste complètement transparente, puis vient se condenser dans les parties froides sous forme d'une véritable pluie de globules d'or, d'une excessive finesse. Dans une de ces expériences, en chauffant 4 minutes avec un courant de 500 ampères sous 110 volts, nous avons distillé 17 gr. de métal. Dans la partie condensée, au milieu d'un grand nombre de sphérules métalliques, on rencontre quelques petits cristaux d'or. Le lingot, examiné avec soin après l'expérience, était encore recouvert de ce voile noir de graphite dont nous avons parlé précédemment.

Nous pouvons conclure de nos expériences que l'or est plus difficilement volatil que le cuivre. En chauffant, en effet, dans les mêmes conditions, ces deux métaux, on voit se produire l'ébullition dans un temps beaucoup plus court pour le cuivre que pour l'or. De plus, à la température de fusion de la chaux, l'or est déjà volatil.

Nous rappellerons que d'après MM. Krafft et Bergfeld * la différence entre le commencement de la vaporisation et le point d'ébullition dans le vide est la même que celle qui existe entre le point d'ébullition dans le vide et la température d'ébullition, à la pression atmosphérique. Dans ces conditions, l'or, commençant à donner des vapeurs dans le vide à 1070° C. et bouillant dans le vide à 1800° C. aurait 2530° C. comme point d'ébullition à 760 mm.

Les propriétés chimiques de l'or distillé sont les mêmes que celles de l'or martelé ou du métal fondu, réduit en poudre fine. Son attaque, soit par l'eau régale, soit par l'eau de chlore, dépend de la ténuité de l'échantillon mis en expérience. Il en est de même de l'attaque par le fluor ou par un mélange d'acide sulfurique chaud et de permanganate de potassium. De telle sorte que l'or pulvérulent, produit par condensation brusque de sa vapeur, ne nous a fourni aucune réaction capable d'indiquer l'existence d'une variété allotropique de ce métal.

* *Krafft et Bergfeld. Vide supra.*

Distillation des Alliages d'Or et de Cuivre.

Les alliages d'or et de cuivre, étudiés jusqu'à la température de 1064° C., forment des solutions homogènes en toutes proportions. La courbe de solubilité de ces alliages a été donnée par Roberts-Austen. Nous avons étudié l'alliage de cuivre et d'or à 10 et à 50 pour cent de ce dernier métal. D'après un grand nombre d'expériences concordantes, nous sommes arrivés à cette conclusion, qu'à haute température, le cuivre est notablement plus volatil que l'or.

Alliage d'Or et d'Étain.—Nous avons déjà fait remarquer que l'étain, qui a un point de fusion très bas, présentait un point d'ébullition très élevé. La distillation des différents alliages d'or et d'étain nous a démontré que l'étain distillait toujours plus rapidement que l'or.

Pourpre de Cassius.—Il est assez curieux de remarquer que la poudre condensée, soit sur le tube, soit sur la cloche, possède la même couleur pourpre que les auréoles qui entouraient les petits globules d'or condensés sur la chaux du four électrique à une certaine distance du creuset. Cette auréole pourpre était de l'oxyde de calcium anhydre coloré par de l'or. C'est un nouveau pourpre préparé par volatilisation de l'or et par fixation de sa vapeur sur la chaux.

Mais revenons à notre distillation de l'alliage or et étain. Lorsque le mélange de vapeurs d'or et d'étain sort à l'état gazeux du couvercle du four électrique, l'étain brûle au contact de l'air et donne de l'oxyde d'étain intimement mélangé à la vapeur d'or sur laquelle l'air atmosphérique n'a pas d'action. La substance recueillie, sur la cloche, possède la composition suivante—

$$SnO_2 : 49{,}15, \quad CaO : 36{,}93, \quad Au\ 9{,}90.$$

Cette composition, variable pour chaque expérience, dépend de la quantité de chaux et de métal volatilisés dans notre four électrique. Mais cette substance possède les propriétés du pourpre de Cassius et, débarrassée de la chaux par un traitement à l'acide chlorhydrique étendu, elle donne, sur les couvertes de la porcelaine, la coloration bien connue.

Cette nouvelle méthode de préparation nous a permis, en volatilisant de l'or en présence de différents oxydes, d'obtenir des pourpres variés.

L'alumine fondue avec l'or, au four électrique, se colore en rose pâle, et par distillation du mélange, puis par condensation des vapeurs, on obtient un pourpre plus ou moins foncé. L'or en excès, restant au milieu de l'alumine fondue et qui a filtré au travers de cet oxyde, présente des cristaux très nets, en octaèdres réguliers.

De même la magnésie fondue est colorée par l'or en violet, et la condensation du mélange des vapeurs d'oxyde et de métal donne un pourpre foncé d'une teinte orangée.

La zircone fondue se colore aussi en rose ou en violet, au contact

de la vapeur d'or et, distillée au four électrique, fournit d'abord de la zircone blanche et bientôt un pourpre lilas.

La silice fond avant le point d'ébullition de l'or, de sorte que l'or liquide, à cause de sa densité, tombe dans la silice fondue. Aussitôt que le métal entre en ébullition, un boursouflement de toute la masse pâteuse se produit. Certains fragments intérieurs de silice ont pris une teinte violette, tandis que la partie supérieure fondue est colorée en jaune pâle et contient de petits globules d'or. La vapeur condensée sur la cloche de verre, fournit un pourpre très fin et de belle couleur.

Ces expériences confirment les idées de Debray, sur la constitution du pourpre de Cassius.* Ce savant avait indiqué que ce pourpre ne formait pas une combinaison définie, mais n'était qu'une laque d'étain colorée par de l'or en poudre très fine.

Solubilité du Carbone dans l'Or maintenu à son Point d'Ébullition. A sa température d'ébullition, l'or dissout une petite quantité de carbone qui, par refroidissement, vient nager à la surface du métal sous forme de graphite. Lorsque l'or saturé de carbone est refroidi brusquement dans l'eau, le métal contient des géodes tapissées de cristaux de graphite.

Distillation des Métaux de la famille du Platine.

Nous rappellerons que la méthode industrielle employée aujourd'hui pour la séparation des différents métaux de la famille du platine est, à peu de choses près, celle qui a été indiquée par Wollaston.†

Mais la fusion du platine dans un four en chaux au moyen d'un chalumeau alimenté par le gaz d'éclairage et l'oxygène, fusion indiquée par Deville et Debray, a rendu très facile le travail du platine et de ses alliages. On sait que le palladium, le platine, le rhodium et l'iridium ont été fondus avec facilité par Deville et Debray au chalumeau oxhydrique. Plus tard, au moyen de l'arc électrique, Joly et Vèzes ont fondu l'osmium, puis Joly a fondu le ruthénium.

Nous avons réuni dans un tableau, les séries d'expériences poursuivies sur l'ébullition des métaux du platine.

	Poids.	Temps.	Ampères.	Volts.	Métal distillé.
	gr.	m.			gr.
Osmium . . .	100	4	500	110	0
Osmium . . .	150	5	700	110	29
Ruthénium . .	150	5	500	110	10
Platine . . .	150	5	500	110	12
Palladium . .	150	5	500	110	9·60
Iridium . . .	150	5	500	110	9
Rhodium . . .	150	5	500	110	10·20

* *Debray.* Note sur le pourpre de Cassius. Comptes Rendus, t. 75, 1872, p. 1025.

† *Wollaston.* Transactions philosophiques, 1819, et Annales de Chimie et de Physique (2), t. 45, 1829, p. 143.

En opérant sur un poids de 150 gr. de métal pur, et pendant une durée de temps de 5 m. avec des courants dont l'intensité a été variable, nous avons reconnu que la fusion de tous ces métaux se produisait après une minute ou deux, et que leur ébullition était nettement établie après quatre minutes. Lorsqu'ils sont liquides, tous dissolvent du carbone dont ils abandonnent la presque totalité par refroidissement sous forme de graphite.

Le palladium, qui est plus fusible que le platine, n'est pas plus volatil que lui.

Enfin, de tous ces métaux, le plus difficile à distiller est l'osmium.

Cette distillation des métaux de la famille du platine ne nous a été possible que grâce à la bienveillance de M. G. Matthey de Londres, qui a bien voulu mettre à notre disposition des échantillons coûteux de ces métaux rares.

Distillation des Métaux de la famille du Fer.

Sauf pour le manganèse, dont la volatilisation a été démontrée dans les hauts fourneaux par Jordan, nous ne possédons aucun renseignement sur la distillation de ces corps simples. Nous avons réuni dans le tableau suivant les résultats de nos expériences :—

	Poids.	Temps	Ampères.	Volts.	Métal distillé.
	gr.	m.			gr.
Nickel	150	5	500	110	56
	200	9	500	110	200
Fer	150	5	500	110	14
	825	10	1000	55	150
	800	20	1000	110	400
Manganèse	150	3	500	110	38
	150	5	500	110	80
Chrome	150	5	500	110	58
Molybdène	150	10	700	110	0
	150	20	700	110	56
Tungstène	150	20	800	110	25
Uranium	150	5	500	110	0
	150	5	700	110	15
	200	9	900	110	200

Les métaux de la famille du fer ont des points d'ébullition très différents. Le manganèse est le plus volatil de tous, et sa distillation se produit avec facilité avant celle de la chaux. Après lui, vient le nickel dont l'ébullition paraît assez tranquille ; puis le chrome qui distille avec rapidité sous l'action d'un courant de 500 ampères sous 110 volts. L'ébullition du fer est plus difficile à obtenir et elle est précédée d'un dégagement tumultueux des gaz que ce métal dissout avec tant de facilité. Cependant, en employant des courants plus intenses et après que cette première effervescence est calmée, l'ébulli-

tion du fer se produit avec facilité. En 20 minutes, avec un courant de 1000 ampères sous 110 volts, nous avons distillé 400 gr. de fer.

L'uranium a un point d'ébullition plus élevé, que celui du fer. La distillation ne se produit qu'avec des courants de 700 ampères sous 110 volts après cinq minutes de chauffe. Au contraire, le molybdène et le tungstène sont beaucoup plus difficiles à porter à l'ébullition, et nous n'avons pu arriver à une ébullition régulière de ce dernier métal qu'avec un courant de 700 ampères sous 110 volts dans une expérience d'une durée de 20 minutes.

La poussière cristalline, obtenue dans ces expériences par condensation de la vapeur métallique, possède les mêmes propriétés chimiques que le métal réduit en poudre très fine.

Distillation des Métalloïdes.

On sait depuis longtemps que le point de fusion du silicium est voisin de 1500° C., et nous avons démontré, qu'au four électrique, ce métalloïde peut être facilement amené à son point d'ébullition. Nous avons établi aussi que le carbone à la pression atmosphérique et à la haute température du four électrique passait de l'état solide à l'état gazeux sans prendre l'état liquide.

De nouvelles expériences méritaient d'être poursuivies sur le titane. Nous sommes partis d'une fonte de titane préparée au four électrique, au moyen de la réduction de l'acide titanique par le charbon. Cette fonte renfermait 3,5 de carbone. Nos expériences sont résumées dans le tableau suivant :—

	Poids.	Temps.	Ampères.	Volts.	Titane distillé.
	gr.	m.			gr.
Titane	150	5	500	110	9
	150	5	500	110	11
	150	6	500	110	17
	300	7	1000	55	110

Elles nous démontrent que le titane est difficilement volatil mais cependant de même que les métaux précédents, il peut être distillé avec régularité.

Conclusions.—De cet ensemble de recherches, nous pouvons tirer les conclusions suivantes :

Le cuivre peut être distillé avec facilité au four électrique ; lorsque sa vapeur est condensée sur un corps froid, on peut obtenir un feutrage de cuivre filiforme présentant toutes les propriétés du cuivre ordinaire. A sa température d'ébullition, le cuivre dissout le graphite et l'abandonne par refroidissement.

L'or distille rapidement au four électrique ; son point d'ébullition est supérieur à celui du cuivre et inférieur à celui de la chaux. Par

condensation, sur un tube froid, sa vapeur produit de l'or filiforme et de petits cristaux microscopiques. Les propriétés de cet or sont les mêmes que celles du métal en poudre fine.

Dans les alliages d'or et de cuivre, dans les alliages d'or et d'étain, le cuivre et l'étain distillent avant l'or. De plus, en distillant un alliage d'or et d'étain, on obtient par voie sèche, le pourpre de Cassius. Cette préparation est générale et permet d'obtenir des pourpres avec différents oxydes tels que la silice, la zircone, la magnésie, la chaux et l'alumine.

Tous les métaux de la famille du platine sont rapidement fondus, puis, portés à l'ébullition dans notre modèle de four électrique avec des courants qui varient de 500 à 700 ampères sous 110 volts. Si nous partons de 150 gr. de métal, la fusion s'opère en une ou deux minutes et l'ébullition régulière est atteinte avant quatre minutes. On recueille sur le tube de cuivre, traversé par un rapide courant d'eau froide, qui se trouve au-dessus du creuset, des sphérules métalliques, des lames cristallines et le plus souvent un feutrage de très petits cristaux visibles seulement au microscope. Tous ces métaux liquides dissolvent du carbone qu'ils abandonnent par le refroidissement sous forme de graphite. Le plus difficile à distiller de tous ces métaux est l'osmium. Le palladium qui est plus facilement fusible que le platine ne parait pas plus volatil que le platine ou le rhodium.

Les métaux de la famille du fer ont des points d'ébullition très différents. Le manganèse est le plus volatil de tous et sa distillation se fait avant celle de la chaux. Après lui vient le nickel dont l'ébullition parait assez tranquille ; puis le chrome qui distille avec régularité sous l'action d'un courant de 500 ampères sous 110 volts. L'ébullition du fer est plus difficile à obtenir et elle est précédée d'un dégagement tumultueux des gaz que ce métal dissout avec tant de facilité ; cependant, en employant des courants plus intenses et après que cette première effervescence est calmée, l'ébullition du fer se produit avec régularité. En 20 minutes avec un courant de 1000 ampères, sous 110 volts, nous avons distillé 400 gr. de fer.

L'uranium a un point d'ébullition plus élevé que celui du fer ; la distillation ne se produit qu'avec des courants de 700 ampères sous 110 volts après 5 minutes de chauffe. Au contraire, le molybdène et le tungstène sont beaucoup plus difficiles à porter à l'ébullition et nous n'avons pu arriver à une ébullition régulière de ce dernier métal qu'avec un courant de 700 ampères sous 110 volts dans une expérience d'une durée de 20 minutes.

La poussière cristalline obtenue dans toutes ces expériences, par condensation de la vapeur métallique, possède les mêmes propriétés chimiques que le métal réduit en poudre fine.

De cet ensemble de recherches, nous pouvons tirer la conclusion qu'il n'existe plus de corps réfractaire. Les composés qui subsistent à la température de l'arc électrique sont volatilisés. Parmi les métalloïdes le carbone et le bore à température très élevée passent, à la pression ordinaire, de l'état solide à l'état gazeux. Tous les métaux, par une

élévation de température suffisante, sont d'abord liquides, puis prennent l'état gazeux avec facilité.

Ainsi se trouve justifiée cette phrase écrite par Buffon * :

" Selon moi, les substances les plus simples et les plus réfractaires ne résisteraient pas à cette action du feu si l'on pouvait l'augmenter à un degré convenable."

Cet ensemble d'expériences sur la distillation des métalloïdes et des métaux nous conduit à une autre conclusion tout aussi importante.

On sait quelles difficultés présente la détermination de la température de la surface solaire et combien les astronomes et les physiciens sont peu d'accord sur ce sujet.

D'après Waterston, cette température serait, de 9 à 10,000,000 de degrés centigrades, d'après le Père Secchi de 1 à 2,000,000, d'après Ericsson de 2,000,000. Il est bien certain que ces températures parurent illogiques.

Les expériences de Pouillet, de Soret, de Desains, puis la discussion de Vicaire fixèrent cette température du Soleil de 1398° à 1700° C. Les déterminations de la constante solaire de M. Crova s'ajoutèrent à celle de Pouillet, puis les expériences, poursuivies par M. Violle, vinrent apporter sur ce point de nouvelles conclusions.

Par deux méthodes différentes, M. Violle fut amené à conclure que la température moyenne probable de la surface solaire était comprise entre 2000° et 3000° C.†

Plus récemment M. W. L. Wilson vient de publier des recherches sur ce sujet. En appliquant à ses déterminations le coefficient de transmission de Langley, lorsque le Soleil est au zénith et en le comparant à celui de Rosetti, la température de la surface solaire serait de 6085° absolus. En admettant aussi que la perte, due à l'absorption par l'atmosphère solaire fût d'un tiers, la température du Soleil serait de 6863° absolus.‡

Sans avoir la prétention de résoudre une question aussi difficile, nos expériences y apportent cependant une modeste contribution.

Quelle que soit la forme extérieure de la partie visible du Soleil, nous savons que cet astre est formé des mêmes corps simples que la terre ou plutôt que la plupart des corps simples qui se trouvent sur la surface terrestre se rencontrent aussi dans le Soleil. D'après les recherches spectroscopiques de Thalen, de Cornu, d'Hasselberg, le titane existe dans le Soleil de même que le fer, le manganèse et le tungstène. Il est bien vraisemblable que le Soleil, à cause même de la grande quantité de chaleur qu'il rayonne, ne peut être formé seulement de matières gazeuses et qu'il doit contenir un noyau solide ou liquide. Nous venons d'amener à l'état gazeux, au moyen de l'arc électrique tous les corps simples ou composés que l'on peut obtenir à

* *Buffon.* Minéraux, t. 3, p. 147.

† *Violle.* Comptes Rendus, t. 78, pp. 1425 et 1816 (1874) ; t. 79, p. 746 (1874) ; t. 80, pp. 662–727 et 896 (1876).

‡ *Wilson.* The effective temperature of the Sun. Proc. Roy. Soc., t. 69, p. 312 (1902).

la surface de la terre. Or la température maximum de l'arc électrique a été mesurée par M. Violle et reconnue voisine de 3500° C. A cette température tous les corps connus sont donc gazeux, et par suite, la température du Soleil ne devrait pas s'élever au-dessus de 3500° C. Mais nos expériences ayant été faites à la pression atmosphérique, il va de soi que des pressions plus grandes pourront modifier les phénomènes d'ébullition des différents corps simples ou composés. Seulement ces températures seront loin d'atteindre les chiffres beaucoup trop élevés indiqués autrefois et elles oscilleront vraisemblablement entre le chiffre de M. Wilson 6590° C. et ceux de M. Violle compris entre 2000° et 3000° C., en se rapprochant vraisemblablement de ces derniers.

[H. M.]

WEEKLY EVENING MEETING,

Friday, April 6, 1906.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. M.A. D.C.L. LL.D. Sc.D. Pres.R.S., Honorary Professor of Natural Philosophy, R.I., in the Chair.

WILLIAM BATE HARDY, Esq., M.A. F.R.S., Fellow of Gonville and Caius College, Cambridge.

*The Physical Basis of Life.**

IN a famous lay sermon on the Physical Basis of Life, written nine years after the publication of *The Origin of Species*, Huxley writes as follows:

"When hydrogen and oxygen are mixed in a certain proportion and an electric spark is passed through them, they disappear, and a quantity of water, equal in weight to the sum of their weights, appears in their place. There is not the slightest parity between the passive and active powers of the water and those of the oxygen and hydrogen which have given rise to it. At 32°, and far below that temperature, oxygen and hydrogen are elastic gaseous bodies whose particles tend to rush away from one another with great force. Water at the same temperature is a strong though brittle solid, whose particles tend to cohere into definite geometrical shapes. . . .

"Nevertheless, we call these and many other strange phenomena the properties of the water, and we do not hesitate to believe that in some way or another they result from the properties of the component elements of water. We do not assume that a something called 'aquosity' entered into and took possession of the oxide of hydrogen as soon as it was formed, and then guided the aqueous particles to their places on the facets of the crystal or amongst the leaflets of the hoar frost. On the contrary, we live in the hope and in the faith that by the advance of molecular physics we shall by-and-by be able to see our way as clearly from the constituents of water to the properties of water as we are now able to deduce the operations of a watch from the form of its parts and the manner in which they are put together. . . .

"If the properties of water may be properly said to result from the nature and disposition of its component molecules, I can find no

* Reprinted from 'Science Progress' by permission.

intelligible ground for refusing to say that the properties of protoplasm result from the nature and disposition of its molecules.

"But I bid you beware that, in accepting these conclusions, you are placing your feet on the first rung of a ladder which, in most people's estimation, is the reverse of Jacob's, and leads to the antipodes of heaven. It may seem a small thing to admit that the dull vital actions of a fungus or a foraminifer are the properties of their protoplasm, and are the direct results of the nature of the matter of which they are composed. But if, as I have endeavoured to prove to you, their protoplasm is essentially identical with, and most readily converted into, that of any animal, I can discover no logical halting-place between the admission that such is the case and the further concession that all vital action may, with equal propriety, be said to be the result of the molecular forces of the protoplasm which displays it. And if so, it must be true, in the same sense and to the same extent, that the thoughts to which I am now giving utterance, and your thoughts regarding them, are the expression of molecular changes in that manner of life which is the source of our other vital phenomena."

This uncompromising, virile attitude towards the most difficult and stupendous problem of science is characteristic both of the man and of the time. Huxley wrote in 1868 at the zenith of a period of strenuous intellectual life without doubt unsurpassed in the history of the world. The strong new wine of scientific discovery was running in men's veins.

A mere chronological table of the chief scientific events shows how fast was the growth. In the forties the labours of Joule provided a basis for the conception of the conservation of energy which at a step unified all the sciences. In the forties, too, the unification of the biological sciences was begun by the recognition of the cell as the unit of all life, and of the glutinous sarcode as its physical basis, and was crowned by the publication of *The Origin of Species* in 1859, which gave force and authority to the older doctrine of the continuous development and progression of life.

The spirit of the age was one of conflict, and men's minds were turned by it to Pisgah-like visions of the country to be conquered. The ideal of the new learning was the unity of all knowledge, its quest the establishment of a scheme of things animate and inanimate which should show them, linked together, without break, in orderly progress from the simple to the complex, from the lower to the higher, and its duty the warfare against a piecemeal and partial outlook of separate creation and catastrophic change. For the new learning no one did battle more strenuously than did Huxley.

The doctrine of the unity of knowledge and experience is not an easy one; it is justified even now rather by the steady trend of science than by its completed demonstrations. Knowledge may be seen to be growing from the sides of many a chasm like the two arms

of a cantilever, and we believe that human industry and the human intellect will one day complete a bridge across which all may pass in safety. But in the meantime there are grave signs in the scientific and semi-scientific literature of the day of a growing impatience with the rate of progress, which, on the one hand, insures ready and uncritical acceptance of the crude attempts of an amateur biologist to make living matter, and, on the other, breeds a feebler purpose which seeks an unhealthy opiate in "vitalism" or some other "ism" of like nature.

Nearly forty years of vigorous scientific work have elapsed since Huxley wrote, and it is still possible for the vitalist to assert that no single vital process can be completely expressed in terms of physics and chemistry, that is, of motion and of matter. The biologist is reproached, for instance, with the undoubted fact that the power which a living cell has of selecting certain chemical substances and of rejecting others cannot yet be explained by, and indeed in some ways seems to contradict, the known laws of molecular physics.

To this reproach I would reply after the fashion of Socrates, and with the same purpose, by a question.

Here are two pairs of gases, one of hydrogen and oxygen, the other of hydrogen and chlorine. I burn the members of each pair together, and from the one pair I get water, a fluid odourless, innocuous, and of relatively slight chemical activity, while from the other I get hydrochloric acid gas, acrid, poisonous, and of the highest chemical activity. Now, the molecules of those three gases have certain inalienable properties, an invariable weight, a fixed capacity for electricity. They perform movements the harmonic periods of which are so fixed that apparent departures from them have been used to detect and measure the velocity of approach of a star towards the earth. I ask the chemist or molecular physicist to explain the amazingly divergent properties of the compounds in terms of the properties of the component gases. I ask him to do what over-hasty people, forgetful of the extreme youth, the paucity in years of human knowledge, ask the biologist to do with respect to living matter, and the reply is that the question is unanswerable.

It cannot be sufficiently insisted upon that in many regions not the simplest more advance has been made towards a material explanation of vital phenomena than towards a solution of the simple question why one pair of gases should combine to form a fluid, while another pair combine to form a gas.

An unanswerable question concerning the elements of natural knowledge is a sharp reminder of our ignorance, and such a reminder is needed to curb the spiritual arrogance which in our time has brought this greatest of all mysteries, the relation of living to non-living matter, to the temples of vulgar credulity, and has prostituted it to the purposes of common charlatans and impostors.

Since Huxley wrote, our knowledge of the physical basis of life

has developed in many directions. The properties of secretion and absorption, of contractility and irritability, have been studied in great detail. The classical fields of physiology, the detailed investigation of form, and the anatomy of function have been continuously worked. But the greatest advance has come in the domain of chemical physiology. Ten years ago this was a scientific No Man's Land, despised by the pure chemist and traversed only in a distrustful, amateurish way by the physiologist. Now that is changed: on the one hand a race of physiologists has sprung up who are at the same time expert chemists; on the other one sees a pure chemist, Emil Fischer, of Berlin, bending all the resources of his great laboratory in men and materials to the central chemical problem of living matter, the chemical structure of proteid.

The few pages at my disposal would not hold even a *catalogue raisonnée* of the new departures. Therefore it is necessary to select a few problems, and for purposes of contrast I choose not the new but the old, which were agitated half a century ago.

At the outset, however, it is necessary to state certain elementary facts—that there is a unit of living matter called the cell, which everywhere and in all places has recognisably the same structure; and that all forms of life are divisible into two divisions: those in which the individual and the cell are coterminous—the simple-celled forms; and those more complex and larger types in which the individual is a cell complex—the multicellular forms. The former are probably the more numerous, but they escape notice by reason of their small size, which is imposed upon them by a law, well nigh without exception, which must strike very deeply into the nature of living matter—namely, that no single unit, no cell, that is, can increase to more than microscopical dimensions. When it reaches the limit of size it becomes unstable, a field of force of a peculiar and special nature is formed within it, and by this field of force the cell is presently rent in twain.

The basis of this curious limitation of size is not far to seek. Living matter is composed of very large molecules, and substances so built possess certain special properties which mark them off from simpler substances. To them the name of colloids is given, after the type of the class of jellies. Now, a jelly is a curious half-way house between the solid and the liquid states. Like a solid, it is capable of retaining differences of state: it is rarely of uniform character throughout. The rate of relaxation, as Clerk Maxwell called it, of jellies is slow, much slower than that of simple liquids, much faster than that of true solids. Combined with this characteristic inertia, however, is a degree of molecular mobility sufficient for chemical changes of great velocity. A jelly is in this way a meeting-place of extremes, and this it is which enables the colloidal state to manifest life.

Consider now a small free cell, an infusorian swimming in a way-

side pool. It displays many activities, it digests in this region of its living body, it maintains a store of starch in that region, in the movements of its parts there is diversity. Both its chemical and physical characters betoken a complexity which show it to be not a homogeneous droplet, but, in spite of its minute size (less than $\frac{1}{100}$ inch), to be heterogeneous. It has a structure, an architecture, the coarser features of which we can decipher with the aid of the microscope.

It is only in the colloidal state that we could have within so small a space so great a diversity of matter, and such differences of chemical potential as must exsist to support the multifarious activities of the living cell, combined with the molecular mobility necessary to give chemical change free play. At the same time this capacity for maintaining differences of state imposes limitations, one of which is that of size. Large molecules can move in the substance of the cell scarcely at all. Therefore, when the size exceeds a certain critical limit, the dynamical balance fails, and internal strains appear of a magnitude great enough to tear the cell apart. On this blending of opposites, on the curious combination of inertia and chemical mobility in the colloidal state, is reared the whole fabric of the dynamics of living matter.

Each living cell is a machine ; it breathes, taking in oxygen : it feeds, and the food is burnt by the oxygen to chemically simpler bodies. The living cell, like the gas engine, can tap the stores of chemical energy—and, like the gas engine, it is an internal combustion engine. Now in a power station where electricity is being produced to run a score of trams, there is a steady hum or drone, the varying pitch of which marks the speed of the engine. To the engineer in charge, from long habit, that varying sound speaks of events happening in remote parts of the system. A glance at the clock, and he will tell you that the sound is falling because the engine is adjusting itself to the increased load due to such and such a tram breasting such and such a hill. In the same way, watching the movement of a living cell under the microscope, if we were sufficiently skilled we could refer the continual change in the rate and direction of its movement to temporary inequalities of temperature, of lighting, or of chemical composition, etc., in the water in which it lives. If we resort to experiment, the effects are obvious : an electric shock causes the irregular amœba to come to rest as a sphere, a trace of acid slows its movements, of alkali accelerates them.

These things—the electric shock, the acid, or the alkali—are what the biologist calls "stimuli," and by varying their nature or intensity he can control the activity of living matter to a very remarkable extent.

Let us return for a moment to the amœba. We watch it crawling amid sand, fragments of decayed leaves, and living diatoms, and we notice that of the particles which it eats some are nutritious food,

some are innutritious and abselutely useless. But we also notice that there is a decided balance in favour of the nutritious particle. Like Autolycus, it is a picker-up of unconsidered trifles, guided by a decided preference for things useful to itself. Therefore, the tiny animal manifests discrimination or choice—imperfect, no doubt, but clearly recognisable. And the choice is beneficial; it contains an element of purpose.

Watch an amœba long enough, and it will be seen to divide into two, and these again into two, so forming successive generations the individuals of which resemble one another. These labile, creeping fragments of jelly have recognisable form, and zoologists classify the various forms in so many species, each of which breeds true. They manifest that property of living matter called heredity.

Lastly, the individual amœba is in incessant movement, and with each successive generation there is growth of individuals and increase in the mass of living matter. Now, to these three features, *choice and purpose*, *growth*, and *heredity*, I propose to confine myself, and I will consider them separately and in the order named.

The Faculty of Choice.

In one of Jules Verne's books, which at one time or another held most of us in thrall, there is an account of a submarine vessel which for a long time was conjectured to be some mighty marine monster. Now, I want you to put yourself in a similar position with respect to a model Whitehead torpedo, to consider yourself as meeting one of these for the first time and studying its movements under the impression that it is a living being. The torpedo must be without its charge, otherwise your experiments would come to an abrupt end; for I wish you to consider yourselves as inquiring why this curious beast should always swim at the same depth. Push it down, pull it to the surface, it would presently be swimming again as many inches below the surface as before, and you would say to yourselves, Why on earth does it *choose* to swim there?

Instead of a model torpedo, here in a drop of fluid are countless thousands of the most minute forms of life, each actively darting hither and thither, each so small that 5,000 would make only one large amœba. Into that drop you introduce two fine capillaries, the one filled with very dilute acid, the other with very dilute alkali, and in no very long time you will find that the vibrios have collected in a mass at one or other of the tubes—probably the acid tube. If you followed ordinary usage, you would express the result again in terms of choice by saying that the animals are attracted to the acid and are repelled from the alkali.

Both cases illustrate the difficulty in freeing the imagination from the tyranny of the counters it employs. The first case, that of the

torpedo, has served its purpose as an illustration, and it interests us no longer. Let us see whether a purely mechanical conception will explain the second.

The acid and alkali diffusing out of the tubes destroy the uniformity of the water, so that starting from one tube and moving to the other, one passes through gradually diminishing acidity, through neutrality, to a region of gradually increasing alkalinity, which reaches a maximum at the orifice of the other tube. The medium between the tubes, therefore, is accurately graded in composition.

Now let us see the effect of a trace of acid and alkali upon these vibrios. It is not always the same; it depends upon the particular forms we are examining. I choose the case where acid slows the movements, alkali increases them. Each individual vibrio, as we watch it, is seen to move in an erratic and irregular orbit, so erratic that we can consider it as completely irregular.

The problem becomes a simple mathematical one. Given a number of particles, each moving in an irregular orbit, and uniformly distributed throughout a homogeneous medium. The medium ceases to be homogeneous and is changed so that in one region the mean velocity of each of the particles is augmented, in another it is diminished. What will be the effect upon the distribution of the particles? The answer is that they will collect where the motion is slowest.

We now try the experiment, and we find that the vibrios do collect where their motion is most slowed—namely, in the region of maximal acidity. And they do not swim directly there; they, as it were, settle out in that region, as the hypothesis demands.

The influence of a chemically heterogeneous medium upon the free cells living in it is called "chemiotaxis." I have analysed a simple case, but it would take a session's lectures to follow out the application of the principle to biological problems. It explains great regions of disease, it has even been applied to the workings of the nervous system. At one sweep it embraces the directive effects of the surrounding medium upon the movements of free cells, in the waters of the earth and in the bodies of animals and of plants.

The choice of food particles, the discrimination manifested by amœba, is the chemiotactic response of its irregularly flowing protoplasm to the chemical atmosphere, if I may so put it, of the food particle. At the Mint a chance collection of sovereigns are presented to a certain machine, and it sorts them into those of full weight and those of short weight. A chance collection of particles are presented to amœba, and it sorts them very imperfectly into those which modify the incessant streaming of its protoplasm so that they become engulphed, and those which do not so modify the streaming. The element of choice, or, as we may now put it, the directive influence of the surroundings, is much less perfect here than in the case of the vibrios, because the oscillations—the movements of

the protoplasm on which it operates—are both less in extent and much more regular than are the movements of the vibrios.

In the choice of food particles, and in the sorting of the coins, there is an end to be served. Looked at from this point of view, chemiotaxis sometimes presents novel features. *Amœba proteus*, large and slow-moving, frequently captures an active ciliate called *Colpidium*. Observers describe the capture as being due to the *Colpidium* swimming as though attracted into the pseudopodial jaws, whence it makes no efforts to escape. Here the element of purpose, looked at from the standpoint of *Colpidium*, is that of a Christmas ox marching to the kitchen to be converted into beef-steaks.

The directive effect of the medium upon a free cell is usually more complex than in the case considered. *Opalina* is a large ciliate which in a uniform medium swims straight forward, owing to the movement of the cilia or vibratile hairs which cover its surface. The movement in this case starts at the front end of the animal and sweeps back as a wave like the wave over a cornfield. In a heterogeneous medium the movement starts excentrically, the waves sweep obliquely down the animal, and the direction of motion changes. The net result again is the same—the animal ceases to be distributed evenly when the water ceases to be of uniform composition.

The next example raises the question of choice to a higher level. It brings into the response of the animal its previous history. We will take the simplest case, as it is offered by *Opalina*. This animal is parasitic in the intestine of the frog, and it thrives in a very slightly acid medium. But its attraction to acid is not an inalienable quality. Glut it with acid, soak it in dilute acid for an hour, and it now collects in a region of alkali ; bathe it for an hour in very dilute alkali, and its chemiotactic response is once more changed—it collects about the acid.

The mechanism underlying this change of response must be patent to every chemist. There are many substances whose chemical and physical characters are completely reversed by change from a trace of acid to a trace of alkali, or *vice versa*. Amongst these substances, and markedly possessed of this character, are the chemical substances called proteids, of which all living matter is composed. The varied response to acid or alkali may unquestionably be traced in the first instance to the directive imfluence of the amphoteric proteid on the surface energy of the animal and upon the train of chemical events in its interior.

A parallel differential response is furnished by *Stentor*—a large trumpet-shaped animalcule—which fixes itself by its foot to some solid object. Touched on one side by a fine glass hair very lightly, it bends towards the hair ; touched more heavily, it bends *away*. Therefore there is a touch of a certain strength which produces no response. To a series of touches regularly repeated it gives the following responses. At first it simply bends away, then it contract

right down on to its foot; if that does not get rid of the irritant, it looses its hold and swims away.

These responses have been analysed with great care in order to elucidate the underlying mechanism. I can stop only to point out the element of purpose. In order to get rid of an irritation, a certain movement is tried; it fails, another movement is tried; it fails, and a third movement is tried.

Now, it must be clearly understood that organisation will account for these phenomena. Quite as remarkable a series of responses, each in turn designed to get rid of an irritant, can be obtained from a frog which has been deprived of its brain, and therefore presumably lacks both consciousness and intelligence. And step by step, as organisation advances, the response gains in complexity, until the human imagination is unable to unravel the chain of cause and effect. But the biologist is cognisant of no break in the series from the choice of a vibrio, which can be analysed algebraically, to the choice of a child between two toys.

The faculty, clearly seen in the case of *Stentor*, of storing impressions, so that the response to any particular stimulus is in part conditioned by the stimuli which have preceded it, is a familiar property of living matter, and also of matter in the colloidal state. The molecular state of a jelly is not fixed by the conditions of the moment. Just as a piece of wrought iron has properties different from those of cast iron, so the circumstances which attend the making of a jelly—temperature, concentration, and the like—confer on it an internal structure which controls its properties for years to come. Each jelly, therefore, has an individuality due to the record which it bears of its past.

Take another case. A vertical rod of wax is bent, first north, then south, then east, then west, and so on. Left to itself, it will quietly work out these movements in the reverse order. It bends first west, then east, then south, then north, and so on. The molecular structure of the wax is such as to preserve a record not only of the fact that it has been moved, but also of the number, direction, and order of the several movements.

The Faculty of Growth.

If it be true, as some chemists think, that in the process of oxidation there are always two processes more or less concurrent—the first one of synthesis, in which bodies of increased chemical complexity are formed by the union of the oxygen and the combustible substance; the second one of analysis, which supervenes only when the synthetic products reach a degree of complexity where they are unstable at the particular temperature and pressure—then, considered in a general way, the processes of assimilation and growth of living matter are exceptional only in the prominence and permanence of the synthetic

stage. The living cell, on this view, is like the flame in being an oxygen vortex; it is unlike it in the extraordinary latency or delay in the advent of the analytic processes.

The peculiar feature of the living cell, however, considered as a machine, lies in the fact that, of the total amount of energy which it acquires, a fraction is retained and devoted to the increase of its own substance. In other words, it grows. After a while it divides, and the daughter cells are like itself, so that there is not only the power of increasing the bulk of living matter by growth, but also a directive faculty called heredity, which constrains the new living matter, made from non-living matter, into the pattern of the old. The problems of growth and multiplication can be reduced to their simplest terms only in the case of minute forms like amœba, each of which is at once a single cell and an individual. Each individual amongst the higher forms of life is built of countless cells, all of which, with one or two exceptions, are predestined to death. The exceptions—the true immortals—are chosen from the germ cells. When, however, an amœba multiplies, it divides bodily into two, and by this simple process a new generation is formed. Clearly, as Weissmann first pointed out, in such cases death intrudes only in the guise of accident.

The conditions of life of these simplest forms are by no means simple. Make an infusion of hay in boiling water, and let it cool. In the course of a day or so it will be found to be swarming with rod-like bacteria (*Bacterium subtilis*), engaged in feeding on the organic matter dissolved out of the hay. A few days later numbers of an actively mobile slipper-shaped animal, called *Paramœcium caudatum*, make their appearance to actively swallow and digest the bacteria; and so the round goes on. The bacteria and the paramœcia alike have developed from wind-carried spores. Therefore in the natural life of these creatures are periods of physiological activity alternating with periods when life seems to be completely dormant—periods which follow one another according to no regular sequence, but in consequence of chance rainfall and of drought, when the inhabitants of the dried-up pool are caught up and carried away as dust.

Watch any chance collection of paramœcia, and individuals will be seen not only to divide, but occasionally to fuse. Two individuals swim together, adhere closely, and effect an extensive interchange of substance. This is the process of conjugation: it is the first beginning of sexual reproduction. It is followed by increased physiological activity, increased rate of growth and of multiplication. If we could follow this mating process fully, if our imagination could grasp the events which lead up to it and the effects which follow, we should see in it the response of life to the flux of cosmical energy, just as the oscillations of a particle in Brownian movement are the response to the flux of electrical potential. This is no careless phrase: it is sober truth, for the air currents carry and mix spores from far-distant places which have had, therefore, different life-histories. They have lived

in waters draining different kinds of soil, and therefore chemically different, the balance of sunshine and shade has been different, and the wind capriciously sows these spores from north, south, east, and west in the pot of hay tea. There they become active, and mate in conjugation, but not fortuitously. Guided by chemiotaxis, unlikes meet and fuse, just as do unlike cells when an ovum and a spermatozoon fuse, and the fusion of a pair of unlike individuals results in a thorough reorganisation, a fresh make-up, of the living matter of each. The continuance of the race depends upon the change of environment, upon the alternation of periods of activity with periods of dormancy, and upon the fusion of unlike individuals; and for these the sequence of natural phenomena, of summer and winter, of sunshine and shade, provides.

The problem of growth is this. Suppose we eliminate these factors; suppose we isolate a pure strain of paramœcia and keep them abundantly supplied with food—will the race continue to flourish and grow indefinitely, or will it attain old age and die off? The problem is far-reaching. It touches the simple questions of function, of digestion and assimilation, on the one side; while on the other it is concerned with the limitations of heredity in moulding successive generations after the common type. Three workers have attacked it with conspicuous success, Maupas,* Calkin,† and Woodworth,‡ and in each case the experimental method was the same.

Maupas was the first. He isolated individual paramœcia under normal and healthy conditions—namely, in hay infusion containing the bacteria on which they feed, which was changed daily. Each individual was the starting point in a sequence of generations, there being, on the average, two generations in three days. The rate of division was recorded, and the records furnished the basis for a curve of vitality.

The experiment established two points, the first being the presence of fluctuations of vitality of fairly regular character—"rhythms," they have been called. The curve alternately rises and falls, and each complete "rhythm"—a rise and a fall, that is—lasts about a month. The second is that the curve, as a whole, steadily falls, each successive rise in vitality is a little less than its predecessor, each depression a little lower, until—about the 170th generation—the period of old age, of senile decay, is reached, and the race dies out.

There the matter was allowed to rest, until fresh experiments were prompted by a remarkable observation made by Prof. Loeb. He found that the unfertilised eggs of sea-urchins could be made to develop by immersing them for a few hours in sea-water containing a higher percentage of salt than ordinary. If the eggs could be artificially aroused, why not the senile paramœcia? So argued Prof.

* *Arch. d. Zool. Exp.* 1889 (2) vii. † *Arch. f. Protistenkunde*, i. 1902.
‡ *Journ. of Exp. Zool.* ii. 1905.

Calkin. Therefore, when the period of decay had arrived and the individuals were dying off rapidly, he tried placing them in various infusions. Vegetable infusions were without effect, but infusions of animal tissues, and particularly beef extract, gave the required result. The rate of growth and of reproduction reached the normal level, and death ceased. Senile decay had given way to artificial rejuvenation. Instead of 170 generations being the limit, by stimulations in the periods of depression Calkin succeeded in carrying a race to the 740th generation, and Woodworth to the 860th generation, when the individuals were still healthy and fully active. The living matter of these cells without doubt is potentially immortal!

Consider for a moment what incredible chemical activity and stability of character these figures imply. If it was possible to preserve alive all the individuals, then at the 900th generation we should have a number which would need a row of some hundreds of figures to express. The parent cell would have produced the 900th power of two individuals like itself. The increase in the bulk of active living matter which would have been formed from non-living had there been space enough and food enough is not less wonderful. At the 350th generation it would have the dimensions of a sphere larger than the known universe!* And the surface of the sphere would be growing outwards at the rate of miles a second. Nor is this all, for, in addition to the enormous chemical activity implied by a rate of growth which would, if unchecked, produce a mass of living matter larger than the known universe in less than two years, there has been throughout continuous expenditure of energy on incessant and active movement. These animals have been watched continuously for five days, and throughout that time they were ceaselessly moving!

The recurring periods of depression show that in the living machine repair is not complete, and that after a time it will, if left to itself, cease working. With the condition of ill-repair there is associated a feature of singular interest. Woodworth specially draws attention to the fact that in the periods of depressed vitality the transmission of characters is imperfect. The moulding power of heredity fails, and many "monsters" are born.

Rejuvenescence can be brought about by a great variety of media, by extracts of muscle, of brain, of pancreas, by simple salts, by alcohol even. It is not food, but a marked and abrupt change of state that is needed—a stimulant, in fact—and beef extract produces its effect not *quâ* food, but as a stimulant pure and simple. Senile decay is due to monotony, under the influence of which the vital potential wears out!

The action of alcohol is remarkable. It was added to the water in which the animals lived so that they were always immersed in a

* I owe this rough calculation to my friend Mr. Punnett.

solution of 1 part of spirit in 5,000 to 10,000 of water. In the effect produced there is the touch of nature which makes the whole world kin. The periods of depression were wiped out. The curve of vitality no longer showed the ominous recurrent depressions. At the same time the rate of growth and division—that is to say, the physiological activity—was increased by as much as 30 per cent.

Something of the same effect is produced by strychnine, but there is a remarkable and significant difference in the fundamental action of the two drugs, for whereas the beneficial effect of alcohol endures after the drug ceases to be administered, that of strychnine does not. Alcohol, as Calkin says, in spite of the prodigiously increased rate of living, "exacts no physiological usury," it is beneficial in its after effects. Strychnine is harmful in its after effects; the onset of decay and death is hastened.

What significance are we to attach to artificial rejuvenescence? There are two possibilities. The chemical agent employed may either add something which is missing or diminished in the chemical make-up of the protoplasm, or it may restore a physical state. The former implies that the chemistry of the growth process is imperfect: the process of converting non-living to living matter is subject to inaccuracies—inconceivably small it is true, since they need to be magnified to the 170th power of 2 before they destroy the working of the machine—but cumulative from generation to generation. I incline to think that senile decay is due not so much to such a chemical insufficiency as to the wearing out of a physical state, of a "potential."

Consider a special case. Thirty minutes' immersion of an individual paramœcium in very dilute solution (1 part of salt in 1,000) of potassium phosphate was found to restore vitality, and the effect persisted for 282 generations. Now, in this case the restoration and maintenance of the "vital potential," as Calkin calls it, cannot be due to the presence in the individuals of a trace of the salt, for each generation would halve the amount, so that as early as the twentieth generation less than a millionth part would be left for each individual. One is therefore driven to believe that the salt acts by restoring a state which, in the absence of natural or artificial rejuvenescence, wears out in about 170 generations.

The continual flux of energy and of matter which seems to be necessary to the maintenance of life implies a high degree of molecular mobility. It is possible that living matter, like all other forms of matter, tends to come into equilibrium with its surroundings, and to attain a condition of too great stability. To restore it the living substance needs stimulating at intervals, just as a coherer needs tapping after each electric wave has passed, in order to restore its particles to the non-conducting position. These are vague possibilities, but physical science furnishes a case so suggestively akin to artificial rejuvenescence as to merit description.

Matter is composed of molecules which in the liquid or solid state are attracted to one another by forces of prodigious power. Each molecule in the interior of a mass is pulled on the average equally in all directions. But consider the surface layer, a film only a few molecules deep. There the intermolecular forces are necessarily to a great extent unbalanced, with the result that this surface film acts something like a stretched elastic skin—it tries always to compress the mass to the smallest possible dimension. This, however, is not the only feature of the surface layer. It is also the layer which is in contact with adjacent masses of matter, gas, liquid, or solid, as the case may be.

Now, masses of matter which do not mix when in contact, and which therefore are defined by a surface of separation, are rarely, perhaps never, without influence upon one another. Interaction of the surface layer takes place, so that the balance of molecular forces is modified, incomplete chemical reactions occur, and a condition of molecular stress is produced which, amongst other things, is manifested by the development of electrical charges.

These molecular events on surfaces are very potent; they can produce effects which are impossible and even inconceivable in matter in bulk. It is, for instance, not only possible, but probable, that in the surface layers the conditions may sometimes be such as to associate decrease of volume with decrease of pressure, a relation so subversive of ordinary experience as to be unthinkable. In the surface layer a gas may be condensed to the liquid state when far above its critical temperature and below its critical pressure. Chemical changes occur or are suspended under conditions of temperature and pressure totally unlike those controlling the same changes in masses of matter. Concentration, electric conductivity, all physical properties in fact, become abnormal; therefore, when the surface energy forms a large fraction of the total molecular energy, as in films, or fluid in fine capillaries, ordinary chemical or physical knowledge fails us.

There is no lack of evidence to prove that the lifelike characteristics of colloidal matter, its capacity for storing impressions, the elusiveness of its chemical and physical states, are due to the fact that an exceptionally large fraction of its energy is in the form of surface energy.

There is also direct and unmistakable evidence in the nature of the effect of various salts upon the heart-beat, and in the optical characters of thin films, that living matter also contains a very large proportion of surface energy per unit of mass, and the curious and extreme physical and chemical powers which it manifests are without doubt largely due to this cause. Now it is just in experiments on surface energy that one finds a case analogous to the effect of the salt in bringing about rejuvenescence of senile protoplasm, or in awaking the dormant powers of an unfertilised egg.

It has been shown recently by a French physicist, M. Perrin,* that by the use of minute amounts of salts one can give to the surface energy of a solid a certain direction—one can fix in the surface layer certain qualities which, for instance, define the electric properties of the surface. The effect once produced, no amount of washing will undo it; the salt can be removed, the effect remains. So far as we know, in the absence of active chemical intervention it will endure for all time, always exerting a directive influence upon the molecular events in its neighbourhood. In these experiments there is, it seems to me, a real clue to the nature of the phenomena of rejuvenescence.

Heredity.

On the earth are some half-million different species of animals and plants, each of which breeds true in virtue of what we call heredity. Each species therefore represents a strain or line of descent of living matter always growing, dividing, and increasing in mass, like the little paramœcia we have already considered, each striving to occupy the whole earth, and restrained in the attempt only by the accident of death.

The strains of living matter are separated from one another by a wide gulf which we do not know how to bridge. Change of state seems to be without effect. Continuous supplies of the richest food will not convert a strain of dwarfs into giants. In the solemn words of the Burial Service, "All flesh is not the same flesh, but there is one kind of flesh of men, another flesh of beasts, another of fishes, and another of birds."

The nature of these differences in the kinds of living matter and their mastery so that we may be able to control them is without doubt the most difficult and the most important problem which science has attempted to solve: most difficult because it deals with a form of matter much more complex than any which the chemist or physicist so far has considered; most important because on the solution of this problem depends the possibility of removing practical medicine, politics, and morality from the domain of empiricism and tradition to that of rational co-ordinate knowledge.

To speak of a strain breeding true is a bald way of describing a force so potent as heredity, so impish in its eccentricities. On the Antarctic ice there abounds a race of birds called penguins. They have never seen a tree since they first were penguins; they do not fly, for their wings have been reduced to small flat paddles with which they swim. The bird cannot tuck its head under its wing, because the wings are too shrunken; but still, in mute worship and touching fidelity to its forbears of thousands of years ago, when it

* *Journ. d. Chem. Physique*, ii. p. 61, and iii. p. 50.

composes itself to sleep each individual bends round its head and tucks the tip of the beak—it is all it can do, poor thing!—under the dwarfed wing.

This lingering instinct, this obsession by the great past, is like a whale dreaming of the green fields in which his forefathers browsed! Now, each individual penguin starts life as a microscopic fragment of living matter, a single cell, so wonderfully compounded, so cunningly devised, as to enshrine without loss all the diverse qualities and powers which the word "penguin" connotes, down to the trivial detail I have described! There is little wonder that the naturalists of half a century ago gave the problem up in despair. There is cause for wonder and for congratulation that, impelled by the divine dipsomania for research, knowledge has moved so far as to make a beginning in the assaults.

Given a fulcrum, anything can be moved. The necessary fulcrum was found when attention was directed, not, as in Huxley's time, to the more obvious resemblances between the different kinds of protoplasm, but to the less obvious differences. The microscope for the most part fails us here, in the first place because the discrimination between different kinds of matter by the agency of sight is possible only when there are associated differences in optical properties, and when there is the possibility of getting a clear image. Now, living matter is singularly free from definite optical differences; it has the optical characters of ground glass. Therefore, the ultimate refinements of microscopic vision are for the most part wasted upon it. The dead cell exhibits remarkable structural details, but in the act of death there is of necessity a redistribution of matter which obscures and defaces the finer details of the real living structure, and replaces them by structure which is formed in the process of dying. For the material basis of the difference in the strains of living matter we have to look below the limits of microscopic vision, below the limits even of the living molecule, to the chemical molecule of which that living matter is built up.

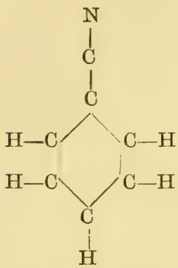
The nearest chemical approach to living matter is the proteid, the chemical substance of which all protoplasm is, water excepted, chiefly composed. And the fulcrum I spoke of, or, better, the thought which loosed the fetters of imagination, was the appreciation of the significance of the fact that proteids chemically are not all alike, and that the strains of living matter differ from one another in the kinds of proteid of which they are built up—that is to say, in their ultimate chemical constitution.

All proteids are not the same proteids: there are proteids of men, others of beasts, others of fishes, and others of birds!

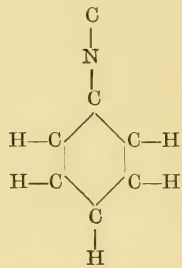
The nature of the differences leads us to a real picture of the underlying differences between the kinds of protoplasm. The tide of thought of the older observers was fettered by the fact that all proteids have about the same atomic composition. The biologist of

to-day owes his emancipation to the chemical discovery that the properties of a complex substance are defined not so much by the kind of atoms or number of atoms of which it is built, as by the arrangement of those atoms in space.

Here is a simple and startling case. The molecules of two chemical substances, benzonitrile and phenylisocyanide, are composed of seven atoms of carbon, five of hydrogen, and one of nitrogen :



BENZONITRILE.



PHENYLISOCYANIDE.

There is a small difference in the arrangement of these atoms which is illustrated by the diagram. Now, what are the properties of these two substances? They are as unlike as possible. The first is a harmless fluid with an aromatic smell of bitter almonds. The second is very poisonous and offensive.

A vivid impression in regard to the odour of the isocyanides may be produced by the following experiment. In a test-tube bring together a *little* chloroform, aniline, and alcoholic potash. The reaction takes place at once. "*It is better to perform the experiment out of doors and in such a place that the tube with its contents can be thrown away without molesting any one.*"

In the building of a complex molecule one has atoms gathered together to form groups, these to form larger groups, and the whole structure is arranged on a fundamental plan or style, like, for instance, the ring of carbon atoms in the two substances just mentioned. There is, therefore, a molecular architecture, and, as in ordinary architecture, there are differences of style and of general plan, Gothic, Norman, etc., with endless variety in detail. Amongst the recognised styles of molecular architecture is the proteid style, and the qualities common to all forms of life are based ultimately upon the essential features of that style, while the differences between one kind of living matter and another are the expression of the differences in detail—the omission of this group, the addition of that.

Some of the atomic groups which find a place in the proteid molecule are readily recognisable by chemical tests—one of these groups occurs as a separate chemical substance called *Tryptophane*. It shows a vivid purple colour with sulphuric acid and reduced oxalic

acid. Here are solutions of two proteids, one from maize seeds, the other from the white of egg. The former lacks, the latter possesses this group.

In order to represent the great varieties of living matter the proteid molecules must be capable of very many variations of structure. That is, after all, mainly a question of size—the larger it is the greater the possibility of variations in detail; and as the molecule of proteid seems to contain from ten to thirty thousand atoms, whereas the most complex molecule known to the organic chemist contains less than a hundred, there is no lack in this respect.

Proteids unquestionably are the material basis of life, but when isolated after the death of the cell they are not living. They are chemically stable bodies. They show no signs of the characteristic chemical flux. It is therefore conjectured on experimental grounds that the living molecule is built up of proteid molecules, that it is so complex, so huge, as to include as units of its structure even such large molecules as these. But when such very large molecules enter into chemical combination with one another, whether by reason of the great magnitude of the masses of matter in each in relation to the magnitude of the directive forces, or because the molecules themselves, owing to their great size, to a certain extent cease to be molecules at all in the physical sense, and possess the properties of matter in mass, it is at any rate certain that in their chemical combinations they cease to follow the law of definite combining weights which is the basis of chemistry. The quantity of the substance A which will combine with a fixed quantity of the substance B is determined not only by the chemical nature of A and of B, but also by the chance conditions of temperature and concentration of the moment. This class of chemical compounds is within limits continuously adjustable to changes in its surroundings, while at the same time it resists those changes by reason of its inertia. Here is a real adumbration in non-living matter of the chemical flux which is the abiding characteristic of the matter of life.

The biologist speaks of those molecular complexes as *molecules*, and in that he is wrong in so far as the word implies a *defined* structure, a chemical unit. The biogen, or chemical unit of living matter, is not a fixed unit like the molecule of dead proteid; it is an average state. That we know from the chemical phenomena of living matter.

Why should this be? Consider what must happen if you make the atomic building much larger than it already is in the molecule of dead proteid. You already have a molecule so large as to be liable to fracture on mere mechanical agitation. A molecule composed of fifty proteid molecules would cease to be a molecule in the physical sense: it would be matter in mass, defined by a surface; it would break up the waves of light, so radiant energy would profoundly affect it.

In a mass so large, a portion of the energy would of necessity be

in a borderland between what we call osmotic energy and surface energy, the fraction in the one state or the other being determined from moment to moment by the changing relations with the enveloping matter. If the chemical structure was such as to produce a shape other than a sphere, surface energy would tend to produce chemical rearrangements, and the opposing play of these forces might result in oscillations of form which would reflect the irregular flux of cosmical forces just as does the particle in Brownian movement. The chemical relations of such a mass would be defined in the first instance by the surface layer, but any simple chemical event on the surface would be likely to fire a train of events leading to an eruption like a sun-spot on the sun.

It is not, I think, difficult on these lines to conceive of a substance the chemical units of which could maintain themselves only in virtue of a continual flux of matter and energy—only, that is, as an average state; but it is certain that to develop the hypothesis we need what has not, so far as I know, yet been begun—namely, a kinetic theory of those intramolecular relations of atoms which are statically expressed by the geometrical methods of stereo-chemistry. The living cell, like a gas engine at work, is a chemical vortex, and there is no hope of analysing the motions of its parts so long as we are limited to statical methods.

In the history of the study of heredity there is a note of tragedy. In the early days of last century Lamarck began the revolt against the dogma of the immutability of species, which culminated in 1859 in the publication of *The Origin of Species*. Between Lamarck and Darwin, however, stand a scanty band of men forgotten by all but a few specialists, who strove by experiments in cross-fertilisation to pierce the mystery of heredity. Amongst them, and the last of the line, was a monk of the Abbey of Brunn, one Gregor Mendel, who in 1865 communicated to the Brunn Natural History Society the results of eight years devoted to experiments with peas, under the modest title of *Experiments in Plant Hybridisation.*

The fate of Darwin's work is known to everyone: how "it was considered a decidedly dangerous book by old ladies of both sexes," and how, "overflowing the narrow bounds of scientific circles, it divided with Italy and the Volunteers the attention of general society." The fate of Father Mendel's work was different. For the rest of the century it lay completely forgotten and buried in the annals of the little local society. But when it was rediscovered in 1900 by Professor de Vries, of Amsterdam, it was at once realised by the very few competent to judge that the pursuit of a hobby in the abbey garden had led to a theory of the nature and workings of heredity so clear and complete as to leave to others only the application of principles and the amplification of details.

To find an achievement parallel to Mendel's, in the difficulty of the problem attacked and the all-embracing nature of the solution

reached, one has to turn to Willard Gibbs's clean sweep of the domain of chemical equilibrium. But the author of the Phase Rule lived to see the work rediscovered—again by a Professor of the University of Amsterdam—and become the inspiration of a cloud of workers in all lands. The Mendelian laws of heredity, established twenty years earlier, are only now beginning to bear fruit, twenty years after Mendel's death.

The magnitude of Mendel's achievement can be appreciated by calling to mind the acute intellects which have been foiled by the problem. For a century the study of heredity has remained a repellent mass of statistics, with scarcely more discernible order than might be found in any chance collection of facts; and of the would-be student it might be said, "*Quæsivit cœlo lumen ingemuitque reperta.*" And for half of that century there has lain hidden a solution of the riddle which brings these facts into an order so straightforward that a child might learn it.

We should have nothing to do with the Mendelian laws here were it not that they have given singular meaning and interest to certain details of cell structure which before were a mere collection of unintelligent facts. To take things in their proper sequence, I will first state the laws of inheritance so far as they concern us, and then consider the structural characters which seem to be their material basis.

The first Mendelian principle which concerns us is this: that what is transmitted from generation to generation may be analysed into certain qualities or characters—constant characters as Mendel calls them—each of which is a unit in heredity, each of which, therefore, is capable of independent transmission. Thus in peas are length of stem, character of inflorescence, colour of seeds, flavour, and so on. Underlying these characters—each of which is capable of being picked out or put back by a breeder, forming a substrate on which they are erected—there would seem to be a basal character which is inalienable and which the breeder cannot, at present at any rate, touch. Thus, in the case of peas, what is of necessity transmitted is the fundamental qualities of "plant" as opposed to "animal," and of "pea" as opposed to other plants. To proceed in Mr. Bateson's words:

"These [unit] qualities or characters whose transmission in heredity is examined are found to be distributed among the germ cells, or gametes, as they are called, according to a definite system. This system is such that these characters are treated by the cell divisions (from which the gametes result) as existing in pairs, each member of a pair being alternative to the other in the composition of the germ. Now, as every zygote—that is, any ordinary animal or plant—is formed by the union of two gametes [in the process of sexual fertilisation], it may either be made by the union of two gametes bearing similar members of any pair, say two blacks or two whites, . . . or the gametes from which it originates may be bearers of the dissimilar

characters, say a black and a white. [In the first case, no matter what its parents or their pedigrees may have been, the zygote breeds true indefinitely, unless some fresh variation occurs.]

"If, however, the zygote be gametically cross-bred, its gametes [or germ cells] in their formation separate the pair of characters again, so that each gamete contains only one character of each pair. At least one cell division in the process of gametogenesis is therefore a differentiating or segregating division, out of which each gamete comes sensibly pure in respect of the unit characters it carries, exactly as if it had not been formed by a cross-bred zygote at all."

For our purposes this may be reduced to three propositions: (1) that inheritance consists in the transmission of independent characters, of which each race or species possesses a definite number; (2) that these characters form pairs of opposites or alternatives; (3) that in the formation of the germ cells these characters are sorted out and distributed so that no germ cell carries both members of a pair. Can any material basis be found for these? To this we will now turn.

Five years ago it is doubtful whether there existed in the whole domain of science such a charnel-house of dead facts as in that of the science of cell structure. Thirty years of active study of animal and plant cells prepared for microscopical examination in various ways had resulted in the accumulation of a multitude of details respecting the structure of the cell nucleus and of the extraordinary way in which it behaves in cell division, and especially in those cell divisions which produce the germ cell. It was known that from the characteristic substance of the nucleus—which stains very deeply with aniline dyes, and hence is called chromatin—a continuous thread is spun as the first step in cell division, and that this thread of chromatin splits across into rods called chromosomes, each of which again splits, this time not across, but lengthwise, so as to form two "daughter" chromosomes, which, under the influence of a peculiar field of force formed in the substance of the cell, move away from one another and gather at the opposite poles of the spindle-shaped field, there to fuse and form the two nuclei of the "daughter" cells.

A further very significant and curious fact was known—namely, that the number of chromosomes formed in the process is not a chance one, but, in the first place, it is always an even number, and, in the second place, each species of animal or plant has a characteristic number. In the division of the cells of the human body, for instance, there are formed thirty-two chromosomes. But to these and many other similar facts no significance could be attached, beyond the obvious one that the nuclear substance is not divided grossly to form a new cell generation, but distributed by a complex and minutely detailed process of subdivision and segregation.

Not only were the facts of nuclear division without significance,

but the presence of the nucleus itself seemed to be meaningless. The contractility of the muscle cell, the conductivity of the nerve cell, the chemical activities of the gland cell, reside in the cell body, and not, save perhaps in the last case, at all in the nucleus. Throughout cell life it lies to all appearance an inert mass, which becomes active only in the process of cell division. And yet actual experiments on enucleated fragments of protozoon cells and on the nerve cells of higher forms had proved that in the absence of the nucleus the cell body cannot live. True, it carries on all the life functions for a while, but it seems to have lost with the nucleus the power of growth and of repair.

The last six years have witnessed the rehabilitation of the nucleus, and biologists now see in it the seat of that influence which directs the formative process by which living matter is produced from non-living matter, and controls the distribution of characters in heredity.

The actual agent in the latter process seems to be the chromosome, and the material basis of the limitation in the number of characters transmitted from generation to generation, and of their definiteness lies in the restriction of the number of chromosomes to a definite number for each species of animal or plant. The chromosomes are not fragments of nuclear substances of accidental composition, nor are they all alike. On the contrary, the probability is that they are unlike, and possessed of a high degree of individuality.

The material process which underlies the segregation of characters in the germ cell and the fusion of characters in pure and cross-bred zygotes can also be followed in the peculiar features of the cell divisions which form the male and female gametes.

As I have already said, each species has a characteristic number of chromosomes, but in the cell divisions which form ova or spermatozoa, ovule or pollen grain, this number is halved, so that each spermatozoon or ovum receives only half the proper number. In this sense, therefore, the germ cell is only half a cell. When two germ cells fuse in the act of fertilisation, the full number of chromosomes is restored. Thus, to choose an instance, the full number of chromosomes which make up a nucleus in a cell of the human body, no matter where it be placed, is 32 ; in the formation of the spermatozoon or ovum, however, there is a redistribution of chromatin, so that each receives only 16 chromosomes. When a spermatozoon fuses with an ovum, a zygote with the full number, 32, is formed.

The chromosomes therefore are the elements, the organs, as it were, of heredity. They have individuality, the limitations of which are not yet known. Each bears a unit character or a group of unit characters. The evidence for the individuality of the chromosomes is very remarkable.

Fundulus and *Menidia* are two fishes belonging to separate orders. Each has 36 chromosomes, but the chromosomes of the former are so much longer than those of the latter as to be readily distinguishable.

Moenkhaus* crossed these two forms, and traced the fusion of the long and short chromosomes in the formation of the hybrid zygote. But when the zygote prepared to divide, the paternal and maternal elements segregated and formed two groups of chromosomes, the one of long and the other of short chromosomes, and in each segment division the paternal long and the maternal short chromosomes reappeared and acted independently. Another case has been furnished by Wilson.† In certain groups of insects there is among the chromosomes of the male cells one distinguished by its small size. The total number of chromosomes, instead of being even, is odd: there are thirteen, and this small chromosome is the thirteenth. It is, in point of fact, only half a chromosome, therefore when each of the others divides into two, it does not divide, but passes bodily to one or other of the two new cells. In this way two different kinds of spermatozoa are formed—those which possess the odd half chromosome, and those in which it is missing—and they are formed in equal numbers. Now, ova fertilised by the former grow into females, those which are fertilised by the latter grow into males. Therefore this particular chromosome is the carrier of the sex character.

I have stated the theory of the mechanism of heredity as it seems to be developing. A word of caution, however, is necessary. It is quite possible that we are attaching too much importance to the chromosome simply because, owing to the affinity of its substance for dyes we can follow it in the phases of cell history. The rest of the nucleus and cell body does not happen to show such constant affinities, and therefore the sense of sight yields no evidence as to their action in cell division. Yet, so far as we know, the same detailed processes of synthesis and analysis which we can follow in the chromatin substance may divide the units of the rest of the cell in cell division, and guide the half of each unit to its allotted place in the architecture of the new cells.

The observations of Conklin * upon a curious ascidian egg makes this even probable. The body of this egg is built of five kinds of protoplasm recognisably different to sight during life. These are (1) deep yellow, (2) light yellow, (3) light grey, (4) slate grey, (5) clear transparent. Each of these has a separate history; the deep yellow protoplasm makes the muscular system, the light grey the brain, the clear transparent the skin, and so on. This egg therefore is a mosaic, an architecture of different kinds of living matter, which we can detect and follow owing to associated optical differences. Had these been absent, we should have known as little of the architecture of this egg as we know of that of eggs in general.

The independent transmission of characters, and the presence in the germ cells of different kinds of living matter, are indisputable

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* *Amer. Journ. Anat.* iii. 1904. † *Journ. of Exp. Zool.* iii. 1906.

* *Journ. of Exp. Zool.* ii. 1905.

They lead us, however, to a riddle which I leave to my readers to solve as they will. We are driven to believe that in the material make-up of any race there are several kinds of living matter which cannot be changed the one into the other, and of which some will mix, others will not or cannot mix. These materials, bricks, as it were, in the building, are transmitted from generation to generation by the agency of the germ cells, which therefore are heterogeneous structures.* Now, the doctrine of the direct transmission of the various living substances employed in the make-up of the individual lands us in this difficulty. The fertilised egg has all the material necessary for the make-up, therefore it can, and does, develop into an adult. The generative cells also possess amongst them all the necessary material. Therefore, amongst the earlier generations of cells produced by the growth and division of the fertilised ovum, that cell or those cells which will form the generative organs must contain all the substances. But direct experiment contradicts this conclusion. Possibly in the very first cleavage, certainly in the second cleavage of the egg, there is a distribution of material amongst the two or four cells such that each one lacks something in the general make-up, and therefore can, and will, grow only to an imperfect monster if isolated. But one of those four incomplete cells will give rise, amongst other things, to the generative organs, each cell of which, in the first instance, is complete. Therefore, as we may "neither confound the persons nor divide the substance," we seem to be in a region of incomprehensibles.

"Just as that normal truth to type," says Bateson, "which we call heredity is in its simplest elements only an expression of that qualitative symmetry characteristic of all non-differentiating cell divisions, so is genetic variation the expression of a qualitative asymmetry beginning in gameto-genesis [the genesis, that is, of the germ cells]. Variation is a novel cell division. . . . What is the cause of variation ?" Cross breeding—that is, the union of unlike germ cells—may modify the character units. So, too, apparently may the long-continued *absence* of cross breeding. It has been noticed in the cycles of a pure strain of paramœcium that the periods of depressed vitality are also periods when the directive force of heredity is weakened. The individuals of successive generations show great departures from the normal type, and monsters are of frequent occurrence. With the lowered rate of growth, the lowered "vitality," as we call it, for want of a more precise word, there is associated a lowered degree of fixity of type. [W. B. H.]

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* The beginnings of the science of their architecture are to be found in the last report of Mr. Bateson and Mr. Punnett to the Committee of the Royal Society on Evolution.

GENERAL MONTHLY MEETING,

Monday, June 11, 1906.

The Right Hon. SIR JAMES STIRLING, P.C. M.A. LL.D. F.R.S., Vice-President, in the Chair.

Mrs. Francis Elgar,
Miss Hilda Hanbury,
Ernest L. Mansergh, Esq., M. Inst. C.E.
Mrs. Vivian Morse,
Carl D. Page, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures :—

Sir Andrew Noble, K.C.B. F.R.S.	£100
Mrs. Frank Lawson	50

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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Washington Academy of Sciences—Proceedings, Vol. VIII. pp. 1–90. 8vo. 1906.
Western Australia, Agent-General—Monthly Statistical Abstract, March, 1906. 4to.
Supplement to Government Gazette for March–April, 1906. 4to.
Western Society of Engineers—Journal, Vol. XI. No. 2. 8vo. 1906.
Yorkshire Philosophical Society—Annual Report, 1905. 8vo. 1906.
Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, 1905, Heft 4. 8vo. 1906.

GENERAL MONTHLY MEETING,

Monday, July 2, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Theophilus Bulkley Hyslop, M.D.
John Gray McKendrick, M.D. LL.D. F.R.S. F.R.C.P.
The Hon. Arthur Stanley, M.V.O. M.P.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to The Right Hon. Lord Sanderson, G.C.B. K.C.M.G. *M.R.I.*, for his Donation of £5 5*s*. to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Report of the Board of Scientific Advice for India, 1904–5. 8vo. 1906.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XV. 1° Semestre, Fasc. 11. 8vo. 1906.
Astronomer Royal—Report to the Board of Visitors, 1906. 4to.
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Belgium, Royal Academy of Sciences—Bulletin, 1906, Nos. 3–4. 8vo.
Boston Public Library—Monthly Bulletin for June, 1906. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIII. Nos. 15–16. 4to. 1906.
British Association for the Advancement of Science—Report of the Seventy-fifth Meeting, South Africa, 1905. 8vo. 1906.
British Astronomical Association—Journal, Vol. XVI. No. 8. 8vo. 1906.
The Total Solar Eclipse, 1905. 8vo. 1906.
Buenos Ayres—Monthly Bulletin for April, 1906. 4to.
Cambridge University Library—Report of the Library Syndicate for 1905. 4to. 1906.
Carson, T. G., Esq. (the Author)—Man's Responsibility. 8vo. 1906.
Chemical Industry, Society of—Journal, Vol. XXV. No. 11. 8vo. 1906.
Chemical Society—Proceedings, Vol. XXII. Nos. 311-312. 8vo. 1906.
Journal for June, 1906. 8vo.
Church of England League—Gazette for July, 1906. 8vo.
Cotton, Major-General F. C., The Executors of the late—On the Water of the Great Rivers of India. By F. C. Cotton. 8vo. 1901.
Editors—Analyst for June, 1906. 8vo.
Athenæum for June, 1906. 4to.

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Chemical News for June, 1906. 4to.
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Concrete for July, 1906. 8vo.
Dioptric Review for June, 1906. 8vo.
Electrical Engineer for June, 1906. 4to.
Electrical Review for June, 1906. 4to.
Electrical Times for June, 1906. 4to.
Electricity for June, 1906. 8vo.
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Engineering for June, 1906. fol.
Journal of the British Dental Association for June, 1906. 8vo.
Journal of State Medicine for June, 1906. 8vo.
Law Journal for June, 1906. 8vo.
London University Gazette for June, 1906. 4to.
Machinery Market for June, 1906. 8vo.
Model Engineer for June, 1906. 8vo.
Motor Car Journal for June, 1906. 8vo.
Musical Times for June, 1906. 8vo.
Nature for June, 1906. 4to.
New Church Magazine for July, 1906. 8vo.
Nuovo Cimento for March, 1906. 8vo.
Page's Weekly for June, 1906. 8vo.
Photographic News for June, 1906. 8vo.
Physical Review for June, 1906. 8vo.
Public Health Engineer for June, 1906. 8vo.

Florence Biblioteca Nazionale—Bulletin for June, 1906. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. II. Disp. 4. 8vo. 1906.
Franklin Institute—Journal, Vol. CLXI. No. 6. 8vo. 1906.
Geological Society—Abstracts of Proceedings, No. 831. 8vo. 1906.
Harlem, Musée Teyler—Archives, Série II. Vol. X. 2e Partie. 8vo. 1906.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Série II. Tome XI. 3e Livraison. 8vo. 1906.
Junior Institution of Engineers—Transactions, Vol. XV. 8vo. 1906.
London County Council—Gazette for June, 1906. 4to.
Mechanical Engineers, Institution of—Proceedings, 1905, Parts 3-4. 8vo. 1906.
List of Members, 1906. 8vo.
Mersey Conservancy—Report on the River Mersey, 1905. 8vo. 1906.
Meteorological Office—Life History of Surface Air Currents. By W. N. Shaw and R. G. K. Lempfert. 4to. 1906.
Microscopical Society, Royal—Journal, 1906, Part 3. 8vo.
Moscow University—Le Physiologiste Russe, Vol. IV. Nos. 68-74. 8vo. 1906.
Munich, Royal Academy of Sciences—Abhandlungen, Band XXII. Ab. 3. 4to. 1906.
Sitzungsberichte, 1906, Heft 1. 8vo.
Navy League—Journal for July, 1906. 8vo.
North of England Institute of Mining Engineers—Transactions, Vol. LVI. Part 2. 8vo. 1906.
Nottingham Public Libraries Committee—Report, 1905-6. 8vo.
Odontological Society—Transactions, Vol. XXXVIII. No. 7. 8vo. 1906.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May, 1906. 4to.
Pharmaceutical Society of Great Britain—Journal for June, 1906. 8vo.
Photographic Society, Royal—Journal, Vol. XLVI. No. 6. 8vo. 1906.
Rio de Janeiro Observatory—Bulletin, April-June, 1905. 8vo.
Annual, 1906. 8vo.

Royal Engineers, Corps of—Journal, Vol. IV. No. 1. 8vo. 1906.
Royal Society of Edinburgh—Proceedings, Vol. XXVI. No. 3. 8vo. 1906.
Royal Society of London—Proceedings, Vol. LXXVII. A, No. 520; B, No. 521. 8vo. 1906.
Reports to the Evolution Committee, No. III. 8vo. 1906.
Saxon Academy of Sciences, Royal—Abhandlungen, Band XXIV. No. 4; Band XXIX. Nos. 5–6. 4to. 1906.
Berichte: Math.-Phys. Klasse, 1905, Nos. 5–6; 1906, Nos. 1–2; Phil.-Hist. Klasse, 1905, Nos. 5–6; 1906, Nos. 1–2. 8vo. 1905–6.
United Service Institution, Royal—Journal for June, 1906. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XVII. Nos. 8–9. 8vo. 1906.
Monthly Weather Review for February, 1906. 4to.
Upsala University Meteorological Observatory—Monthly Bulletin, Vol. XXXVII. 1905. 4to. 1906.
Western Australia, Agent-General—Monthly Statistical Abstract for April, 1906. 4to.
Acts of Parliament, 1905. 8vo. 1906.
Supplement to Government Gazette for May, 1906. 4to.

GENERAL MONTHLY MEETING,

Monday, November 5, 1906.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

The Special Thanks of the Members were returned to Dr. Albert P. Brubaker for his gift of a Portrait of the late Professor Tyndall; and to Mr. W. Hugh Spottiswoode, *M.R.I.*, for his donation of Physical Apparatus.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Lords of the Admiralty—Report of H.M. Astronomer at the Cape of Good Hope, 1905. 4to. 1906.

The Secretary of State for India—Geological Survey of India: Records, Vol. XXXIII. Part 4; Vol. XXXIV. Part 1. 8vo. 1906.
Kodaikanal Observatory Bulletin, Nos. 5–6. 4to. 1906.
Agricultural Journal of India, Vol. I. Part 3. 8vo. 1906.
Memoirs of Department of Agriculture, Vol I. No. 4; Botanical Series, Vol. I. Nos. 1–3. 8vo. 1906.
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Ethnographic Notes in Southern India. By E. Thurston. 8vo. 1906.

British Museum (Natural History)—Catalogue of the Tertiary Vertebrata of the Fayûm, Egypt. By C. W. Andrews. 4to. 1906.
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Aberdeen, University of, Publications Committee—Handbook to University of Aberdeen-Quatercentenary, September 1906. 8vo. 1906.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XV. 1º Semestre, Fasc. 12; 2º Semestre, Fasc. 1–6. 8vo. 1906.
Classe di Scienze Morali: Rendiconti. Serie Quinta, Vol. XV. Fasc. 1–4. 8vo. 1905.
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American Academy of Arts and Sciences—Proceedings, Vol. XLI. Nos. 30-35; Vol. XLII. Nos. 1–11. 8vo. 1906.

American Geographical Society—Bulletin, Vol. XXXVIII. Nos. 6–9 8vo. 1906.

American Philosophical Society—Proceedings, Vol. XLV. No. 182. 8vo. 1906.
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Amsterdam, Royal Academy of Sciences—Verhandelingen, 1e Sectie, Dl. IX. Nos. 2–3; 2e Sectie, Dl. XII. Nos. 3-4. Proceedings, Vol. VIII. Verslag, Vol. XIV. Jaarboek, 1905. 8vo. 1905–6.

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Basel, Naturforschenden Gesellschaft—Verhandlungen, Band XVIII. Heft 3. 8vo. 1906.
Batavia, Royal Magnetical and Meteorological Observatory—Observations, 1904. 4to. 1906.
Bear, J., Esq. (the Author)—Impressions of a Victorian Abroad. 8vo. 1906.
Belgium, Royal Academy of Sciences—Bulletin, 1906, Nos. 5–6. 8vo.
Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1906, Nos. 23-38. 8vo.
Bohemian Section, Austrian Exhibition, London, 1906—Guide to the Bohemian Section and the Kingdom of Bohemia. 8vo. 1906.
Booraem, J. V. V. (the Author)—Internal Energy. 8vo. 1906.
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Botanic Society, Royal—Quarterly Record, Vol. IX. Nos. 106–107. 8vo. 1906.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIII. Nos. 17–20. 4to. 1906.
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List of Members. 8vo. 1906.
British Royal Commission, St. Louis Exhibition, 1904—Souvenir of the British Section. Compiled by Sir Isidore Spielmann. 4to. 1906.
Brooklyn Institute—Science Bulletin, Vol. I. No. 8. 8vo. 1906.
Cold Spring Harbour Monographs, VI. 8vo. 1906.
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Cambridge Observatory—Annual Report, 1905-6. 4to.
Cambridge Philosophical Society—Transactions, Vol. XX. Nos. 9–10. 4to. 1906.
Canada, Geological Survey—Annual Report, Vol. XIV. 1901. (With Maps.) 8vo. 1905.
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Chemistry, Institute of—List of Official Chemical Appointments. Compiled by R. B. Pilcher. 8vo. 1906.
Chicago, Field Museum of Natural History—Publications: Geological Series, Vol. III. Nos. 3–4; Zoological Series, Vol. VII. Nos. 2–3. 8vo. 1905–6.
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Cortie, Rev. A. L. (the Author)—The Total Solar Eclipse of 1905. 4to. 1906.
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Dax, Société de Borda—Bulletin, 1905, Parts 3–4. 8vo.
East India Association—Journal, Vol. XXXIX. No. 42. 8vo. 1906.
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Author for July–Nov. 1906. 8vo.
Chemical News for July–Oct. 1906. 8vo.

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Chemist and Druggist for July–Oct. 1906. 8vo.
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Dioptric Review for Aug.–Oct. 1906. 8vo.
Dyer and Calico Printer for Aug.–Oct. 1906. 4to.
Electrical Contractor for July–Oct. 1906. 8vo.
Electrical Engineer for July–Oct. 1906. 4to.
Electrical Industries for Sept.–Oct. 1906. 4to.
Electrical Review for July–Oct. 1906. 4to.
Electrical Times for July–Oct. 1906. 4to.
Electricity for July–Oct. 1906. 8vo.
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Horological Journal for July–Nov. 1906. 8vo.
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Journal of State Medicine for July–Oct. 1906. 8vo.
Law Journal for July–Oct. 1906. 8vo.
London University Gazette for July–Oct. 1906. 4to.
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Model Engineer for July–Oct. 1906. 8vo.
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Nuovo Cimento for April–June, 1906. 8vo.
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Photographic News for July–Oct. 1906. 8vo.
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Public Health Engineer for July–Oct. 1906. 8vo.
Terrestrial Magnetism, Vol. XI. No. 2, June 1906. 8vo.
Zoophilist for July–Oct. 1906. 4to.

Electrical Engineers, Institution of—Journal, Vol. XXXVI. No. 178; Vol. XXXVII. No. 179. 8vo. 1906.

Faraday Society—Transactions, Vol. II. Part 1. 8vo. 1906.

Florence Biblioteca Nazionale—Bulletin for July–Oct. 1906. 8vo.

Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. III. Disp. 1–3. 8vo. 1906.

Franklin Institute—Journal, Vol. CLXII. Nos. 1–4, 1906. 8vo.

Geneva, Société de Physique—Mémoires, Vol. XXXV. Fasc. 2. 4to. 1906.

Geographical Society, Royal—Journal, Vol. XXVIII. Nos. 1–5. 8vo. 1906.

Geological Society—Abstracts of Proceedings, No. 832. 8vo. 1906.
Quarterly Journal, Vol. LXII. Part 3. 8vo. 1906.

Gillespie, Mrs. Honyman, The Trustees of—The Argument A Priori. By W. H. Gillespie. 8vo. 1906.

Göttingen, Royal Academy of Sciences—Nachrichten, 1906: Math.-Phys. Klasse, Heft 2; Geschaftliche Mittheilungen, Heft 1. 8vo.

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Historical MSS. Commission—Report on American MSS. in the Royal Institution, Vol. II. 8vo. 1906. (2 copies.) Also 2 copies, 4to (unbound).

Imperial Institute—Bulletin, Vol. IV. No. 2. 8vo. 1906.

Iron and Steel Institute—Journal, Vol. LXIX.–LXX. 8vo. 1906.
List of Members. 8vo. 1906.

Jefferson Physical Laboratory—Contributions, Vol. III. 8vo. 1906.

Johns Hopkins University, Baltimore—American Journal of Philology, Vol. XXVII. No. 2. 8vo. 1906.

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Lehmann, O., Esq. (the Author)—Papers on Crystallography. 8vo and 4to. 1906.

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Journal: Botany, Vol. XXXVII. Nos. 260–1; Zoology, Vol. XXIX. No. 194. 8vo. 1906.
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Liverpool University, Institute of Commercial Research in the Tropics—Quarterly Journal, Vol. I. No. 3. 8vo. 1906.
London County Council—Gazette for July–Oct. 1906. 4to.
Madrid, Royal Academy of Sciences—Revista, Tomo III. No. 6; Tomo IV. Nos. 1–6. 8vo. 1905–6.
Memorias, Tomo XXIV. 4to. 1906.
Manchester Geological Society—Transactions, Vol. XXVIII. Part 21. 8vo. 1906.
Manchester Literary and Philosophical Society—Memoirs, Vol. L. Part 3. 8vo. 1906.
Manchester Municipal School of Technology—First Annual Report of the Godlee Observatory, 1905. 8vo. 1906.
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Massachusetts Institute of Technology—Technology Quarterly, Vol. XIX. No. 2. 8vo. 1906.
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Report on Scarlet Fever and Diphtheria, 1902–4. 4to. 1906.
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Milan School of Agriculture—Annuario, Vol. V. 8vo. 1905.
Monaco, H.S.H. The Prince of—Bulletin du Musée Océanographique de Monaco, Nos. 78–82. 8vo. 1906.
Mond, Robert, Esq., M.A. F.R.S.E. M.R.I.—Aramaic Papyri discovered at Assuan. Edited by A. H. Sayce. Text and Plates. fol. 1906.
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Natal, Agent-General for—Twentieth Century Impressions of Natal. 4to. 1906.
National Physical Laboratory—Report of Observatory Department, 1905. 4to. 1906.
Navy League—Navy League Journal for Aug.–Oct. 1906. 8vo.
New Jersey, Geological Survey—Annual Report, 1905. 8vo. 1906.
New South Wales, Agent-General—Year Book, 1904–5. 8vo. 1906.
Report on Prisons, 1905. 4to. 1906.
New York Academy of Sciences—Annalls, Vol. XVI. Part 3. 8vo. 1906.
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Nodon, Dr. A. (the Author)—L'Action Electrique du Soleil sur la Terre (MSS.) 4to. 1906.

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North of England Institute of Mining Engineers—Transactions, Vol. LVI. Part 3. 8vo. 1906.
  Annual Report, 1905-6. 8vo.
Odontological Society—Transactions, Vol. XXXVIII. No. 8. 8vo. 1906.
Onnes, Dr. H. Kamerlingh, Hon.Mem.R.I. (*the Author*)—Communications from the Leiden Physical Laboratory, No. 85, and Supplements 11-12. 8vo. 1906.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, 1906, Nos. 6-7. 4to.
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Pennsylvania, University of—Bulletin, Sixth Series, Nos. 4-5. 8vo. 1906.
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Pharmaceutical Society of Great Britain—Journal for July-Oct. 1906. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LVIII. Part 1. 8vo. 1906.
Photographic Society, Royal—Journal, Vol. LXVI. Nos. 7-9. 8vo. 1906.
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Potter, C., Esq. (*the Translator*)—Dante, Divina Commedia in English Verse. 8vo. 1904.
Rennes, University—Travaux Scientifiques, Tome IV. 8vo 1905.
Riccó, Professor A.—Memorie della Società degli Spettroscopisti Italiani, Vol. XXXV. Disp. 6-9. 4to. 1906.
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Rockefeller Institute of Medical Research—Studies, Vol. V. 8vo. 1906.
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Röntgen Society—Journal, No. 9, July, 1906. 8vo.
Royal College of Surgeons—The Calendar, 1906. 8vo.
Royal Engineers' Institute—Journal, Vol. IV. Nos. 2-5, 1906. 8vo.
Royal Irish Academy—Proceedings, Vol. XXVI. Section A, No. 1; Section B, Nos. 4-5; Section C, Nos. 5-9. 8vo. 1906.
  Transactions, Vol. XXXIII. Section A, Part 1. 4to. 1906.
Royal Society of Edinburgh—Proceedings, Vol. XXVI. No. 4. 8vo. 1906.
Royal Society of London—Philosophical Transactions, A, Nos. 405-411; B, Nos. 247-250. 4to. 1906.
  Proceedings, Vol. LXXVIII. Series A, Nos. 521-3; Series B, Nos. 522-6. 8vo. 1906.
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Sanitary Institute, Royal—Journal, Vol. XXVII. Nos. 6-10. 8vo. 1906.
Scottish Society of Arts, Royal—Journal, Vol. XVII. No. 4. 8vo. 1906.
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Sennett, A. R., Esq., M.R.I. (*the Author*)—Garden Cities in Theory and Practice. 2 vols. 8vo. 1906.
Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXII. Nos. 7-11. 8vo. 1906.
  Transactions, Institution of Naval Architects, Vol. XLVIII. 4to. 1906.
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  Handlingar, Band XL. No. 5; Band XLI. Nos. 1-3 and 5. 4to. 1906.
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Turner, Professor H. H., D.Sc. F.R.S. (*the Author*)—Various Astronomical Papers. 8vo. 1905–6.
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United States Army, Surgeon-General's Office—Index Catalogue to Library, Second Series, Vol. XI. 4to. 1906.
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Diurnal Periods in the Lower Strata of the Atmosphere. By F. H. Bigelow. 4to. 1906.
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Bulletin of the Bureau of Standards, Vol. II. Nos. 1–2. 8vo. 1906.
United States Patent Office—Official Gazette, Vol. CXXII. Nos. 6–10; Vol. CXXIII.; Vol. CXXIV. Nos. 1–8. 8vo. 1906.
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Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1906, Heft 6–8. 4to.
Victoria Institute—Journal, Vol. XXXVIII. 8vo. 1906.
Vienna Imperial Geological Institute—Verhandlungen, 1906, Nos. 5–10. 8vo.
Washington Academy of Sciences—Proceedings, Vol. VIII. pp. 91–166. 8vo. 1906.
Washington Philosophical Society—Bulletin, Vol. XIV. pp. 339–450. 8vo. 1906.
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List of Members, 1906. 8vo.
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GENERAL MONTHLY MEETING,

Monday, December 3, 1906.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Cyril James Davenport, Esq.
George S. Hein, Esq.
Sir Henry Kimber, Bart., M.P.
Mrs. Robarts,
Major Percy Alexander MacMahon, D.Sc. F.R.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Dr. M. M. Bleekrode for his gift of a Portrait of the late Dr. R. Bleekrode, *Hon. Mem. R.I.*

The following Lecture Arrangements were announced :—

W. Duddell, Esq., M.I.E.E. Six Lectures (adapted to a Juvenile Auditory) on Signalling to a Distance, from Primitive Man to Radiotelegraphy. On Dec. 27 (*Thursday*), Dec. 29, 1906; Jan. 1, 3, 5, 8, 1907.

Professor Percy Gardner, Litt.D. F.S.A., Fellow of the British Academy. Two Lectures on The Sculpture of Aegina in Relation to Recent Discovery. On *Tuesdays*, Jan. 15, 22.

Professor A. C. Seward, M.A. F.R.S., Professor of Botany, University of Cambridge. Two Lectures on Survivals from the Past in the Plant World. On *Tuesdays*, Jan. 29, Feb. 5.

Professor William Stirling, M.D. LL.D. D.Sc., Fullerian Professor of Physiology, *R.I.*, Dean of the Medical Faculty and Professor of Physiology, Victoria University of Manchester. Six Lectures on The Visual Apparatus of Man and Animals. On *Tuesdays*, Feb. 12, 19, 26, March 5, 12, 19.

William Napier Shaw, Esq., M.A. LL.D. Sc.D. F.R.S. *M.R.I.*, Director of the Meteorological Office. Two Lectures on Recent Advances in the Exploration of the Atmosphere. On *Thursdays*, Jan. 17, 24.

Major Percy A. MacMahon, D.Sc. F.R.S. *M.R.I.*, Deputy Warden of the Standards. Two Lectures on Standards of Weights and Measures. On *Thursdays*, Jan. 31, Feb. 7.

Professor W. W. Watts, M.A. M.Sc. F.R.S., Professor of Geology, Royal College of Science. Two Lectures on (I.) The Building of Britain, (II.) Recent Light on Ancient Physiographies. On *Thursdays*, Feb. 14, 21.

Dr. W. Martin, Assistant-Director of the Royal Picture Gallery at the Hague, and "Privatdocent" on Art at the University of Leiden. Two Lectures on Old Dutch Painting and Painters. On *Thursdays*, Feb. 28, March 7.

Caleb Williams Saleeby, M.D. F.R.S.E. Two Lectures on Biology and Progress. On *Thursdays*, March 14, 21.

Sir Alexander C. Mackenzie, Mus.Doc. D.C.L. LL.D. *M.R.I.*, Principal of the Royal Academy of Music. Two Lectures on Latest Phases of Music. On *Saturdays*, Jan. 19, 26.

The Rev. William Barry, D.D. Two Lectures on Papal Deposing Power. On *Saturdays*, Feb. 2, 9.

Professor Joseph John Thomson, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy, *R.I.*, and Cavendish Professor of Experimental Physics, University of Cambridge. Six Lectures on Röntgen, Cathode, and Positive Rays. On *Saturdays*, Feb. 16, 23, March 2, 9, 16, 23.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Chemical Society—Proceedings, Vol. XXII. Nos. 315–316. 8vo. 1906.
Journal for November, 1906. 8vo.
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Church of England League—Gazette for November, 1906. 8vo.
de Kantzow, Admiral H. P., R.N. M.R.I.—Greek Coins and their Parent Cities. By J. Ward. 8vo. 1902.
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Athenæum for November, 1906. 4to.
Chemical News for November, 1906. 4to.
Chemist and Druggist for November, 1906. 8vo.
Dyer and Calico Printer for November, 1906. 4to.
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Engineer for November, 1906. fol.
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Engineering for November, 1906. fol.
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Motor Car Journal for November, 1906. 8vo.
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Nature for November, 1906. 4to.
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Palmer-Thomas, R., Esq., F.R.G.S. M.R.I. (the Author)—Notes on the Order of the Temple. 8vo. 1906.
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WEEKLY EVENING MEETING,

Friday, June 8, 1906.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Fullerian Professor of Chemistry *R.I.*

Studies on Charcoal and Liquid Air.

THE object of the lecture was to demonstrate experimentally a few novel applications of liquid air observed in the course of laboratory experiments. Some of them may be said to take the character of lecture illustrations, while others deal with an extension of the scientific uses of charcoal at low temperatures, a subject which was first discussed in a Friday Evening Discourse delivered by the Professor in the year 1896.

Electrical Stimulation at Low Temperatures.

Bodies cooled to the temperature of liquid air have the surface film of water, which is more or less deposited on them, frozen exceptionally hard, and therefore not in a good condition for the dissipation of any electric charge that may be given to them. The result is that a substance like glass cooled in liquid air is exceedingly easily electrified by friction, and retains its charge for a long time. This property of glass was shown in the following manner :—

A glass tube shaped like a two-pronged fork was arranged so that a magnified image of it could be projected on a screen. (See Fig. 1.) The prongs were cooled in liquid air, and one prong, A, while it was being removed from the liquid air, was gently flicked with flannel or silk to electrify it. On being exposed to the air the moisture in the atmosphere was deposited as ice on both prongs, but the character of the deposit was different on the unelectrified and the electrified prong, crystals of ice beginning to form on the latter, speedily causing it to assume the appearance of a miniature forest of growing and moving crystals shown on A in Fig. 1, while B shows simply a dead coating of ice. The electrified prong by induction attracts the moisture in the air, and as fresh moisture is drawn in repulsion between like electricities causes the new crystals to be deposited as far out as possible, thus presenting the appearance in the diagram. With good projection one can see some of the crystals on A repelled and shot across to B. A similar experiment

of another kind is the facility with which an electrified glass rod clears up ordinary opalescent liquid air by attaching the finely suspended impurities of ice crystals, solid carbonic acid, and organic matter to its surface when moved about in the liquid. The same thing may also be done by moving about a crystal of nitrate of uranium, which by cooling becomes highly electrified without the need of direct friction as when glass is used.

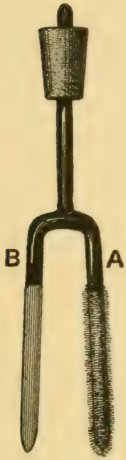
The Spheroidal State of Liquid Air on the Surface of Liquids.

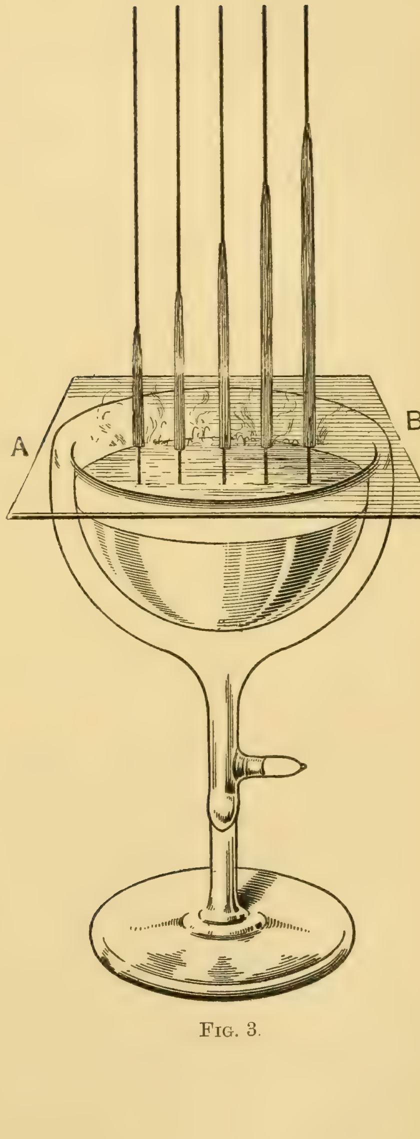
As volatility depends on vapour pressure, of two liquids that having the higher vapour pressure at any given temperature is necessarily the more volatile. The difference in the facility of condensation of the vapours produced by different liquids may easily be demonstrated by the use of liquid air.

A series of liquids with different boiling points, like : tetrachloride of carbon ; water ; caustic potash solution ; benzoic ether ; and strong sulphuric acid ; were selected, and each placed to the depth of about an inch in a number of shallow cylindrical cups, as shown in Fig. 2, the surfaces of which could in succession be projected on a screen by means of a horizontal lantern.

On allowing drops of liquid air to fall on the surfaces of these liquids, best by filtration through a small filter-paper funnel, they immediately assume the spheroidal state, and the projection through the liquid showed they were moving about with considerable rapidity, impinging on the sides of the vessel and getting deflected like an elastic body, while followed by clouds of condensed vapour, the relative amount of which may be taken as roughly proportional to the volatility of the liquid. With water the cloud was very dense, being less so in the case of the alkali solution, more so for the tetrachloride of carbon and least for sulphuric acid. On the sulphuric acid the drops moved about slowly owing to the high viscosity of the liquid, but with hardly any cloud, while on the surface of benzoic ester, a lively agitation accompanied by a light cloud was observed. In the case of the tetrachloride of carbon, the effect was most striking : dense clouds swayed to and fro over the surface, and the drops of liquid air shot to the walls of the vessel at all angles of incidence and rebounded, followed by tails of vapour, making them look like miniature comets. In order to secure a good view of the movements, it is necessary occasionally to blow the fume away from the upper part of the vessel by a current of dry air. Care must be taken to avoid the fall of too much liquid air in one place, which would cause a rapid local cooling of the liquid attended with solidification, which inevitably arrests all spheroidal motion. This may be prevented to a great extent by having the liquids in the vessels used for projection slightly heated before the experiments are begun.

FIG. 1.





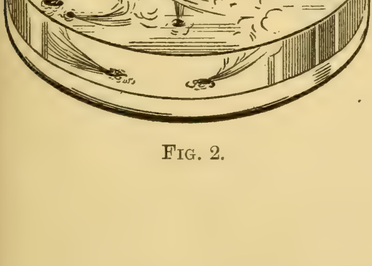


FIG. 2.

FIG. 3.

Thermal Conductivity.

Going to another subject, the well-known experiment of Ingenhousz on the relative conductivities of metals may be repeated at low temperatures by means of liquid air.

A vacuum-jacketed cup V (see Fig. 3) is filled with liquid air and covered with a sheet of mica (A B), having a set of small holes in it, through which a series of equal wires of different metals (copper, brass, bismuth, etc.) are fixed. After immersing their lower ends in the liquid air a coating of frozen moisture is deposited from the surrounding atmosphere, and is seen to collect round the lower parts of the wires above the mica screen, ultimately forming definite heights of an ice cap on each wire according to their conductivities. As in the Ingenhousz experiment, the relative conductivities are proportional to the squares of the heights to which the ice ultimately rises on the wires above the level of the mica sheet. In this manner it is possible to determine rapidly conductivities at low temperatures. The following table gives some rough measurements made in this way :—

	Bismuth.	Fusible Metal.	Lead.	German Silver.	Tin.	Steel.	Iron.	Brass.	Copper.
Height of ice in mm. . .	10	27	38	40	54	57	62	65	145
Conductivities —copper being taken as 1 .	·005	·035	·069	·076	·139	·155	·183	·201	1·000

A similar method might be employed to determine conductivities at temperatures lower than the melting point of ice ; for instance, the experiment might be made in a dry hydrogen atmosphere containing a small amount of sulphurous acid, continuously renewed, in which case the relative conductivities would be found from the melting point of sulphurous acid instead of from that of ice.

Gas Absorption by Charcoal at Low Temperatures.

Some of the more important facts regarding the absorption of gases by charcoal at low temperatures were detailed in the address given in 1905, entitled "New Low Temperature Phenomena," and to those a few additions were made due to further experience. Other porous materials are found to possess the power of absorption for gases at low temperatures to a less extent. Thus dry aluminium oxide shows a remarkable power of absorbing air just like charcoal, one gramme condensing at atmospheric pressure some 70 c.c., and the

same property is possessed by meerschaum and silica. The retentive power of alumina is much inferior, however, to that of charcoal, and consequently is of no use in forming high vacua. All charcoals possess the property of gas absorption at low temperatures. Light charcoal, such as is used in the manufucture of gunpowder, or the variety got from animal substances like blood, both act, but experiments made at the Royal Institution show that charcoal prepared by careful carbonisation from cocoa-nut is one of the most convenient varieties to use. While using the same process of carbonisation, the absorptive effect is enhanced by making it take place slowly and with gradually increasing temperature. With the samples made a year ago, it was possible to get an absorption of about 150 c.c. of air per gramme of charcoal at -185° C., but with care the amount can be raised to as much as 350 to 400 c.c. per gramme.

The amount absorbed at atmospheric pressure and at the temperature of liquid air can be quickly determined. A gramme of charcoal, previously heated to a red heat is placed in a glass bulb connected by an india-rubber tube and stop-cock to a graduated vessel containing air kept over strong sulphuric acid or a high boiling point oil, so that on cooling the charcoal and opening the stop-cock the absorption is measured. This absorptive power can be shown by means of the balance and the electro-magnet. From one arm of a balance was suspended a copper-wire gauze sphere containing charcoal which was carefully counterpoised. A flask partly filled with liquid air was now brought up below it, so that the vapour rising from the liquid air surrounded the charcoal. The absorption soon became visible through the beam of the balance descending as the charcoal became charged, and by adding weights the amount of gas absorbed per unit of time could be determined. If a rod of charcoal was hung up by a thread between the poles of an electro-magnet and sufficient torsion applied, so that its length was at right angles to the line joining the poles, when the magnetic field was produced, on bringing a vessel of liquid air close below it after a short time on turning on the electric current to "make" the magnet, the rod now set itself along the line joining the poles, proving that it had become magnetic. This action is dependent on the well-known magnetic property of oxygen, one of the constituents of the air, which surrounded the charcoal rod and which had condensed within its pores. On taking away the cool vapour of the liquid air the temperature soon rose and the behaviour of the rod under magnetic action showed that it had lost the magnetic property.

Effect of Increased Pressure of Gas on the Absorption of Charcoal at Low Temperatures.

The absorption of hydrogen by charcoal at the temperature of liquid air and under atmospheric pressure is very considerable, and

the question arises how much more could be got in by the use of higher pressure. The amounts absorbed by 6·7 grammes of charcoal, at the temperature $-185°$ C. for various pressures are given in the following table :—

Pressure in Atmospheres.	Volume in Cubic Centimetres.
1	620
5	925
10	1,050
15	1,000
20	975
25	925

The amount absorbed is seen to increase with the pressure to 10 atmospheres, after which the absorption in the pores of the charcoal seems to be independent of the pressure. At the temperature of liquid air this sample of charcoal would not absorb more than one litre of hydrogen, even when the pressure was raised from 10 to 25 atmospheres or the absorption had come to a limit. Increased absorption, which does not reach more than twice that at the ordinary atmospheric pressure, is the sole result of the use of higher pressures.

Vacua produced by Charcoal under different circumstances.

The absorptive power of charcoal for air is very remarkable. On stopping the passage of a stream of air into a charcoal bulb immersed in liquid air, to which a small manometer was attached, hardly any gas pressure was shown. This was further exemplified by the action of a Röntgen tube, partially exhausted, connected to a charcoal bulb. On immersing the bulb in liquid air the characteristic phosphorescent glow of the Röntgen tube was soon reached, but disappeared when the liquid air was removed. When hydrogen was used instead of liquid air, the pressure was so much reduced that the glow was entirely stopped. A Crookes's radiometer, filled with hydrogen at atmospheric pressure, had a charcoal bulb attached. On immersing the charcoal bulb in liquid air no rotation took place, even when the beam of an electric lamp was thrown upon it ; but when liquid hydrogen replaced the liquid air the motion became exceedingly rapid. This shows that high vacua can be attained by the use of charcoal in an atmosphere of pure hydrogen, provided it is cooled in a liquid hydrogen bath instead of liquid air. A similar radiometer, filled with helium at atmospheric pressure, remains inactive even when liquid hydrogen is the cooling agent, thus proving that the absorption of the helium by charcoal at 20° abs. is relatively inefficient for the production of vacua as compared with hydrogen. This is only another mode of proving the much greater volatility of helium over hydrogen.

Laws of Absorption at Low Temperatures.

The general laws of the dependence of the absorptive power of charcoal upon the pressure, temperature, and volume of the gas absorbed are apparently complicated, but in average circumstances they can be stated approximately. For a supply of gas at constant pressure the volume and temperature are related by a curve of hyperbolic form. There must clearly be a lower limit of temperature for absorption, for the gas will eventually liquefy; on the other hand, however small the absorption may be, as the temperature rises it can never entirely vanish. If the temperature be kept constant we should expect an increased absorption with increasing pressure up to a certain limit. Experiment indicates that this is the case, and we have again a curve of hyperbolic form connecting pressure and volume, but differing from the previous case in having the convexity turned the opposite way. When the pressure is small the absorption will be a minimum, but any slight increase of pressure will be associated with the molecular attraction of the charcoal, and the absorption may be expected to increase much more rapidly in proportion to the pressure. Finally, when the volume absorbed is kept constant the relation between the pressure and the temperature is of a logarithmic form, like those of a saturated vapour or a dissociating body.

For the absorption of hydrogen in charcoal at the temperature of liquid air the expression $\log p = a + b\text{V} - c\text{V}^2$ holds, where p is pressure, V the volume of gas occluded, and a, b, and c constants. For small concentrations of gas the pressure grows in a linear relation to the volume.

Hypothetical Densities of Gases Occluded in Charcoal.

Mitscherlich's measurements made on the charcoals used by him showed that in a piece of charcoal the space occupied by charcoal-substance is to the pore-space nearly in the ratio of 13 to 20. The Professor's observations on the real and apparent densities of cocoanut charcoal lead to the conclusion that in 100 grammes the pore-space may be taken on the average as about 15 c.c. This enables us to compare the average hypothetical density of any absorbed gas with the density of the same gas when liquefied. The following table shows some such comparisons, and it may be noted that the densities of the absorbed gases at their boiling-points are equal to or a little greater than the corresponding liquefied gases at the same temperatures, so far as they are known. Whether the density of the monadic gases will turn out as satisfactory is a matter for future inquiry.

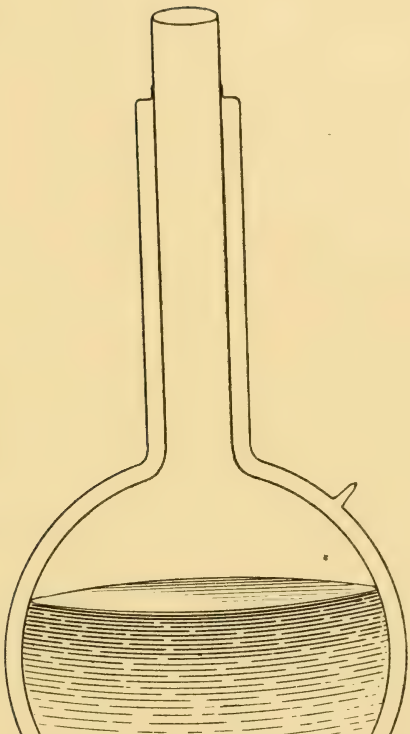


Fig. 4.

Gas.	Temperature of Absorption. Cent.	Abs.	Density of Gas Occluded in Charcoal.	Density of Gas Liquefied.
Carbonic acid . . .	+ 15°	288°	0·7	0·8
Oxygen	– 183°	90°	1·33	1·12
Nitrogen	– 193°	80°	1·00	0·84
Electrolytic gas . . .	..	..	0·58	?
Hydrogen	– 193°	80°	0·06	0·07
,, ...	– 210°	63°	0·08	..
,, ...	– 252°	21°	0·11	..
Helium.	– 258°	15°	0·17	?

Metallic Vacuum Vessels.

With the aid of charcoal, it is easy to make useful vacuum vessels of metal instead of glass. The metallic vessels resemble the ordinary glass silvered vessels and are of from 2 to 10 litres capacity (see Fig. 4). The envelopes may be made of brass, copper, nickel, or tinned iron, with necks made of a bad conducting alloy. The vacuum between the walls of these vessels was maintained by enclosing some charcoal in a small globular space, A, constituting part of the inner vessel that is filled with liquid air. The necks may be covered with silvered glass vacuum cylinders which act as stoppers, and at the same time utilise the cold of the slowly evaporating liquid. The efficiency of the best metallic flasks is equal to that of the chemically silvered glass vacuum vessels, now generally used in low temperature investigation. Vessels of this type will be of use in industrial cryogenic operations and for the storage and safe transit of liquid air.

Diffusion of Gases into Charcoal at Low Temperatures.

Although, as regards general absorption, charcoal under corresponding conditions behaves in a similar manner to all gases, nevertheless it possesses a certain selective power, when in presence of a mixture of gases. A quantity of charcoal which has been heated, exhausted, and then saturated with ordinary air at -185° C., must have gases occluded in its pores of the average composition of the air. Let, however, a stream of air at the same low temperature pass slowly and continuously over it for some hours, and it will be found that at first the issuing gas is almost pure nitrogen, while the occluded gas reaches a new and definite composition. If now the gas absorbed by the

charcoal be expelled by heating it to the ordinary temperature, it will contain on the average some 60 per cent. of oxygen. Fig. 5 represents roughly the molecular composition of the air initially present in the pores of the charcoal, namely, about four molecules of nitrogen to one of oxygen. The final average molecular composition of the absorbed gas after the passing of the slow current of air, cooled to -185° C., over the charcoal is similarly represented in Fig. 6, where it can be seen that diffusion, or fractional distillation at constant temperature, has gone on until about two of the molecules of nitrogen have been replaced by two of oxygen. If the charcoal, in equilibrium with air gases in the condition specified in Fig. 6, has a current of hydrogen at the temperature of liquid air passed over it, the hydrogen will diffuse in, until about one molecule in five is present, as shown in Fig. 7. Again, if charcoal be saturated with oxygen to begin with, and hydrogen be passed over it when cooled to -185° C., the hydrogen will diffuse in, until it constitutes one-third of the gas present in the pores (Figs. 8 and 9). Similarly, if we employ nitrogen in place of oxygen, the hydrogen will displace about two-thirds of the nitrogen (Figs. 10 and 11). On the other hand, if the charcoal be initially saturated with hydrogen at -185° C. (Fig. 12), and air passed over it at that temperature, the whole of the hydrogen is practically displaced, and the gas remaining in the charcoal is represented by the composition shown in Fig. 13, which is the same as in Fig. 6.

If, instead of analysing the whole of the occluded gas that is given off, samples are taken from the charcoal between definite temperatures, from the boiling point of air up to the ordinary temperature, then a regular fractionation of the occluded gases takes place, resembling that of an ordinary mixed liquid, and the problem is an entirely different one. Further, even in the simpler mode of treatment as described above, the relative proportions of the absorbed gases depends at any time after the experiment has started upon the relation between the current of gas used at the constant temperature of -185° C., and the nature and condition of the charcoal. The simplification of the problem is due to making the time very long.

Chemical Interactions at High Vacua.

If a piece of pure sulphur (S) is put in one end and some mercury (Hg) in the other end of a U-tube (Fig. 14), both substances being kept at the temperature of liquid air during the time required to reach a high vacuum by the mercurial pump or by charcoal exhaustion, and the whole sealed off and left to stand at the ordinary temperature ; then in a few hours, it will be noted that the surface of the mercury which was at first a brilliant reflecting one gets tarnished from the formation of a film of sulphide of mercury. The mercury vapour pressure being much greater than that of sulphur, it would be expected that any formation of sulphide of mercury taking place

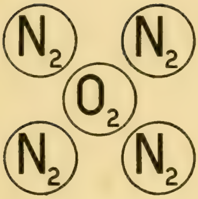


FIG. 5.

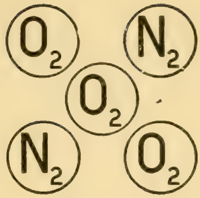


FIG. 6.

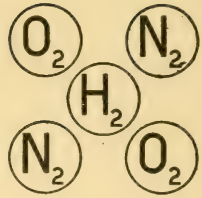


FIG. 7.

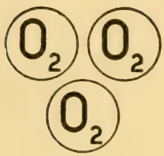


FIG. 8.

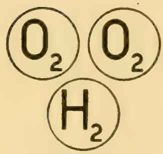


FIG. 9.

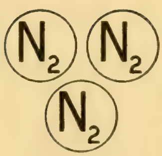


FIG. 10.

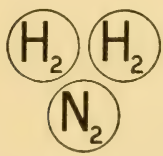


FIG. 11.

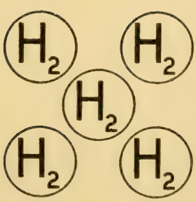


FIG. 12.

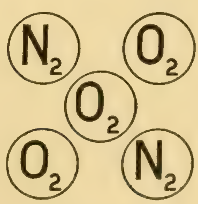


FIG. 13.

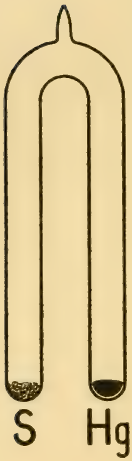


Fig. 14.

Fig. 15.

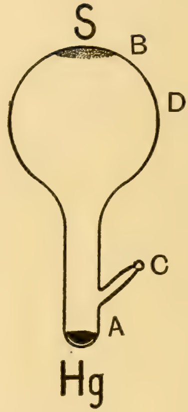


Fig. 17.

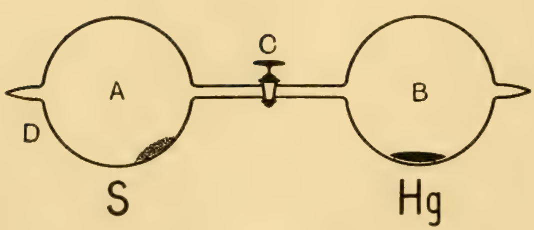


Fig. 16.

would be seen on the sulphur side of the U-tube. But the contrary is the case, as the sulphide is seen to form at first on the surface of the mercury (Hg). The explanation that suggests itself is that the ordinary molecule of sulphur containing 6 to 8 atoms, dissociates and throws off single or double atoms which make their way rapidly through the mercury vapour without wholly combining with it, until they reach the surface of the liquid mercury, when the conditions for chemical interaction are more favourable. If the tube be constricted as at M (Fig. 15), the activity of the sulphur atoms on the mercury surface is stopped, as they encounter the mercury atoms under the most favourable circumstances for combination in the narrow tube at M, and there form the sulphide which is quite visible after long keeping.

It is interesting to note the excessively small quantities of the respective elements that are interacting in this case. The pressure of mercury vapour at 15° C. is about $1\frac{1}{2}$ millionths of an atmosphere, whereas that of sulphur is of the order of one thousandth part of the millionth of an atmosphere.

The same phenomenon was exhibited in an even more striking manner by the following experiment. Two equal bulbs A, B (Fig. 16) were united by a tube containing a finely-ground stop-cock C. A small piece of pure sulphur was put into A, and a small quantity of mercury into B, and the whole exhausted through the side-tube D by either a mercury pump or by the use of charcoal cooled in liquid air. While the exhaustion was going on the sulphur in A was fused, and finally the tube D was sealed off. On shutting the stop-cock C, and allowing the apparatus to stand for some time, the application of a liquid air sponge to any part of the bulb B at once caused the deposition of a mercury mirror, which disappeared on allowing the cooled spot to regain the ordinary temperature. If the same cooling is applied to any part of the bulb A, no deposit takes place ; but, if the sulphur has been recently fused so as to get it deposited on a portion of the glass surface in what is called the *utricular* state, then, in a short time a fine film of highly refracting sulphur is seen to have distilled to the cooled part, just like what takes place with the mercury in the other bulb. Now let the stop-cock C be opened for a few seconds and shut again, and the local liquid air cooling on parts of both A and B repeated. On the cooled part of B nothing but mercury will be deposited, as before ; but on the cooled part of A a brilliant metallic deposit will be noticed, which is, however, not wholly mercury, for, on heating it with the finger, it is only partially volatilised, the portion which remains being sulphide of mercury. On repeating the operation on another part of A in about a quarter of an hour, all metallic deposition has disappeared. Thus, mercury vapour requires time to react with sulphur, but ultimately all the mercury vapour is removed by the sulphur. The apparatus enables the experiment to be repeated at any time.

Shortly after the discovery of oxygen by Priestley some Dutch physicists began the study of the gases absorbed and given off by living plants; and for this purpose the plants were covered by bell-jars whose lower edge was laid in a channel of mercury. But a difficulty arose, as it was found that the plants in these circumstances soon became sickly and died. Then the remarkable discovery was made that if sulphur was sprinkled over the leaves and the surface of the vessel the deleterious effect of the mercury was overcome and the plants became healthy. Boussingault repeated and confirmed these results in 1868, and made many experiments to elucidate the remedial action of sulphur. He explained this action by pointing out that the poisoning of the plants arose from the presence of mercury vapour, and that as one volume of sulphur vapour was able to combine with six volumes of mercury vapour, the sulphur although less volatile was able effectually to neutralise the deleterious effects of the mercury vapour.

Now, the ratio of the pressure of mercury vapour to that of sulphur at the temperature of 115° C., when both bodies are in the liquid condition, is as 10 to 1. Seeing the sulphur molecule is S_6 to S_8, one volume of sulphur vapour is sufficient to neutralise or combine with 6 to 8 volumes of mercury vapour, so that an excess of mercury pressure of from 6 to 8 times that of the sulphur vapour can always be removed by combination with the sulphur. If, on the other hand, we compare the relative pressures of mercury at 100° C. to that of solid prismatic sulphur at the same temperature, this ratio is 37 to 1, so that the removal of the mercury vapour at the ordinary temperature, when the pressure ratio is still higher, cannot be fully explained. The effect of air and water vapour complicates the action in the case of plants as the molecules of sulphur and mercury have not the free play they have when no inert gas is present. It would seem that in high vacua the S_2 molecule of sulphur may be considered as produced by molecular dissociation, and that moving with great rapidity relatively to the mercury gas molecules soon gets at the surface of the liquid mercury and coats it with a thin layer of mercury sulphide, which acts as a trap, stopping the exit of further mercury molecules. The effect of such a coating in preventing the escape of mercury vapour may be shown by the use of an inverted boiling point flask like Fig. 17 containing a layer of fused sulphur at B, and mercury at A. Before and during very complete exhaustion, the mercury at A is kept in liquid air and finally the flask is sealed off at C. On standing at the ordinary temperature, the surface of the mercury gets acted upon by the sulphur, and if the mercury is not shaken so as to crack the film of mercury sulphide, a little sponge of liquid air placed upon the surface at D shows no metallic deposit; but the moment the mercury is slightly shaken to break the surface film of the liquid mercury, metallic deposit is instantly formed by the local cooling. This deposit is not all mercury, but partly sulphide, because on taking away the sponge

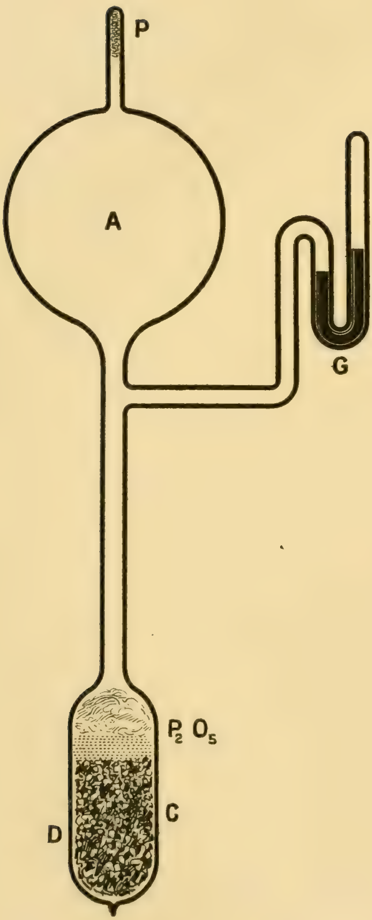


FIG. 18.

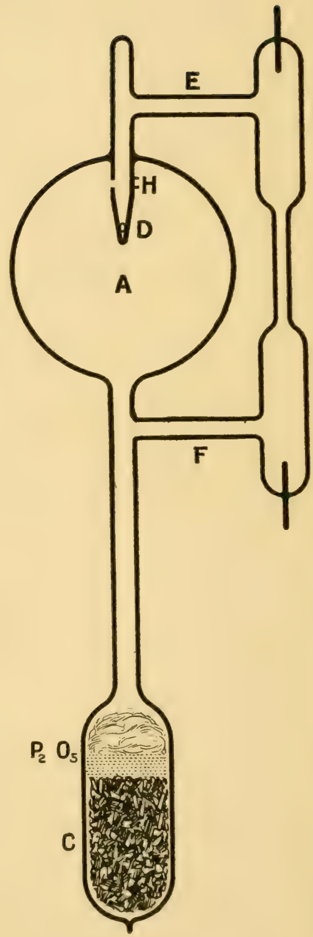


FIG. 19.

and heating the cooled spot by contact with the hand, a permanent brown part remains, whereas, if it had been all mercury it would have completely volatilised. On standing for a little time the liquid air cooling causes no metallic mirror, as all the mercury vapour has been removed by combination with the sulphur, and the sulphide film reforms on the mercury, when the experiment may be repeated.

Phosphoric Action at High Vacua.

In the Friday Evening address of 1905 an experiment was shown illustrative of the well-known fact that the phosphorous glow caused by oxidation does not take place in dry oxygen at atmospheric pressure, and that it was only when the pressure was very considerably reduced that such action took place. This may be shown more clearly in the following way. A globe (Fig. 18), A, has a small capillary-side tube, P, attached, containing a little phosphorus, that has been melted at its end, and is at the same time connected by another tube to a reservoir, D, containing charcoal covered with a layer of phosphoric anhydride to absorb all traces of moisture. To the side of this tube a small mercury gauge, G, is attached. After the whole vessel is thoroughly exhausted it is filled up to atmospheric pressure with pure oxygen gas and the whole sealed off. On immersing the charcoal bulb in liquid air the pressure falls rapidly to a fraction of a millimetre pressure, and soon the globe A becomes filled with a phosphorescent glow, showing that chemical action is taking place. As the absorption of the oxygen by the charcoal continues the phosphorescence becomes less, and at last disappears, nothing finally filling the bulb but the vapour of phosphorus as it distils from P into the liquid air condenser. Meanwhile the gauge G shows that the pressure at which the phosphorescence begins must be only a fraction of a millimetre, and that it continues at a pressure below what could be measured on the gauge. When the charcoal is taken out of the liquid air, and the temperature allowed to rise, the course of the experiment is inverted, the oxygen occluded in the charcoal being given off, and as it meets the vapour of phosphorus in the bulb A, suddenly combines with it in a few oscillating flashes, and soon all is dark again. The experiment may be repeated after weeks of keeping with the same uniform results. The same thing is also very beautifully shown by the arrangement in Fig. 19. Here the gauge is replaced by a sparking tube B, having free access to the globe A by the tubes E and F, the former having communication by means of the small hole H; D is the phosphorus. The high vacuum at which the glow takes place in the bulb is shown by the character of the discharge taking place in B. A curious phenomenon is observed on stopping the electric discharge; now the glow travels from the bulb along E and F and meets in the middle of B. It looks as if the electric

discharge expelled all the phosphorous vapour from the discharge tube, and that the moving phosphorescent stream is due to the vapour of phosphorus coming back again. While the electric discharge is passing in B, the spectroscope reveals only the oxygen bands.

Separation of the volatile gases Neon, Hydrogen, Helium.

Attention has already been called to the selective nature of the absorption when a mixture of gases is presented to cooled charcoal, and it has been noted that the gas obtained from the charcoal after absorption of air has the quantity of oxygen increased from 21 to 50 or 60 per cent. The general law of this absorption is that the lower the boiling point, in other words, the more volatile or less condensable the gas, the less is the absorption. Thus for air, nitrogen with the lower boiling point is not absorbed to so great a degree as oxygen, which has the higher boiling point. This is beautifully shown in the following experiment. A number of spectroscopic tubes connected in series with a large charcoal U-tube, are highly exhausted by cooled charcoal so that the discharge will hardly pass. The charcoal tube is now placed in liquid air, and a slow current of air is allowed to enter the system. The less volatile gases, oxygen, nitrogen, argon, are absorbed by the charcoal, while the more volatile gases, helium, neon, and hydrogen, are allowed to pass. In a short time, when the pressure of the latter gases has risen sufficiently, the first tube begins to glow with the well-known rich orange hue of neon. As the air-absorption progresses, the characteristic discharge of neon and helium gradually extends to the other tubes, the slowness of the current displaying vividly the march of the neon and helium glow. In this connection the sensitivity of the neon tube to induced electric oscillations from a coil of wire placed at right angles to the tube, was shown. This property of neon Professor Fleming makes use of in his ingenious wave-detector for wireless telegraphy—the cymometer.

The most volatile gases being neon, hydrogen and helium, when a charcoal bulb attached to a sparking tube filled with helium, was placed in liquid air, the nature of the luminosity remained unchanged, thus indicating that hardly any absorption had taken place at the temperature of liquid air. But on replacing the liquid air by liquid hydrogen under exhaustion, the pressure was so much reduced that the discharge no longer passed. This observation corroborates the statement already made that the boiling point of hydrogen is a temperature for helium which corresponds with the boiling point of air for hydrogen, and leads us to infer that the boiling point of helium is about 5° to 6° absolute.

Measurements made in 1902, by what was called the float-method of separation, showed that the atmosphere contained as a minimum one part in 70,000 of neon, and one part in 362,000 of helium.

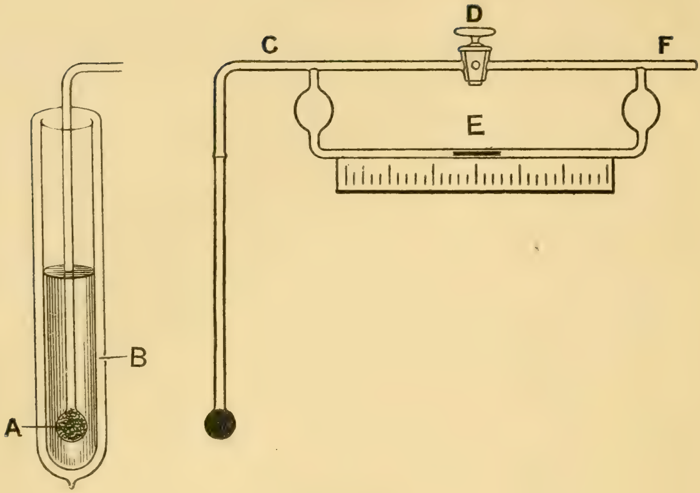


FIG. 20.

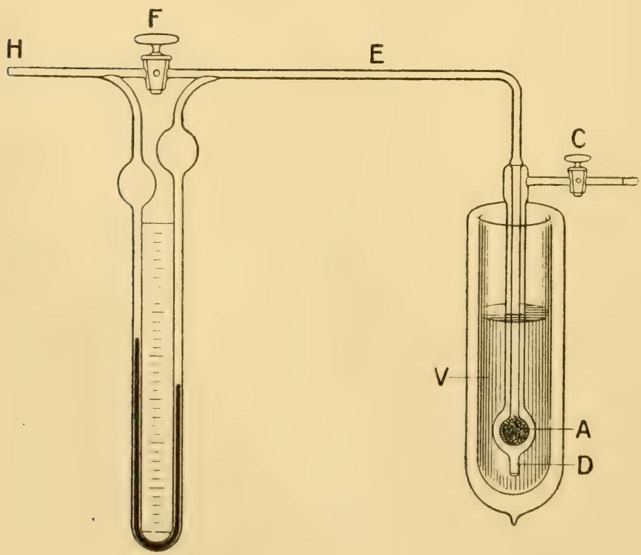


FIG. 21.

Recent determinations by the charcoal method, made by Sir W. Ramsay, give these proportions as one part in 80,790 of neon, and one part in 245,300 of helium or one part in 61,000 of both these gases together. The absorption of 30 litres of air by 500 grammes of charcoal gives a residuary pressure amounting to between 1·4 and 1·5 mm. of mercury pressure; thus representing a partial pressure of unabsorbed gases, helium, neon and hydrogen amounting to $\frac{1}{530000}$ of the volume of the original air used in the experiment.

Separation of the less volatile gases, Krypton and Xenon, by Charcoal.

The method of separating carbonic acid from air shown in the Friday Evening Address for 1905 is equally applicable to the separation of krypton and xenon. A current of air passes through a series of tubes immersed in liquid air for the purpose of purification, the final tube containing cotton wool in order to retain any dust of the solid condensed impurities. This pure air is passed through a tube containing about 100 grammes of charcoal, the current being maintained for at least 24 hours. The charcoal tube is now removed and placed in solid carbonic acid, any gas coming off being allowed to escape. The gas remaining in the charcoal at $-78°$ C. is now got out by heating and exhaustion, and all the carbon compounds and oxygen removed from it. The remaining gas, nitrogen containing krypton and xenon, is separated into its constituents by condensation and fractionation. Instead of passing a current of air over charcoal at $-183°$ C., a few hundred grammes of charcoal may be covered with old liquid air, and the latter allowed to evaporate in a silver vacuum vessel, the gases remaining in the charcoal being separated as above. In this way krypton and xenon are readily separated from air and spectrum tubes easily prepared.

Charcoal Thermometer and Calorimeter.

Charcoal saturated with gases at low temperatures may be used as a thermoscope or calorimeter. Attention has been called to the law of absorption, that when the volume is constant, the pressure falls very rapidly with the temperature; in fact, the curve connecting them, when pressure is plotted as ordinate to temperature as abscissa, drops almost vertically as the temperature diminishes to the absolute zero, and a similar relation holds for the volume absorption at constant pressure. Hence, a small change of temperature is accompanied by a great change of pressure or volume absorption as the case may be. This was exemplified in the following experiment. A charcoal bulb A (Fig. 20) previously saturated with air at $-185°$ C. or hydrogen at $-253°$ C. at any required pressure, the most convenient being

atmospheric pressure, was immersed in a bath B of the corresponding liquefied gas contained in a vacuum-jacketed vessel; and a tube C was led from it to a Leslie thermometer containing a sulphuric acid cylinder used as a registration instrument for volume. The gauge consisted of a U-tube, with bulbs on each side, to prevent any sudden sucking back of the drop of sulphuric acid E, renders visible the slightest alteration of the volume of gas occluded in the charcoal. The upper ends of the U-tube were connected by a by-pass containing a stop-cock D. On shutting this stop-cock after the complete saturation of the charcoal with air has been effected, equilibrium of pressure between C and F could be obtained, while the sulphuric acid drop E occupied a convenient position in the gauge. On the approach of a candle-flame to the charcoal bulb, the gauge immediately showed a considerable expansion, notwithstanding that the radiant heat had to traverse a considerable quantity of liquefied gas, and several thicknesses of glass tubing. In the same manner a beam of light thrown on the charcoal bulb when charged with hydrogen instead of air immediately affected the gauge in a striking way.

The apparatus can be rendered still more sensitive by the following device (Fig. 21). The charcoal bulb A, saturated with air at $-185°$ C., is surrounded with another larger bulb giving an annular space that can be filled with liquid air by opening the stop-cock C, the orifice D being open while immersed in a large vacuum-jacketed reservoir, V, of liquid air. The tube E leading from A passes to a sulphuric acid manometer, with by-pass and stop-cock, F, as before, thence to the open end H. The apparatus works thus: Before the radiant beam is turned on A, the annular space between the bulbs is cleared of liquid by blowing a little air through the tube C, and shutting the stop-cock, thus forcing the liquid air out by the aperture D. The charcoal bulb is now surrounded by an atmosphere of liquid air vapour, or hydrogen, as the case may be, maintained at the boiling point by the liquid in V. In these circumstances the maximum effect of any heat given to the charcoal bulb is obtained. The effects are magnified two or three times compared with those given by the charcoal bulb left in direct contact with the liquid.

The special value of this instrument for thermometric measurements is that it becomes more sensitive as the temperature falls, thus placing in our hands a most efficient thermometer for the lowest temperatures which can possibly be reached. Thus, when the charcoal is saturated with hydrogen gas at the boiling point of hydrogen, the instrument has its maximum delicacy under ordinary circumstances; but provided helium was absorbed in charcoal at and below the temperature of solid hydrogen, then we can predict that the instrument would be still more sensitive to radiant energy.

If, further, the charcoal occupies an annular space surrounding another smaller bulb, into the interior of which any small heated

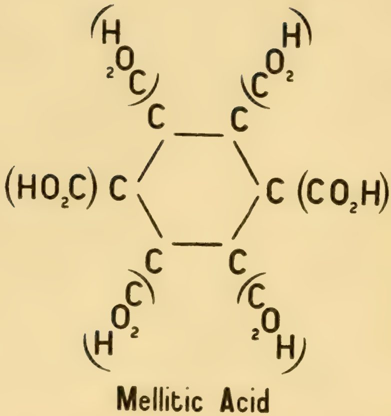


FIG. 22.—Graphical Formula of Mellitic Acid.

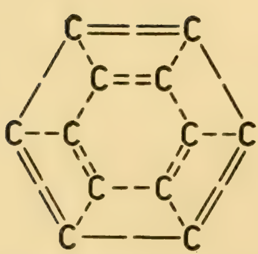


FIG. 23.—Graphical Formula of Charcoal Molecule.

object is inserted, the apparatus becomes a very sensitive calorimeter.

Much investigation is still necessary to clear up the nature of the various charcoals and their properties. One chemical character they possess, and that is that, by powerful oxidising agents like concentrated nitric acid or permanganate of potash, they seem all capable of giving a greater or less yield of an acid called Mellitic Acid, the graphical formula of which is represented in (Fig. 22), which occurs in nature as the aluminium salt, commonly called Honeystone. This acid, we know, is a derivative of benzol, and its production from charcoal suggests that the latter has a structure of double benzine rings, each containing twelve atoms of carbon such as is shown in (Fig. 23). This gives a fundamental molecule containing a large number of latent valences available for chemical or physical combination, and this structure may have something to do with the general absorptive power of charcoal.

The Professor thanked his assistants, Mr. R. N. Lennox and Mr. J. W. Heath, for valuable aid in the conduct of these experiments.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 18, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

SIR ANDREW NOBLE, Bart., K.C.B. D.Sc. F.R.S. *M.R.I.*

Fifty Years of Explosives.

I PROPOSE in my lecture to give a short account of the changes which have taken place in propellants during the last fifty years: 1856 being the year when, during my voyage to South Africa, my attention was first called to the curious discrepancies between the views of the distinguished men who had written upon the motive force and pressure of gunpowder; and ever after that year I determined to do my best to solve the difficulties which surrounded the question.

Fortune has been kind to me in enabling me to carry out my wish, for few men, either when in the Service or since I left it, have been placed in a better position for carrying out the difficult and somewhat dangerous investigations I had at heart.

During these fifty years, besides various experiments as to explosives other than propellants, and modifications to propellants, I have completely examined no fewer than eighteen different propellants, determining, for most of them, the changes in transformation which take place when the explosive is fired under very various pressures.

The composition of these various explosives was approximately as follows:—

OLDER EXPLOSIVES.

—	Powder A	Powder B	Powder C	Powder D	Cocoa	Pebble W. A.	R. L. G. W. A.
KNO_3 .	·8130	·7783	·6374	·7724	·7883	·7476	·7456
S . .	·0018	·0028	·1469	·0615	·0204	·1007	·1009
C . .	·1671	·1972	·2018	·1543	·1780	·1422	·1429
H_2O . .	·0181	·0217	·0139	·0118	·0133	·0095	·0106

—	F. G. W. A.	Spanish	C. & H. No. 6	Mining	Amide
KNO_3 .	·7391	·7559	·7468	·6192	·40 KNO_3
S . .	·1002	·1242	·1037	·1506	·38 $(NH_4)NO_3$
C . .	·1459	·1134	·1378	·2141	·22 C
H_2O . .	·0148	·0065	·0117	·0161	— —

MODERN EXPLOSIVES.

—	Cordite Mark I. I	M. D. Cordite II	Norwegian 165 III	Norwegian 167 IV	Italian Ballistite V	Nitrocellulose VI
Nitroglycerine .	58·0	30·0	36·0	40·0	47·1	—
Nitrocellulose .	37·0	65·0	52·0	50·0	52·9	100·0 per cent., of which 14·5 per cent. insoluble
Nitronaphthaline	—	—	6·0	5·0	—	—
Secret ingredient	—	—	6·0	5·0	—	—
Mineral jelly .	5·0	5·0	—	—	—	—

I have added to the list of high explosives with which I have experimented the French B.N. (blanche nouvelle), which consisted of nitrocellulose partly gelatinised, associated with tannin and potassium and barium nitrates ; also Lyddite, which, on account of the violence with which under certain circumstances it detonates, cannot be used as a propellant, but is still interesting as a most useful explosive for shell when ignited by means of a suitable detonator.

I shall return presently to the consideration of some interesting points connected with the above explosives, but I desire first to give you some information about the powders in use fifty years ago, and the various views which were then held as regards the pressures which they were capable of exerting and the potential energies which they possessed.

I do not know any physical fact upon which for many years there was so wide a difference of opinion among eminent men as the pressure developed by the explosion of gunpowder. Robins, the father of gunnery, put the pressure developed as low as 1000 atmospheres (about 6·6 tons per square inch), while Hutton put it at twice that value—viz. 2000 atmospheres. Rumford's celebrated experiments induced that philosopher to put the pressure of gunpowder as high as 101,000 atmospheres (662 tons per square inch), and, in order to account for the comparatively small velocities realised, supposed that the powder in both guns and rifles burned very slowly, and that thus the initial tension was never realised. It is sufficiently curious that the weight of Robins's remark, that the powder he employed must all be exploded before the bullet was much removed from its seat, was not recognised, and he pointed out that if it were not so, a much greater energy would be realised when the weight of the shot was doubled, trebled, etc. ; but his experiments showed that in all these cases the work done was nearly the same. Later on I shall return to this point.

In my own day similar discrepancies existed. In text-books at the Royal Military Academy, Woolwich, so late as 1870, the tension of fired gunpowder was put at 2200 atmospheres, while Piobert con-

sidered that Rumford's first series of experiments was tolerably correct, and he fixed the tension when fired in its own space at 23,000 atmospheres (about 150 tons per square inch). Cavalli in 1867 arrived at nearly the same conclusion, making the pressure 24,000 atmospheres.

I think I may say the question was set at rest by the experiments made by myself, and described in the paper I wrote, assisted by my friend and colleague, Sir F. Abel, in which I succeeded in determining the tension of the exploded gases at various densities, and in altogether retaining the gaseous products of combustion, even of charges which filled entirely the chambers of the closed explosion vessels. I also was able to discharge at pleasure, and measure the gaseous products of combustion. The results of my experiments gave for the density of unity a pressure of about 42 tons per square inch, or, say, 6500 atmospheres.

My attention was very early called to the great variation in energy developed by Service powders of the same make and recent manufacture; and in 1860, being then an Associate Member of the Ordnance Select Committee, I pointed out that, in the experiments which I was then carrying out for that body, the variation in energy developed by new powders of different makes occasionally amounted to 25 per cent. of the total energy developed.

Powders on Service which were subjected to climatic influences would, of course, show much greater variations, but I must add that much of this variation was due to the method of proof then in force —the Eprouvette Mortar, than which nothing can be conceived better adapted to pass into the Service powder unsuitable for the guns of that time.

It was no doubt in a very great measure due to the unsuitable nature of the powder then in use, that many of the serious failures of the early rifled guns were due. With these powders, chamber pressures were endured which would not be permitted in the much more powerful and stronger guns now in use.

The velocity in those days given to spherical shot by the large smooth-bore guns was generally between 1600 and 1700 f.s., but when rifled guns were introduced the velocities were at first only between 1200 and 1300 f.s., gradually increased to about 1400 f.s.

The first improvement in the old powders was due to Major Rodman, U.S.A., who not only appreciated the importance of the size and density of the grains in diminishing excessive pressure, but invented a most ingenious instrument for determining the pressure of the exploded gases. With breech-loading guns this instrument gave reliable results, but as the English guns at that date were muzzle-loading, it was necessary to place the instrument on the outside of the gun, where, owing to the run of the gas, the results were not reliable. and the more convenient crusher gauge has since been almost universally used.

Before the adoption of the Pebble, P_2, and Prismatic powders, which were due to the labours of the first Explosives Committee, the chamber pressures were very high, and occasionally ran exceptionally high when a rapidly lighting or brisante powder was employed ; but it must be remembered that these exceptional pressures did not exist at the same instant throughout the chamber, but passed in waves backwards and forwards from the end of the chamber to the base of the projectile.

A curious and interesting instance of this action occurred in one of two consecutive rounds fired from a 10-inch gun during experiments made by the first Explosives Committee. In the first three feet of the chamber and bore, five crusher gauges were placed at various points, the gauge at the base of the bore being in the axis of the chamber.

The first round, which was nearly normal, gave the pressures 28·0, 29·8, 30·0, 29·8, and 19·8 tons per square inch. The second, with which wave action was set up, gave 63·4, 41·6, 37·0, 41·9, and 25·8 tons per square inch. Yet with both rounds the muzzle velocity was the same, proving that the mean pressure in the bore had been identical.

But I must not detain you on these experiments, interesting as they are, and I only point out to you that the labours of the first Explosives Committee resulted in an increase of velocity to the same projectile of over 200 f.s., viz. from over 1400 f.s. to over 1600 f.s., equivalent to an increase of energy of about 33 per cent., while the maximum pressure was reduced by about the same percentage, a matter of very great importance in the case of breech-loading guns.

I now pass to give you the volumes of gas, and the units of heat developed, also the comparative potential energy of the explosives of which I have given the composition. You will observe by the table that I have placed the explosives in descending order of potential energy, and you will also observe how very much more powerful are the modern explosives than is gunpowder, which held its own during so many hundred years.

Older Explosives.

—	Amide	Powder B	Powder D	Powder A	Pebble	R.L.G.	F.G.
Volumes of gas	400	315	282	254	278	274	263
Units of heat	832	715	745	800	721	726	738
Energy	332,800	225,225	210,000	203,200	200,438	198,924	194,094

—	Mining Powder	C. & H. No. 6	Powder C	Spanish Powder	Cocoa
Volumes of gas .	360	241	347	234	198
Units of heat .	517	764	525	767	837
Energy . . .	186,120	184,124	182,175	179,478	165,726

MODERN EXPLOSIVES.

—	Cordite Mark I.	Italian Ballistite	M.D. Cordite	Norwegian 167	Nitro-cellulose	Norwegian 165	B.N.	Lyddite
Volumes of gas .	875·5	810·5	913·5	899·9	934·0	909·9	822	960·4
Units of heat .	1,246·0	1,305·0	1,030·0	1,005·5	924·0	935·5	1,003	856·3
Energy . .	1,090,873	1,057,703	940,905	904,850	863,016	851,212	824,466	822,390

In these tables are given: first, the quantity of gas formed by the explosion; second, the units of heat developed; third, the product of the units of heat and volume of gas which represents approximately the comparative potential energy of the explosion.

I say approximately, because, as I shall have occasion to point out to you shortly, the units of heat and the quantity of gas vary very considerably, dependent on the pressure under which the propellant is exploded. In the present tables I have taken the transformation at the pressures at which the propellants are generally employed in guns.

To two or three points I may direct your attention.

You will note that, among the modern explosives, I have placed Lyddite. It is not suitable as a propellant, on account of its capacity for detonating; but although, when detonated effectively, it will reduce to dust the greater part of a cast-iron shell, you will observe its potential energy is lower than that of the seven propellants with which I have placed it. You will also note that the potential energies of the modern propellants are about four times that of the old gunpowder.

Observe also that, with the Amide powder, in which sulphur is altogether dispensed with, and which consists of 40 per cent. of potassium nitrate, 38 per cent. of ammonium nitrate, and 22 per cent. of carbon, the potential energy is about 65 per cent. higher than that of the old Waltham Abbey powders; and, in spite of the tendency of the ammonium nitrate to absorb moisture, Amide powders might have displaced the old powders had the much higher

energies of the nitroglycerine and nitrocellulose powders not made their adoption a necessity.

I have never been able to understand why sulphur was so long retained as a component of gunpowder. In the English Service it was, shortly before the adoption of modern powders, almost dispensed with in Cocoa powder when the proportion of sulphur was reduced to 2 per cent.; but as I was curious on the subject, after the dissolution of the first Committee of Explosives, I had four powders made, called in the tables A, B, C, and D, and you will observe that in two of these powders sulphur was altogether dispensed with (the small quantity of sulphur shown in the table is accidental, derived, doubtless, from the incorporating mills), while in C and D one powder had the sulphur increased by about 40 per cent., and in the other there was a reduction of 40 per cent., and you see from the table that the sulphurless and reduced sulphur powders give the highest energies, while the increased sulphur is near the bottom of the list.

Another point worthy of attention is, that in the old powders made up with different proportions of the same ingredients, if ever the volume of gas is high you will find the units of heat low. Thus, taking four of the last five powders on the table, you see that in the Mining and C powders the volume of gas is large, 360 and 347 c.c. per gramme, while the units of heat are very low—namely, 517 and 525 respectively. With the Spanish and the Cocoa, on the other hand, the volume of gas is very low, 234 c.c. and 198 c.c. per gramme, but with these two powders the units of heat are large—namely, 767 and 837 units respectively—and, in consequence of the higher heat, the erosive power of these two last powders is correspondingly high.

With the view, however, of studying the question of the most effective use of the gunpowders then in the Service, I had, two or three years before experiments commenced with the Nitroglycerine and Nitrocellulose powders, a gun of 32 calibres in length bored to a diameter of 5·87 inches, and I had five successive chambers, the largest chamber being five times the capacity of the first chamber, and in each chamber four densities were employed—namely, approximately, ·24, ·48, ·72, and ·96—while the powders employed were R.L.G_2, Pebble, Prismatic, and two descriptions of Prismatic cocoa powder.

I should only fatigue you were I to attempt to epitomise the whole of these experiments, but I may mention that at the end of the series with each chamber I fired a series with increasing weights of shot. The successive weights of shot were 30, 60, 90, 120, 150, and 360 lb., and I give, for the sake of comparison, the results of this series in the first chamber for the R.L.$G._2$ and one of the Cocoa powders.

R.L.G_2.

Weight of shot	Velocity, f.s.	Chamber pressure, tons per sq. inch	Energy developed, foot-tons	Mean pressure in bore
30 lb.	2126	14·0	972	2·29 tons
60 „	1641	17·5	1125	2·65 „
90 „	1370	18·5	1178	2·78 „
120 „	1209	18·9	1196	2·82 „
150 „	1083	19·6	1192	2·81 „
360 „	691	22·7	1192	2·81 „

Taking the first series in the first chamber, you will see from the table, the charge being 10 lb. of R.L.G_2.

The velocity realised with the 30-lb. shot was 2126 f.s., that velocity falling to 691 f.s. with the 360-lb. shot, while the chamber pressure rises with the successive weights from 14 tons on the square inch to 22·7 tons.

To the next column, which represents the energy realised in the projectile, I call your particular attention. The energy realised with the 30-lb. shot is 972 ft.-tons, increasing with the 60-lb. shot to 1125, with the 90-lb. shot to 1178, and with the 120-lb. to 1196 ft.-tons; but here the limit of energy was reached, as with the 150-lb. and the 360-lb. shot the energy is practically the same.

Thus, although Robins was much mistaken in his estimate of the maximum pressure due to fired gunpowder, his remark that the whole of the powder he employed must have been fired before the shot was materially moved from its seat was fully justified, as he had found that, when the weight of the shot was doubled, trebled, etc., the energy realised was not very materially increased, as it should have been if the combustion of the powder was so slow as some authorities supposed.

Robins's argument is unanswerable, and it is difficult to understand why the great authorities, such as Rumford, Piobert, Cavalli, etc., who supposed that the ignition of powder was comparatively slow, did not make the simple test that Robins had proposed and had himself tried.

Alongside the table for R.L.G_2 you see a table of results from one of the slow-burning Cocoa powders, the same weight of charge being used.

Cocoa (Slow burning).

Weight of shot	Velocity, f.s.	Chamber pressure, tons per sq. in.	Energy developed, foot-tons	Mean pressure in bore
30 lb.	1515	5·0	493	1·16 tons
60 „	1291	5·5	693	1·64 „
90 „	1143	5·8	811	1·91 „
120 „	1040	6·0	878	2·07 „
150 „	948	6·8	921	2·17 „
360 „	654	9·7	1065	2·51 „

With this powder the results are very different, the velocities are lower with all weights of shot, and the chamber pressures are all through less than half of those with the R.L.G.$_2$; but, although the energies realised with the slow-burning powder are considerably less than with the R.L.G.$_2$, the increment of energy is much greater.

In the former, between the 30-lb. and 360-lb. shot, there is only an increase of energy of 25 per cent.; with the Cocoa powder, the energy is more than doubled. But I must not lecture on these experiments, and I will only refer you to the table here, in which the results are given with two powders—Pebble and a quick-burning Cocoa powder; the charge with this second chamber being 20 lb.

PEBBLE.

Weight of shot	Velocity, f.s.	Chamber pressure, tons per sq. in.	Energy developed, foot-tons	Mean pressure in bore
30 lb.	2479	10·6	1300	3·17 tons
60 ,,	1984	13·2	1638	3·99 ,,
90 ,,	1675	14·9	1742	4·24 ,,
120 ,,	1513	17·2	1873	4·57 ,,
150 ,,	1358	17·5	1889	4·60 ,,
360 ,,	903	22·6	2034	4·96 ,,

COCOA (Quick burning).

Weight of shot	Velocity, f.s.	Chamber pressure, tons per sq. in.	Energy developed, foot-tons	Mean pressure in bore
30 lb.	2382	7·8	1200	2·92 tons
60 ,,	1934	10·5	1543	3·76 ,,
90 ,,	1660	11·9	1677	4·09 ,,
120 ,,	1457	13·1	1794	4·37 ,,
150 ,,	1356	14·3	1881	4·58 ,,
360 ,,	901	19·1	2017	4·91 ,,

From these two tables you will see that with the increasing weight of shot the velocities vary from 2479 and 2382 f.s. to 903 and 901 f.s., the chamber pressure with the heaviest shot being more than double that with the lightest, while the energy of the shot is increased by nearly 60 per cent.

These experiments, however, led to most important results with respect to the velocities and energies obtainable from the old powders; and a very great step in advance was made by my firm, who, acting on deductions from these and from other experiments in closed vessels carried out at Elswick, made 8-in. M.L. and B.L. guns, in which at a single bound the velocities at the same chamber pressures were

raised from about 1600 f.s. to about 2100 f.s., while the energies developed were increased by about 75 per cent., causing an immediate reconstruction both of guns and mountings.

From all these powders, upon which I have spent much time, I part with regret. They belong to the past, but I am not sure that I can say the same with respect to the Amide powders.

You will remember I pointed out to you that, with the Amide powder with which I experimented, there was an increase of potential energy over that of the Service powders of about 65 per cent., but, in that powder, less than half of the potassium nitrate had been replaced by ammonium nitrate, this smaller quantity being doubtless taken on account of the deliquescent properties of the latter salt; but that powder I have been able to keep, without difficulty, in Service powder cases for more than eighteen years, and if there be no practical difficulty in manufacturing a powder in which the whole of the potassium nitrate is replaced by ammonium nitrate, a much greater energy might be obtained, and it must be remembered that the Amide possesses the advantage of low erosion.

Some three or four years ago Sir W. Crookes suggested to me a method of protecting these powders, which both he and I have tried with success, and we may, therefore, hope that the difficulty to which I have referred may possibly be overcome.

I now turn to modern explosives, and the points to which I wish to direct your attention are:—

First, the tensions of the gases of the various propellants at the moment of explosion for all densities from ·05 to ·5.

Second, to ascertain the variation in the proportion of the resultant gases which accompanies a change in the density of the explosive fired.

Third, to determine the volume of permanent gases and aqueous vapour generated by explosion.

Fourth, to determine the corresponding units of heat generated by the explosion, and thence to arrive at the approximate temperature of explosion.

Fifth, to ascertain the time of combustion of the explosives under different pressures, and of different dimensions of cords, tubes, etc.

Sixth, to find the rapidity with which the explosives part with their heat to the walls of the vessels in which they are fired.

I proceed to describe the apparatus employed to obtain the data I have enumerated.

In the diagram I have thrown on the screen, "A" is the vessel in which the explosion takes place, "B" is the plug closing the vessel, on which also is shown the arrangement by which the gas, when desired, is allowed to pass at a feeble pressure either into the gasometer, or, at pleasure, into the gas tubes, and which, before the experiment, are filled with mercury, the stop cocks above and below being closed.

Immediately after the explosion, if the vessel be quite tight, the valve at "B" is slightly opened, and the gas allowed to pass into the tubes containing pumice stone and concentrated sulphuric acid to absorb the aqueous vapour, and then into the gasometer, where the volume is measured and the temperature of the gas at the moment of measurement and the barometric height is taken.

When it is quite certain that all the air is removed from the conducting tubes, the gas is allowed to flow into one of the tubes, and shortly afterwards, or at fixed intervals, into the other two tubes for the analysis of the permanent gases.

When the whole of the gas has been transferred to the gasometer, the explosion vessel is opened. There is always a considerable quantity of water on the cylinder surface, generally smelling slightly of ammonia.

As much as possible is removed by a weighed sponge, and placed in a weighed vessel closed by a ground-glass plate.. After all that it is possible to absorb has been removed, a weighed vessel of calcium chloride is placed in the chamber, and left for one or two days, when the same procedure is repeated with a second vessel, when the vessel is generally found to be quite dry.

The analyses of the permanent gases produced were chiefly determined by Dr. Sodeau with his gas analysis apparatus, which is admitted to be the most convenient that has yet been devised. The reconciliation of the analyses showed how accurate were the determinations—I may say much closer than I expected.

Several of the earlier analyses were made by the kindness of Sir James Dewar, at Cambridge, and a few analyses as checks were made at the National Physical Laboratory.

Having found that there was a considerable difference in the products of explosion when the explosives were fired under different densities, I considered it necessary to determine the heat for all points between the densities of ·05 and ·5. For this purpose I used a calorimeter arranged as is here shown, and it is practically, with slight variations, the same as that given by Ostwald in his "Manual of Physico-Chemical Measurements," the surfaces of the various vessels being nickel-plated and carefully polished.

The heat capacity of the equipage and the exploding vessel being carefully determined, for some hours before the experiment the calorimeter is kept in a room maintained at as even a temperature as possible, the explosion vessel, with the charge to be exploded, being in the water, so that the whole system may be at the same temperature.

The rise in temperature by the explosion being approximately known, the water in the outer cylinder is, before firing, kept at a temperature approximately half-way between the initial and final temperatures.

The thermometers employed were calorimetric, used only for

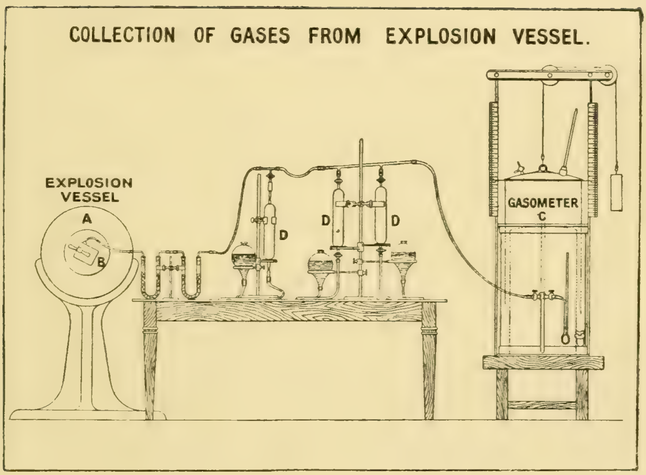

COLLECTION OF GASES FROM EXPLOSION VESSEL.
EXPLOSION VESSEL
A
B
D
D
D
GASOMETER
C

Fig. 1.

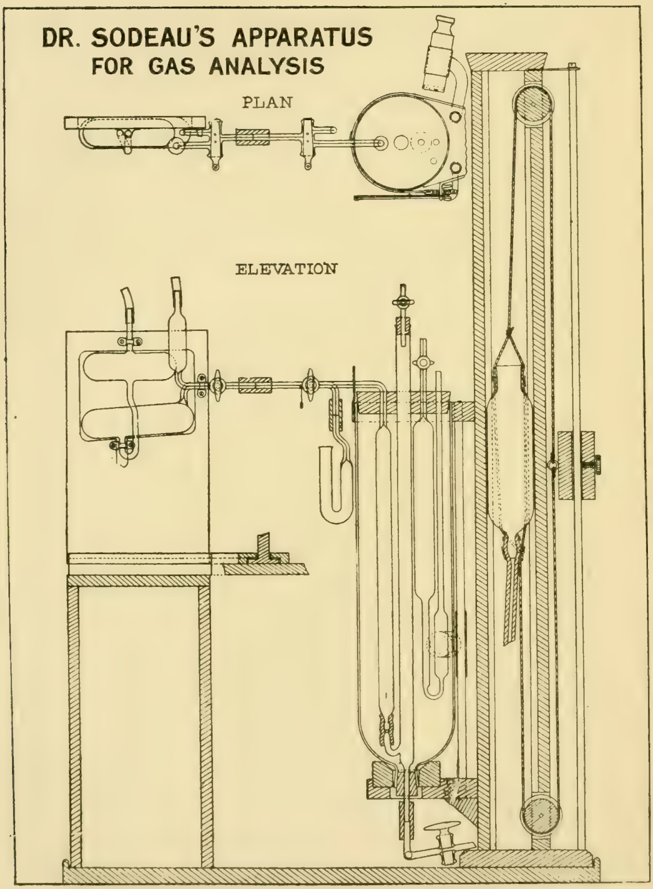


FIG. 2.

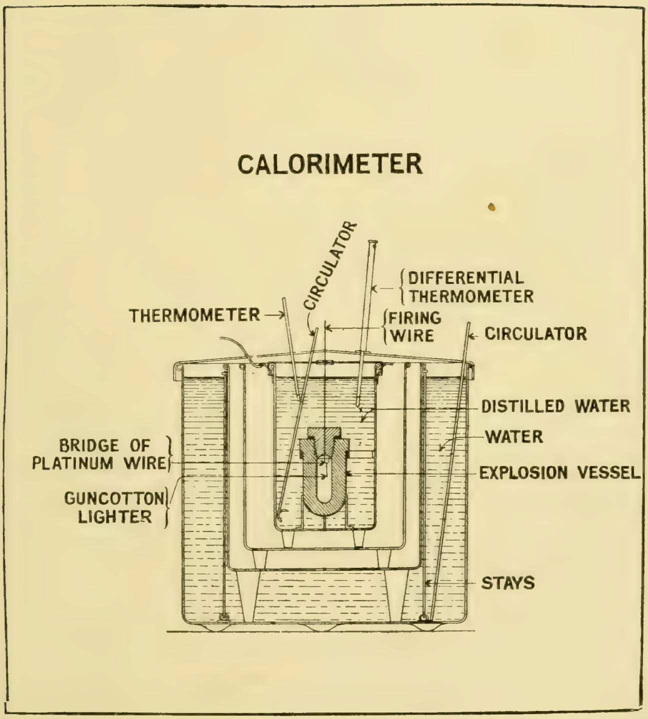

CALORIMETER
CIRCULATOR
DIFFERENTIAL THERMOMETER
THERMOMETER
FIRING WIRE
CIRCULATOR
DISTILLED WATER
WATER
BRIDGE OF PLATINUM WIRE
EXPLOSION VESSEL
GUNCOTTON LIGHTER
STAYS

FIG. 3.

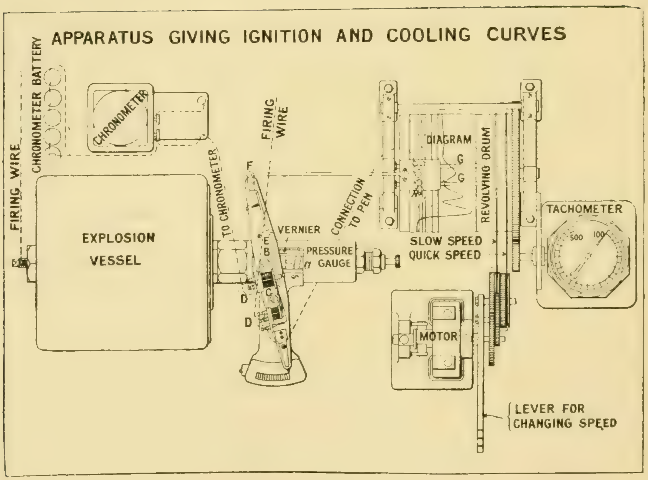

APPARATUS GIVING IGNITION AND COOLING CURVES
FIRING WIRE
CHRONOMETER BATTERY
CHRONOMETER
TO CHRONOMETER
FIRING WIRE
EXPLOSION VESSEL
F
E
B
C
D
D
VERNIER
PRESSURE GAUGE
CONNECTION TO PEN
DIAGRAM
G
G
REVOLVING DRUM
SLOW SPEED
QUICK SPEED
TACHOMETER
500
MOTOR
LEVER FOR CHANGING SPEED

FIG. 4.

determining changes of temperature, not absolute values. They could be read approximately to 0°·001 C.

Generally two observations were sufficient to determine the heat units. Thus, in the explosive which showed the least variation in tranformation, Italian Ballistite, lowest heats were 1308·9 and 1293·5; mean, 1301·2 units. Highest heat, 1340·6, 1329·9; mean, 1335·3.

Norwegian, Lot 165. Lowest, 925·1 and 901·4; mean, 913·3. Highest, 1099·9 and 1102·3; mean, 1101·1.

Norwegian, Lot 167. Lowest, 994·7 and 986·1; mean, 990·4. Highest, 1136·7 and 1142·1; mean, 1139·4.

The last apparatus with which I shall trouble you is that employed for determining the time that explosives of various forms and natures require for their transformation, and also for determining the rate at which the gases communicate their heat to the walls of the vessel in which they are confined.

The apparatus, which I show on the screen, consists of the usual explosion vessel, closed at the two ends by gas-tight plugs, through one of which passes the firing wire, while at the other end is fitted a pressure indicator provided with a steel plunger of small area, which is exposed to the gas pressure. An enlarged continuation of this plunger engages the end of a spiral spring, the strength of which has been accurately determined. Attached to the plunger at B is a lever, the fulcrum of which is fixed to the stationary bracket of the indicator, so that when the spring is compressed, motion is given to the end of the lever.

Fixed to the lever are two electric magnets, D—one to record seconds, the other to complete the firing circuit. A rocking bar, E, is coupled up over the seconds magnet, which is again coupled at the other end by a link, F, thus conveying the seconds beats of the chronometer to the pen tracing its path on the revolving drum.

Fixed to the frame of the revolving drum are two rods, G G, upon which slide the carriage carrying the recording pen. The pen is not in contact with the drum, being held up by a detent which is only liberated by the current which fires the charge.

It is necessary to have two speeds to the drum—one very rapid, about forty inches to a second; the other very slow, about one inch per second.

Before firing, the fast speed is employed, the slow speed band running free. Between one and two seconds after firing the change speed lever is raised, thus releasing the fast and employing the slow motion. The diagram is traced on a sheet of tinfoil backed by paper.

The chronometer is of the ordinary marine type, but is furnished with a seconds make and break arrangement.

The conduct of the experiment is as follows: All connections being made, the chronometer is coupled up, the pen carriage beating seconds, but no mark yet being made, as the pen is held up from the recording paper by the detent.

The drum is started, and when it has reached the desired speed, as shown by the tachometer, the button is pressed and the circuit is completed at the beat of the next second. The current simultaneously releases the pen and fires the charge. Between one and two seconds after firing, the speed lever is raised, and the speed of the motor reduced. The chronometer continues to beat seconds, thus giving the relation between time and pressure until the experiment is concluded.

Having now described the principal apparatus employed, I proceed to give you some of the results obtained; but, before doing so, I draw your attention to the samples, not only of the six explosives of which I am giving you details, but some other interesting samples showing, among other things, the variety of forms under which high explosives have been introduced in different countries. I am afraid you will have to wait till the conclusion of the lecture to examine closely the samples, but I draw your attention to the finish and accuracy of dimension of the Norwegian Ballistite, which I have not seen elsewhere equalled.

It is desirable to give you an idea of the difference of pressures at different densities of some of the explosives shown in the tables, and upon this diagram I have placed curves giving the relation of pressure to density of six of the modern explosives, and for purposes of comparison I have also given the same relation for Amide powder and the old gunpowder.

Taking first the density of ·4, and I need hardly point out that, so far as modern explosives are concerned, the pressures at that density are far above what is permissible in guns; but at that density Mark I. Cordite gives 41 tons per square inch (6249 atmospheres); Italian, and 167 Norwegian (about 39 tons per square inch); M.D. and Norwegian, 165—38·3 tons; Nitrocellulose, 34 tons (5182 atmospheres); Amide, 16·5 tons per square inch (2515 atmospheres), and Gunpowder, 7·8 tons per square inch (1189 atmospheres).

If we take the density which gives approximately the pressure permissible in guns—say density ·23—the comparative pressures are as follow:—

Cordite, a little over 20 tons per square inch (3048 atmospheres); Italian Ballistite and M.D., a little under 19 tons per square inch (2896 atmospheres); the two Norwegian and Nitrocellulose, between 16·5 and 17·5 tons (2515 and 2667 atmospheres).

I may observe, in passing, that, although the figures I have given represent as nearly as possible the pressures due to the samples with which I have experimented, the pressures would vary somewhat,

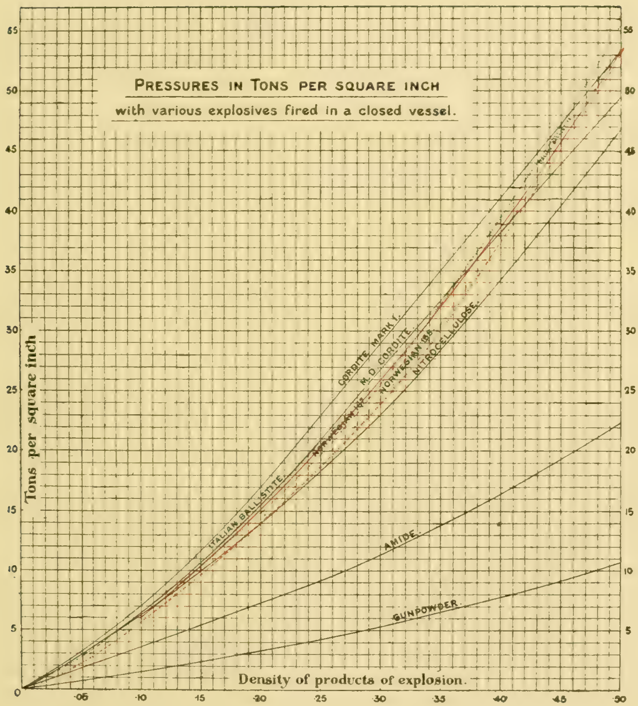
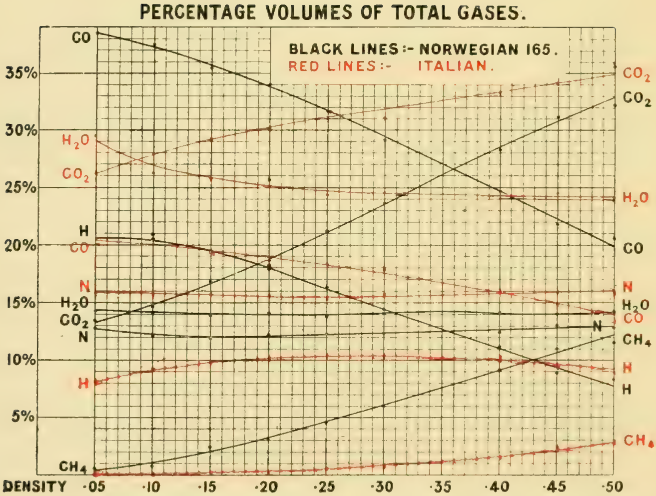

PRESSURES IN TONS PER SQUARE INCH
with various explosives fired in a closed vessel.
Tons per square inch
Density of products of explosion.
ITALIAN BALLISTITE.
CORDITE MARK I.
M.D. CORDITE.
NITROCELLULOSE.
AMIDE.
GUNPOWDER.
0
5
10
15
20
25
30
35
40
45
50
55
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50

Fig. 5.



PERCENTAGE VOLUMES OF TOTAL GASES.
BLACK LINES:- NORWEGIAN 165.
RED LINES:- ITALIAN.
35%
30%
25%
20%
15%
10%
5%
CO
H
H_2O
CO_2
N
CH_4
DENSITY ·05 ·10 ·15 ·20 ·25 ·30 ·35 ·40 ·45 ·50

FIG. 6.

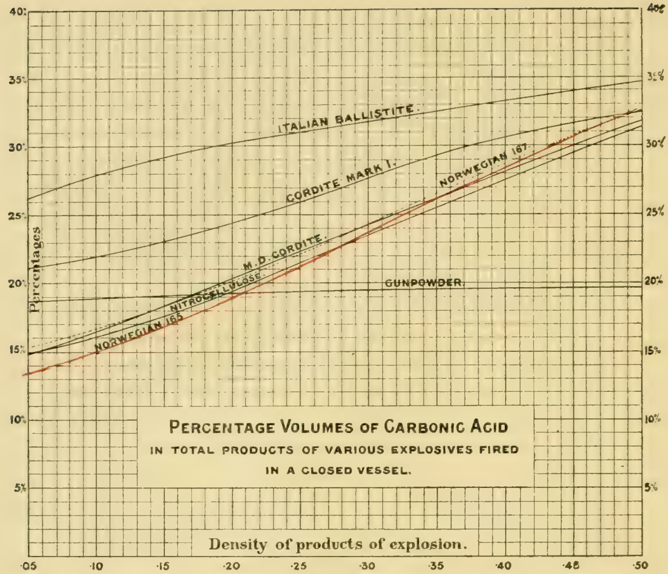

Percentage Volumes of Carbonic Acid
in total products of various explosives fired
in a closed vessel.
Italian Ballistite.
Cordite Mark I.
Norwegian 167.
M.D. Cordite.
Nitrocellulose.
Norwegian 165.
Gunpowder.
Percentages
Density of products of explosion.
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50

Fig. 7.

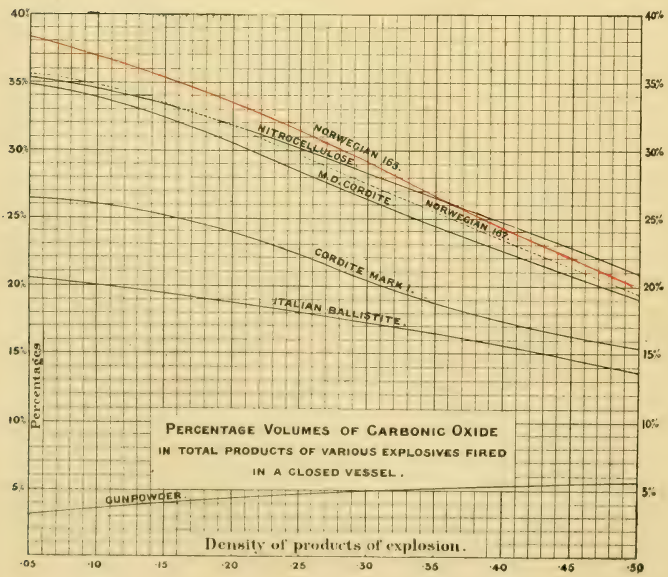

Percentage Volumes of Carbonic Oxide
in total products of various explosives fired
in a closed vessel.
Norwegian 165.
Nitrocellulose.
M.D. Cordite.
Norwegian 167.
Cordite Mark I.
Italian Ballistite.
Gunpowder.
Percentages
Density of products of explosion.
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50

Fig. 8.

dependent on the rapidity of combustion ; the loss of heat, and consequently of pressure, being due to the communication to the explosion vessel, that communication being extremely rapid.

I now turn to this diagram, which shows the difference in the transformation of explosives more readily than if I gave merely the figures.

The explosives I have selected for comparison are the Italian Ballistite and Norwegian 165. The lines for the former are in red, those for the latter in black, and you will observe how great the differences are.

Thus, with Italian Ballistite the percentage of CO_2 commences at 26 per cent., rising to 35 per cent., while the Norwegian commences at 13 per cent., rising, at ·5 density, to 33 per cent. With the Italian the CO commences at a little above 20 per cent., falling to a little less than 14 per cent., while the Norwegian commences at 39 per cent., falling to 20 per cent. The hydrogen with the Italian varies very little at about 10 per cent., while the Norwegian falls from 20·5 per cent. to 7·7 per cent. With marsh gas both explosives commence with a trace ; but the Norwegian rapidly increases to over 12 per cent., while the Italian only reaches 2·7 per cent.

The Italian has from 29 per cent. to 24 per cent. aqueous vapour, while the Norwegian is practically constant at 14 per cent.

The wide differences of the transformation under considerable differences of pressure can be seen from the diagram I have just explained, but a better idea of these changes and of the different percentages of the component gases can be arrived at by throwing on the screen for the modern explosives the changes and the proportions of the same gas for the several explosives.

Here, for example, you see how widely the percentage of CO^2 varies with all the modern explosives with the density. Take M.D. With a density of ·05 there is only 15 per cent., while at ·5 density it is nearly 32 per cent. You will observe also that, although there is a very wide difference, nearly 13 per cent., between Italian Ballistite and Nitrocellulose, as the density increases the difference in all the explosives lessens, so that there is only about 3 per cent. difference in CO_2 between the whole of the explosives at a density of ·5. The percentage of CO_2 in the old gunpowder is also shown.

Taking now CO, the difference between the several explosives is quite as great as in the case of CO_2, but here the curves show that, with increase of pressure, the percentages are decreasing rapidly, while, as we would expect, the tendency of the curves to approximate at the higher densities is clearly shown.

You will note that the Italian Ballistite, which in the last diagram was the highest curve, is now the lowest, and, in fact, as might perhaps be anticipated, the position of the curves is exactly reversed.

The next element, of which I show the curves, represents the per-

centages in hydrogen. The percentage with the density of ·05 varies from 8 per cent. to over 20 per cent. The initial percentage in the whole of the explosives increases slightly with increase of pressure, and then (with the exception of the Italian Ballistite) decreases rapidly to about 8 per cent., the curves at the density of ·5 being so close that a difference of little more than 1 per cent. includes all the curves. The rapid decrease of hydrogen is doubtless due to the great increase of CH_4. You will, no doubt, have noted that the hydrogen of the Italian Ballistite is nearly constant, the constancy being due to the decrease in the amount of aqueous vapour, and to the small quantity of CH_4 formed ; but it is remarkable that with Italian Ballistite there is far less variation, due to density in the products of explosion, than is the case with any other of the explosives.

I will only trouble you with one more diagram of the products, and that shows the volumes of marsh gas. The order, with one slight exception, is the same as with the carbonic oxide. At the density of ·05 there is, with the whole of the explosives, a trace only ; but, as you see at density ·5, there are both increase and very considerable variation, the percentage reached being in one case over 12 per cent.

The next point to which I draw your attention is the units of heat liberated by the explosion. These, as before, I show on the screen, and you will note how all the curves which give the units of heat, the water being gaseous, have approximately the same form, all dropping a little at first, and subsequently rising somewhat rapidly, the higher heat being attributable chiefly to the much larger volume of CO_2 formed at the highest pressures, the weight of the gas at the density of ·5 being, with all the explosives named, more than half of the total weight of the gases, and the heat of the Italian Ballistite, as in the case of the volumes of the gases, showing but little variation.

The difference between the heat of these two explosives, Cordite and Italian Ballistite, as compared with that of the four other explosives, is at the lower densities, including those with which artillerists are concerned, great ; but this difference is much reduced at the high densities, due, as I have said, to the rapid increase in the quantity of CO_2 with higher densities.

I now approach a point concerning which it is necessary to give a few words of explanation. The observations I have laid before you hitherto depend upon direct observation, and these observations have been confirmed in so many ways that they must be accepted as correct, subject to the slight errors from which no human work is exempt ; but the point we have now to determine is, what are the temperatures which the firing of the explosives we are considering produce ?

I fully admit that many physicists may differ from the view I have taken, but I have a good deal to corroborate my view.

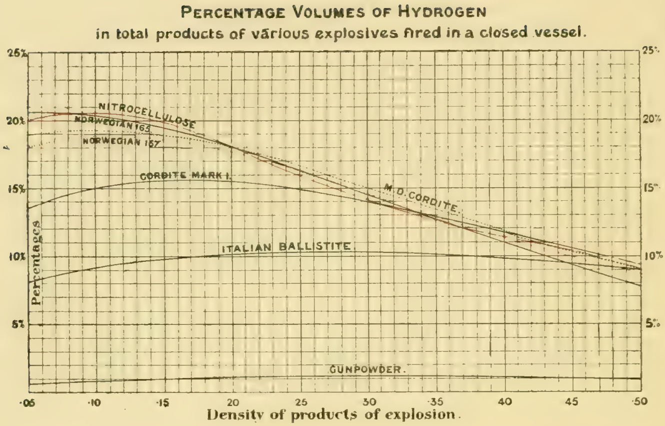

PERCENTAGE VOLUMES OF HYDROGEN
in total products of various explosives fired in a closed vessel.
25%
20%
15%
10%
5%
Percentages
NITROCELLULOSE
NORWEGIAN 165
NORWEGIAN 167
CORDITE MARK I.
M.D. CORDITE.
ITALIAN BALLISTITE.
GUNPOWDER.
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50
Density of products of explosion.

FIG. 9.

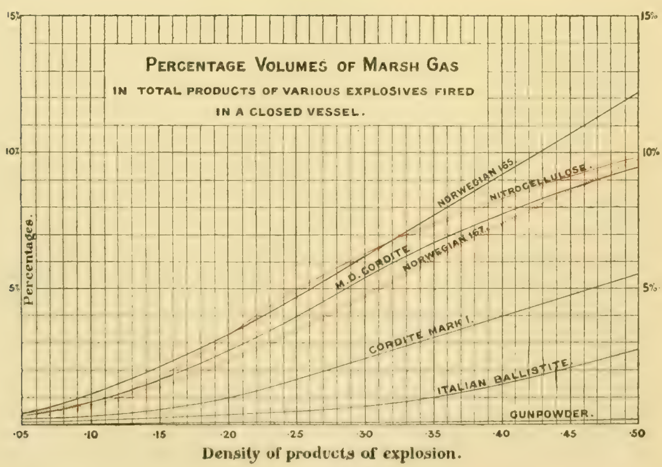
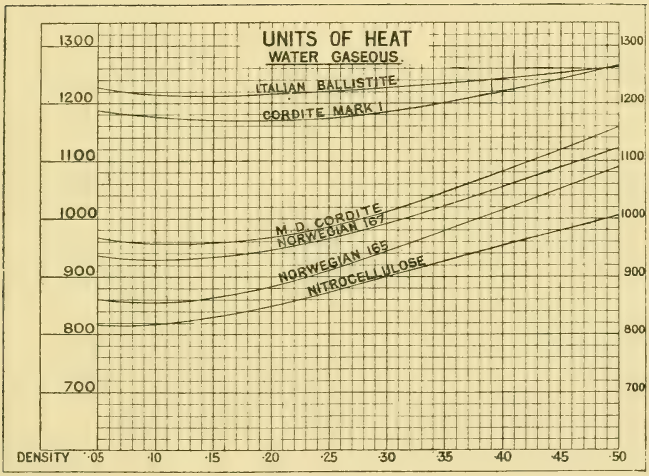

PERCENTAGE VOLUMES OF MARSH GAS
IN TOTAL PRODUCTS OF VARIOUS EXPLOSIVES FIRED
IN A CLOSED VESSEL.
15%
10%
5%
Percentages.
NORWEGIAN 165.
NITROCELLULOSE.
NORWEGIAN 167.
M.D. CORDITE
CORDITE MARK I.
ITALIAN BALLISTITE.
GUNPOWDER.
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50
Density of products of explosion.

FIG. 10.



UNITS OF HEAT
WATER GASEOUS.
ITALIAN BALLISTITE
CORDITE MARK I
M.D. CORDITE
NORWEGIAN 167
NORWEGIAN 165
NITROCELLULOSE
1300
1200
1100
1000
900
800
700
DENSITY
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50

FIG. 11.

Commencing with the old gunpowders, Sir F. Abel and I differed from the opinion of our distinguished predecessors, Bunsen and Schischkoff, in placing the temperature of explosion of gunpowder much lower than their estimate. Dependent on the nature of the powder, I satisfied myself that the temperature of explosion of the various powders I examined ranged from 1800° C. to about 2200° C., the latter temperature being apparently reached by the Spanish Pellet, with which powder Iridio-platinum wire was fully melted, while the Mining powder just melted platinum.

Pellet gun-cotton gave a much higher temperature, the platinum wire being apparently volatilised.

I may mention that, in these early experiments, I made the determination of heat for only one density for each explosive; but finding in modern explosives that the transformation varied so greatly with the density of charge, determinations were made for every density, the results being the curves I have just shown you.

To obtain the temperature of explosion I used two methods; the first was that followed by Bunsen and Schischkoff, who determined the temperature by dividing the units of heat by the specific heat.

The second was, knowing the pressure at the moment of explosion, and also at 0° C., it was possible, the co-efficient of expansion with temperature being known, to calculate what temperature was necessary to raise the pressure of the volume of gas at 0° C. and 760 mm. to the pressure at the moment of explosion.

With respect to the first of these two methods, for any degree of accuracy it is essential that the specific heats should be as accurately determined as possible. Carbonic anhydride, as I have already pointed out, on account of its great preponderance of weight, is, as regards specific heat, by far the most important product, and fortunately there has been recently published by Holborn and Austin a very careful determination of specific heat for CO_2 and some other gases. The formula they give connecting the specific heat and the temperature has been proved by them up to 800° C., and their determinations up to the same temperature have been confirmed by Langen.

At the lower temperatures the increments to the specific heat with increase of temperature is very considerable, but these increments rapidly decrease, and vanish altogether when 1400° C. is reached, at which temperature CO_2 is partially dissociated.

The other principal gases, carbonic oxide, hydrogen and nitrogen, although no gas rigorously follows Marriotte's Law, are among those which follow it most closely, the specific heat increasing but slightly between 0° C. and 800° C., and the increments as with CO_2 being smaller as the temperature is increased. I have treated all these gases at the temperature with which we are concerned as perfect gases.

Now the two formulæ I have referred to give, for five of the

explosives, identical temperatures somewhere between the densities of ·3 and ·35, and above these densities agree as well as we have any right to expect, but below these temperatures there are wide differences, and the question is, how are these differences to be explained ?

Before attempting an explanation, I throw on the screen two pairs of curves, showing the temperatures derived from the two formulæ.

The red lines show the temperatures derived from the first formula, the black from the second, and you will note how wide is the difference between the two curves at the lowest density, the differences being approximately 2000° C. and 1500° C. You will observe also, that the equations give identical temperatures for the two explosives at the density of ·35, and above that density the accordance is as close as we have any right to expect from observations of this nature.

At the densities with which artillerists are concerned, say a mean density of ·2, you will observe that in the case of the Cordite the difference of 2000° C. is reduced to 800° C., and with the Norwegian the difference of 1500° C. is reduced to 600° C., and, as you see, at a density of ·35 there is, for both explosives, no difference at all.

Now the question is, how are we to account for this remarkable difference ? And the answer I have ventured to give is as follows :—

At atmospheric pressure CO_2 commences to be dissociated at about 1300° C., and this dissociation will give rise to a fall in temperature.

At high densities, as I have said, the two equations give accordant results. I therefore think it reasonable to suppose that the results recorded are due to dissociation at the feeble pressures at low densities, which dissociation is prevented by the high pressures existing at densities above ·3.

It is worth while examining whether there is any corroboration to be obtained for this view with the explosives I am referring to. The difference given by the two formulæ varies from 2200° C., in the case of the explosive that gave the highest heat, to 950° C. in that which gave the lowest. Now if my view be correct, the difference between the two formulæ ought to depend chiefly upon the amount of CO_2 to be dissociated, and on the amount of heat to cause that dissociation—the higher heat, of course, dissociating a larger proportion of CO_2, while if there be a larger quantity of CO_2 there is more for the higher heat to operate on.

My view is, that these facts are sufficient to account for the difference between the formulæ in the case of the explosives of which I am speaking.

Now what are the facts ? With Italian Ballistite, which gives the highest difference, the resultant CO_2 amounts to 37 per cent. of the permanent gases, while Nitrocellulose gives only 18 per cent. Again, Italian Ballistite gives 1228 units of heat, while Nitrocellulose

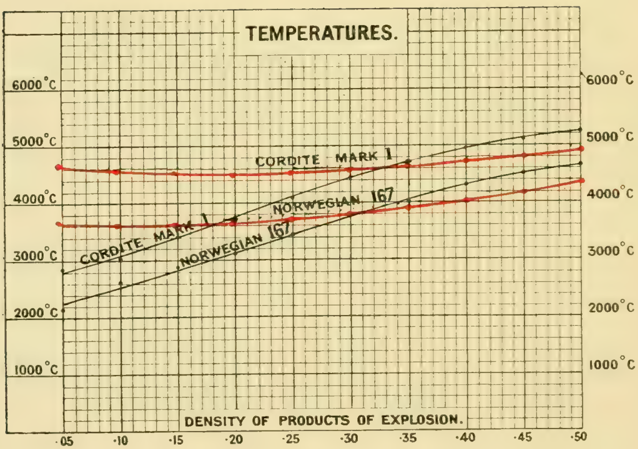

TEMPERATURES.
6000°C
5000°C
4000°C
3000°C
2000°C
1000°C
CORDITE MARK I
NORWEGIAN 167
CORDITE MARK I
NORWEGIAN 167
DENSITY OF PRODUCTS OF EXPLOSION.
·05
·10
·15
·20
·25
·30
·35
·40
·45
·50

FIG. 12.

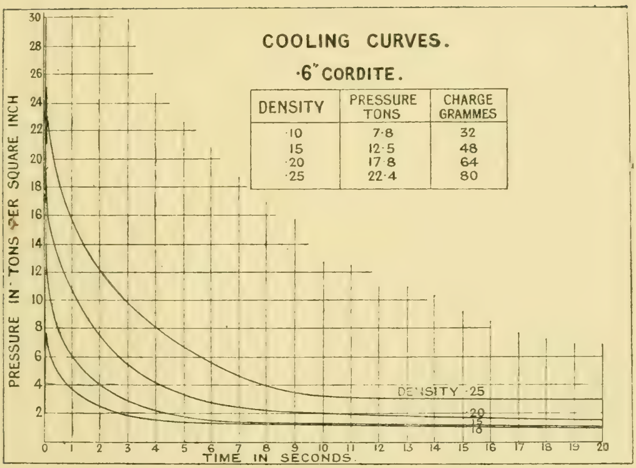

COOLING CURVES.
·6" CORDITE.
DENSITY
PRESSURE TONS
CHARGE GRAMMES
·10
7·8
32
15
12·5
48
·20
17·8
64
·25
22·4
80
PRESSURE IN TONS PER SQUARE INCH
TIME IN SECONDS.
DENSITY ·25
·20
·15
·10
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
2
4
6
8
10
12
14
16
18
20
22
24
26
28
30

FIG. 13.

gives only 818. The other explosives give intermediate differences, and these differences are, as I have said, dependent on the units of heat and the quantity of CO_2.

I now come to the question, can I produce any experiment to corroborate the temperatures I have tentatively assigned?

I shall give you two examples—one with the explosive giving the lowest temperature, the other with Mark I. Cordite, which is nearly the highest.

Eighteen months ago I fired a charge of nearly 3 kilogrammes ($6\frac{1}{2}$ lb.) of Nitrocellulose at a density of ·28. In the centre of the charge a small packet of osmium was placed.

On opening the vessel after explosion, and after the gases, etc., were removed, the walls of the vessel were scraped, and these scrapings were found to contain osmium. This appears to me to prove that at least a portion of the osmium had volatilised. I do not know the volatilising point of osmium, but its melting point is 2500° C., and I suppose its boiling point is considerably higher.

In the curve I have given in my paper in the "Royal Society Transactions," the temperature of explosion at the density of ·28 is given at 3200° C.; and, if allowance be made for the heat necessary for volatilisation, and also for the rapid cooling of the gases, about which I shall have something to say presently, I think the temperature I have given is not very much removed from the truth.

Again, at the same density, I fired the same charge of Mark I. Cordite, and in the centre of the charge I placed a small packet of electrically deposited carbon. The temperature I have given for this density is 4300° C.

The solid and fluid products of combustion were handed over to Sir W. Crookes, for whom I made the experiment.

In the latest "Physikalisch-Chemische Tabellen" carbon is given as "unschmelzbar," but Mark I. Cordite appears to have melted it, as Sir W. Crookes, after long and careful examination, obtained crystals which were undoubtedly diamonds. Sir William gives the melting point of carbon at approximately 4400° C. absolute temperature, and I think that this is fairly confirmatory of the temperature that my curve assigns.

I shall only trouble you on one or two further points. I throw on the screen a diagram which shows the rapidity with which small charges of an explosive (Mark I. Cordite) part with their heat to the walls of the vessel.

The curves you see are the diagrams on an altered horizontal scale, traced by the explosive itself. You will observe the axis of abscissæ gives the time in seconds, and the vertical axis the pressure in tons per square inch. The charges have densities varying from ·1 to ·25. With the higher densities there is considerable oscillation, due to the run of the spring, but it is not difficult to ascertain from the diagram the true pressure. Besides, when accurate time of

lighting is not required, this vibration can easily be prevented by compressing the spring to a point a little short of the expected pressure. Taking first the ·25 density, and noting that the maximum pressure is 22·4 tons per square inch, you will note that the pressure and temperature are approximately halved in $2\frac{1}{2}$ seconds. With ·2 density, the maximum pressure is 17·8 tons, and this pressure is approximately halved in a second and a half. Density ·15 gives a pressure of 12·5 tons per square inch, and that pressure is halved in a little over a second, while the pressure due to the density of ·1 is halved in a little less than a second. In this last density you see there is no vibration of the pen, and the pressure is identical with that given by the crusher gauge.

I now throw on the screen the last diagram, which gives the times that the charges, the rate of cooling of which you have just seen, take to completely burn.

You will understand that the curves for densities ·1 and ·15 are facsimiles of the curves traced by the explosion. The densities ·2 and ·25 are also traced by the explosion, but the vertical heights are doubled in order to have the four curves on the same scale. The vertical line gives the pressures in tons per square inch.

The actual times from firing the charge are given by the black figures, but I have also shown separately the time taken to consume the cordite after the charge was fully lighted. For the densities ·15, ·2 and ·25, these times are approximately fifteen thousandths of a second, eleven thousandths of a second, and a little less than ten thousandths of a second; while the times taken fully to light are approximately nine thousandths of a second, fifteen thousandths of a second, and thirteen thousandths of a second; the smallest charge, probably due to a stronger primer, being the most quickly lighted. The red lines drawn on density ·2 and ·25, mark the increment in the rate of burning each as compared with the previous experiment. The black lines across, mark the maximum pressure and the time taken to reach it.

Earlier to-night I referred to the improvements in velocity and energy obtained with the old propellants. I conclude my lecture by pointing out that the new propellants give an additional velocity of nearly 1000 feet per second, and more than double the energy of the projectile.

[A. N.]

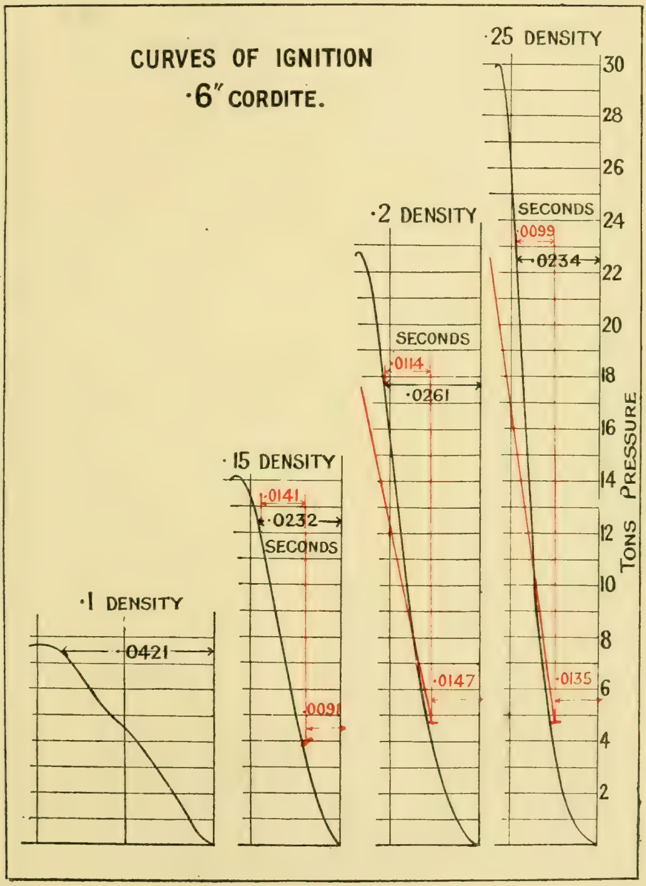

CURVES OF IGNITION
·6″ CORDITE.
·1 DENSITY
·0421
·15 DENSITY
·0141
·0232
SECONDS
·0098
·2 DENSITY
SECONDS
·0114
·0261
·0147
·25 DENSITY
SECONDS
·0099
·0234
·0135
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
TONS PRESSURE

FIG. 14.

WEEKLY EVENING MEETING,

Friday, January 25, 1907.

The Right Hon. Lord Alverstone, G.C.M.G. P.C. M.A. D.C.L. LL.D. F.R.S., Vice-President, in the Chair.

Charles Welch, Esq., F.S.A., Late Librarian to the Corporation of London.

The Guildhall Library.

The story of the Guildhall Library has its origin in the interesting personality of Sir Richard Whittington, around whose memory has gathered a halo of romance and veneration which has endeared him to Londoners of every generation since his day. The good knight well deserved this homage—a merchant prince whose far-sighted enterprise not only brought great gain to himself, but largely contributed to build up and extend his country's commerce; a patriot whose munificent assistance to his sovereign was on a scale befitting a king rather than a subject; a magistrate whose wisdom and probity secured for him the approbation of the king and of his fellow citizens, by whom he was four times placed in the honourable position of Lord Mayor; a terror to evil-doers, a friend of the poor, and lastly, an enthusiastic lover of learning, and by means of his great wealth one of its most generous supporters—Sir Richard is truly described by Canon Lysons, his biographer, as "The model merchant of the Middle Ages." His benevolence to the poor is fittingly commemorated by one of the historical paintings which now decorate the interior of the Royal Exchange in which he is represented with his wife, Dame Alice, relieving the wants of poor people who are crowding round his door. That his charitable disposition was associated with genuine piety is clearly seen in the kindness that pervaded all the acts of his busy life, and is further shown by the institution of a religious service at the Guildhall Chapel before his election for his third mayoralty in 1406, a service which has lasted in its post-Reformation form to the present day.

It would seem that Whittington was too much occupied with his many public duties to arrange during his life-time for the exact disposition of his immense fortune, which was left absolutely at the disposal of his executors. These were men who enjoyed his complete confidence and with whom he must have frequently discussed in outline the various beneficent schemes which they so faithfully carried out after his decease. Their names were John Coventry, a brother alderman, who was mayor in 1425; John, or Jenkin,

Carpenter, the Common Clerk of the City, and well known as the author of 'Liber Albus,' and founder of the City of London School; William Grove, and John White, a cleric.

In fulfilment of their duties as Whittington's executors, Carpenter and his colleagues, after the good knight's death in 1423, and after duly procuring the necessary official permission, pulled down and rebuilt the gaol of Newgate, contributed largely to the construction of the new Guildhall, established a library at the Grey Friars, and built a library at the Guildhall. In this last work they were joined by the executors of William Bury, of whose history nothing is known. John Stow in his 'Survey of London,' speaking of the Guildhall Library, says, "The arms of Whittington are placed on the one side in the stone work, and two letters, to wit, W and B, for William Burie, on the other side."

The first official notice of a Library at the Guildhall is contained in the following extract from the records of the Corporation, the original being in Latin: "Item, the same day (to wit the 27th September A° 4 Henry VI., 1425) it was granted by the said Mayor and Aldermen and Commonalty that the new House or Library, which the said executors (to wit of the testament of Richard Whityngton) and the executors of William Bury made near the Guildhall, and the custody of the same, together with the chambers built underneath the same, should be in the disposition and management of the said executors. In such manner that all and everything, which the same executors should think fit to ordain, touching the placing the books or doing other matters—shall be done and executed as fully and perfectly as if they had been ordained by the said Mayor, Alderman, and Commonalty, by their own authority or by authority of the franchises of the said City without any kind of refusal or contradiction, etc."

The College, or Chapel, of St. Mary Magdalene and all Saints, to which the new Library was attached, stood on the east side of Guildhall Yard, and adjoined the south side of the Hall itself. It was built about the year 1299 and rebuilt in 1449, the establishment consisting of a custos, seven chaplains, three clerks, and four choristers.

The Library thus erected was a separate structure of two floors, and conveniently approached from Guildhall Yard. It is described in a schedule of the possessions of the Guildhall College taken at the Dissolution, and dated 24th July, 1559, 3 Edward VI., as "a certen house nexte unto the same Chapell apperteyning, called the Library all waies res'ved for studente to resort unto w^t^ three chambres under nithe the saide library, which library being covered w^t^ slate is valued together w^t^ the Chambres at xiij s iiij d" yearly. From the same document we learn, that "the saied library is a house appointed by the saied Maior and cominaltie for resorte of all students for their education in Divine Scriptures."

The generous liberality of Richard Whittington and William Bury, the founders of the Library, was well supported by Whittington's

executor, John Carpenter. His will, proved in the Consistory Court of London on May 12, 1442, contains the following bequest: "If any good or rare books shall be found amongst the residue of my goods, which, by the discretion of the aforesaid Master William Lichfield and Reginald Pecock, may seem necessary to the *common library* at Guildhall, for the profit of the students there and those discoursing to the common people, then I will and bequeath that those books be placed by my executors and chained in that library, under such form that the visitors and students thereof may be the sooner admonished to pray for my soul."

The Library was placed under the official charge of one of the priests of the Guildhall Chapel.

The earliest Librarian of whom any record exists, was John Clipstone, priest, who presented the following quaint petition to the Court of Aldermen, on July 13, 1444: "To the full Honorable Lord and Souveraignes Maire and Aldermen of the Citee of London, Besechitch lowely your Prest and Bedeman Maister John Clipstone, Keper of your Liberary atte Guyldehalle for as moche as it hath likede you for to take to hym the kepinge and charge of the said Liberary. Please it to you, for to considre the greet attendaunce and charge the which he hath with it, and in waytenge therupon to graunte that he may be made so sure of his lyflode, housyng, and easement of the gardyn which he hath for that occupacion atte this day, that he be nat hereafter putte away therefro ne noo part thereoff, nor noon other charge put upon hym so that he may have more cause and occasion to pray besyly for the weele of you and of the said Citee, etc." The minute of the Court, in Latin, states that the request having been read and fully considered, and the great merits of the petitioner and his diligence having been weighed, his request was granted by the said Maior and Aldermen as long as the said Master John may be willing to hold the office in person, for the whole of his life, so that he may enjoy the emoluments, even though he should be laid by through sickness. He died in 1457, and was buried in Guildhall Chapel. He appears to have been succeeded by Thomas Mason, one of the chaplains, who was appointed to a perpetual chantry in 1466. On the first page of a volume of tracts and essays preserved in the library of Magdalen College, Oxford, is a MS. Latin note, from which it appears that Sir Thomas Mason, besides his official position as the Librarian at Guildhall, acted afterwards in a similar capacity to Master Richard Langharne, for whom he bought the book in question, in the year 1468, at a cost of 13*s.* 4*d.* Again, in 1510, we meet with the burial in the Chapel of Edmond Alison, priest, and custos of the Library, he being the last of the early Librarians of whom any record is preserved.

The Guildhall Library thus became one of the first, perhaps actually the first, of the libraries established in this country for the free use of the public.

The University of Oxford soon afterwards found a benefactor in

Humphrey, Duke of Gloucester, who contributed to build the Divinity School, and, by undertaking the cost of a library over it, became the Founder of the Library. His gift of books began in 1439, and in 1447 had reached a total of 600 volumes. The after-fortunes of Duke Humphrey's Library unfortunately followed only too closely those of Whittington's Library at the Guildhall.

London shared in the general awakening to literary and educational effort, and many of the prominent merchants who were Whittington's contemporaries, and held office as Lord Mayor, gave lavishly of their great wealth for educational endowments. Such an one was Sir William Sevenoak, Lord Mayor in 1418 (who founded a school in his native town of Sevenoaks), one of the earliest of a long line of London citizens who established and endowed famous grammar schools in London and various parts of the country.

Among the students who enjoyed the use of the Guildhall Library in the early years of the sixteenth century we may certainly include the celebrated Sir Thomas More, afterwards the famous Chancellor of Henry VIII. Early in his career More had held office under the Corporation as Under-Sheriff, and, with his great love of learning, must have been a frequent visitor at their Library. His taste for theological studies led him, while yet a young man, to give a course of public lectures on Augustine's 'De civitate Dei.' These were delivered in the church of St. Lawrence Jewry, on the opposite side of Guildhall Yard, almost facing the Library, whose rich store of theological books he doubtless used for preparing or revising his lectures.

Yet another eminent statesman, in his early days, made use of the Library. In 1448–49 the Court of Common Council received a request from the great Cecil, then Secretary of State to Edward VI., for the loan of St. Augustine's works, the subject of More's previous studies. In their reply to the distinguished suitor the Corporation showed themselves fully alive to their responsibilities as custodians of the Library :—

"Jovis xxxj^{o} Januar' A^{o} iij E. vj (1548–49, Journal, Court of Common Council). Item for sundrye consyderac̄ons movyng the Co^{r}te it ys ordered & agreid by the same that Mr. Cycyll shall have all suche boks of St. Augustyns works and other as he nowe desyreth that Remayne in the guylde hall chappell wth this gentle Requeste to be made to hym vpon the delyu^{r}ye of the same that this howse trusteth that he havynge ꝑused theym wyll Restore theym to the seid lyberarye there to Remayne to suche vse as they were ꝑvyded for."

Nothing more is known of the history of this old library. It only remains to tell the brief story of its destruction at the hands of the Protector, the Duke of Somerset.

John Stow, writing of the Guildhall Chapel, gives the following account of the Library and its unhappy fate : "Adioyning to this chappell on the south side was sometime a fayre and large librarie

furnished with bookes, pertaining to the Guildhall and colledge: these bookes (as it is said) were in the raigne of Edward the 6 sent for by Edward, Duke of Sommerset, Lord protector, with promise to be restored shortly: men laded from thence three Carries with them, but they neuer returned. This librarie was builded by the executors of R. Whittington, and by William Burie: it is now lofted through, and made a store house for clothes." No reference to this act of selfish rapacity is to be found in the City records, but it probably happened in 1549, when the revenues of the Guildhall Chapel were seized by the Commissioners of King Edward. No remonstrance was made by the Corporation, who probably knew only too well how much reliance was to be placed upon the Protector's promise that the books should be "shortly restored." The dismantling of the Library followed just a week later. The following entry is in the Journal of the Common Council: "Jovis xiij Marcij A° iiij° E. vj[ti] 1550. Item it is agreyd that the Chamberleyn shall for the ꝑfytte of the Citie sell all the deskes of the liberary of the Guildhald college to them that wyll gyve most for them."

At this time the Corporation were treating with Somerset and the Court for the purchase of the lands and buildings belonging to the dissolved College at the Guildhall, and they may have refrained from pressing the Duke for the return of their books from fear of prejudicing the negotiations. The King's Letters Patent for purchasing the lands and buildings were received on April 17, 4 E. VI. (1550). On the 6th of the previous month a lease of the building was granted to Sir John Ayloffe for use as a cloth market. "Jovis vj° Marcij A° RRg E vj iiij°. Item for certeyn consyderac̄ons movyng the Co[r]te yt is agreid by the same that S[r] John Aylif knight now kep[r] of blackwell hall shall have the hole lybarye of the Guyldhall Colledge as well above as ben̄eth from the feste of the Anūncyac̄on of o[r] ladye nowe nexte comyng for the terme of his naturall lyf, yeldyng therfore duryng the same terme to the Mayer & Coīaltye & Cytezens of this Cytie to thuse of the poore v[li]. So alweyes that he vse & occupye the same as a coēn m'ket howse for the sale of clothes and none other wyse."

A similar fate befell Duke Humphrey's Library at Oxford. In 1550 the Commissioners for reformation of the University, appointed by Edward VI., laid waste its contents in a strange spirit of ignorant and fanatical zeal. So complete was the destruction that in 1556 the very bookshelves and desks were sold as things for which there was no longer any use. Fortunately for the University of Oxford a new founder of her Library soon appeared in the person of Sir Thomas Bodley. The City of London had to wait nearly three centuries before any attempt was made to repair her grievous loss. The sad reflection also occurs, that within twenty-five years of the disappearance of the Library Sir Thomas Gresham established his College in the City. Had the Guildhall Library been then in

existence it would have been a great help to, and would itself have received much benefit from, its sister institution, with the probable result that the University of London at the present day would have been older by some 250 years, and the best endowed University in Europe.

It is deplorable to think of the priceless manuscripts and incunabula that perished in the destruction of this old library. Its books have never since been heard of, and not even a catalogue of them remains. There is just a possibility that a small, thick Latin manuscript on the duties of a priest, preserved in the present library, and entitled 'Oculus sacerdotis,' *may* have formed part of the original collection. Perhaps as the books were chiefly theological they were wantonly destroyed by the Duke of Somerset, and they may even have been "borrowed" by him expressly for that purpose.

Nearly three hundred years passed before any attempt was made by the Corporation to repair their loss. The second founder was Mr. Richard Lambert Jones, whose wide and liberal views, great energy, and conspicuous talent for public work in many departments, were of the highest advantage to his fellow citizens. On April 8, 1824, upon his motion, the Court of Common Council unanimously referred it to a Special Committee "to enquire and examine into the best mode of arranging and carrying into effect, in the Guildhall, a Library of all matters relating to this City, the Borough of Southwark, and the County of Middlesex, and to report thereon to this Court." The Committee thus appointed consisted of thirteen members, and Mr. R. L. Jones was unanimously elected Chairman.

The new Library did not start under such favourable auspices as its predecessor; no wealthy citizen appeared to follow Sir Richard Whittington's example in building for its reception a "fayre and large" house, and there is little doubt that the obscure and unsuitable apartments devoted to its use had a most unfavourable influence upon its development, notwithstanding the well-directed energy of the Committee under their indefatigable Chairman. On June 2, 1828, they reported, recommending that the rooms then occupied by the Irish Society, in the east wing of the front of the Guildhall, should be adapted for the purposes of the new Library, and that, meanwhile, the front room by the Exchequer Court should be used as a temporary depository. They also reported that the sum of £500 would be required for the outfit, and £200 annually for maintenance. At the outset the Committee wisely confined their purchases to books relating to the manners, customs, laws, privileges, and the history of the City of London and the neighbouring localities. The condition of the book market was then favourable for the procural of old and scarce London books, private collectors being fewer than at present, and our American rivals not being then in the field.

No better fortune could attend any library at its inception than to have the lines of its growth clearly laid down, and well is it for

the institution if those lines are firmly kept in view throughout its history. It is only a national collection which can afford to make adequate acquisitions in *all* departments of knowledge. In the case of ordinary libraries the line of growth soon becomes determined by the special circumstances of its locality, the needs of its readers, the acceptance or purchase of special collections, and the particular needs or tastes of the authorities. The Guildhall Library was especially fortunate in being founded as a "Library of all matters relating to the City," and its present position fully justifies its claim to have succeeded in this aim. The London collection far exceeds in extent and importance any similar collection that exists elsewhere, and contains many rare and some unique books and maps.

The services of Mr. William Upcott, Librarian of the London Institution, having been engaged for arranging the books, the Committee, early in 1828, recommended that the Library should be opened for use, that Mr. William Herbert should be appointed Librarian, and that a catalogue of the books should be prepared. The Library was accordingly opened in June following, with 1700 volumes, a catalogue of which was prepared by Mr. Edward Tyrrell, Remembrancer. The collection had increased by November 5, 1829, to 2800 volumes (1050 of which were donations) and nearly 2000 prints and 100 drawings, consisting chiefly of London topographical views and portraits of City celebrities. The books included a complete series of the 'London Gazette,' from its commencement in 1665 to 1792, and sets of the 'Gentleman's' and 'European' Magazines. By June 15, 1840, the Library contained nearly 10,000 volumes, and extensive additions to the premises had been obtained, including a room to accommodate a museum of local antiquities. A new edition of the catalogue was prepared this year by Mr. Herbert, the Librarian.

From this time the growth of the Library, though not rapid, was steady and continuous, and marked at intervals by acquisitions of importance. In the beginning of December 1840, an experiment was made of keeping the Library open in the evening from six to nine o'clock. This did not then prove successful, and was discontinued in June 1841. In May 1843, the autograph signature of Shakespeare, attached to the purchase-deed of a house in Blackfriars, dated March 10, 1612, was bought at a sale in Messrs. Evans's rooms in Pall Mall for £145. This was secured for the Library in the first instance by the Chairman, Mr. Jones, on his personal responsibility, the purchase being afterwards confirmed by the Court of Common Council. The mortgage-deed of the same property, also bearing Shakespeare's signature, and dated the following day, was purchased by the British Museum in 1858 for £315.

Mr. William Herbert resigned the office of Librarian early in 1845, on account of failing health, and on the Committee's recommendation a pension was awarded to him. His name will long live

in connection with his valuable 'History of the Twelve great Livery Companies,' a work which is still the best authority for the extensive subject of which it treats. He was succeeded as Librarian, on February 13, 1845, by Mr. William Turner Alchin. This gentleman has left proofs of his diligence and skill in a valuable subject-index to the catalogue, printed in 1846, a beautifully-written catalogue of the prints and maps, and an interleaved folio copy of the catalogue of books, with additions and press-marks inserted in a very neat hand. In addition to his duties as Librarian, Mr. Alchin was also engaged in indexing the records of the Corporation, and the valuable results of his labours in this department constitute a remarkable monument of his industry and ability.

In 1847 Mr. Philip Salomons presented to the Library a valuable collection of about 400 Hebrew books, for which the special thanks of the Court of Common Council were voted to him. A portion of the munificent bequest, in 1873, of £1000, left by his brother, Alderman Sir David Salomons, Bart., was applied to increasing this Hebrew library, and adding to it a collection of works illustrating the history and present condition of the Jews throughout the world. This was done under the advice of the Rev. Albert Löwy, to whom the Committee entrusted the preparation of a catalogue of the entire collection of Hebrew and Jewish literature. The catalogue was issued in 1891, and has been greatly appreciated for its practical usefulness. It involves a considerable departure from recognised bibliographical axioms, as, instead of presenting an exact transcript of the title-page, which, in the case of Hebrew works, more often hides than reveals the nature of the book, the initial words only of the title have been given, and a concise description of the book supplied in English. This main feature of the catalogue will doubtless be welcomed by students, and Hebraists will appreciate the learning and research displayed by Dr. Löwy in this untrodden field.

A new catalogue of the Library, prepared by the Librarian, Mr. Alchin, was printed in 1859. In the same year the Library received an interesting collection of the writings of Hackney Nonconformist Ministers, mostly Unitarian, which had been formed by the donor, Mr. John Robert Daniel Tyssen. The collection numbers over 1000 volumes, and the authors include Belsham, Burder, Lindsey, Price, Priestley, Wakefield, and many others; the editions of the works of some writers being so numerous as almost to amount to a bibliography.

In July 1863, the custody of the valuable library belonging to the Dutch Church in Austin Friars was offered to, and accepted by, the Corporation. Among the printed books, which number nearly 2000 volumes, are the first printed Dutch Bible, 1477, and Froissart's 'Croniques,' editio princeps, 1495. The manuscripts include a Dutch Bible, in two volumes, dated 1360, having the signatures of the sheets preserved at the end of the second volume, and a fine copy of the Koran, which was used by Sale for the purposes of his translation.

The advantages of removing these valuable works from neglected cupboards in a City church to the shelves of a library where, under proper restrictions, they are freely accessible to all, are self-evident.

Early in 1865, on the death of Mr. Alchin, Mr. William Henry Overall was appointed to succeed him in the office of Librarian, the present writer having some months previously been appointed as Assistant. The staff continued to consist of two officers only until the erection of the new building. In 1867, the collection of maps, prints, and drawings, was arranged and placed in portfolios, and a new catalogue prepared and printed.

Many are the pleasant memories recalled by this old Library in which I spent the first eight years of my official life. The accommodation for readers was cramped and insufficient, the farther of the two rooms provided for their use was badly lighted, the intrusion of museum cases and objects made progress difficult, and on dark winter days even somewhat dangerous; in winter time too, the place was not properly warmed, and was never free from draught. Specially privileged readers were sometimes invited to study in the Committee Room, a large and comfortable apartment over the Guildhall porch, which is still used as a store-room for books. But with all its drawbacks, the old Library was much appreciated by its habitués, most of whom had their regular seats, those within easy reach of the fire being of course most prized. Among the many readers at this period whom my memory recalls, were Professor Henry Morley, the author of 'The History of Bartholomew Fair'; Mr. H. T. Riley, the editor of 'Liber Albus,'; Dr. Munk, the historian of the College of Physicians; Sir Charles Reed, who was twice Chairman of the Committee; a group of London antiquaries, of whom Mr. Charles Roach Smith, Dr. W. H. Black, Mr. Walker Baily, Rev. Thomas Hugo, and Mr. J. G. Waller were the most prominent; and a little knot of students who regularly attended the lectures at Gresham College, which were then delivered in Latin as well as English. Another less distinguished coterie was that formed by several frequenters of the debates held by the Ancient Society of Cogers, who, it must be confessed, did not put in an early appearance, and were not specially distinguished for the neatness and cleanliness of their person and attire.

The time had at length come when the Guildhall Library was to be provided with a home more suitable to its needs and importance, and more favourable to its future growth. The rooms, or rather cupboards, in which its books were stowed away, were considered unworthy of the Library at its establishment, and three years later, in 1831, the Committee were empowered to consider "whether there are any premises attached to, or connected with, the Guildhall which can be converted into a handsome and capacious Library worthy of the Corporation, or any ground similarly situated on which such a Library can be built." This was but the first of a long series of references and plans which came to no result until, in the year 1869,

Dr. William Sedgwick Saunders, then Chairman of the Library Committee, drew a forcible picture of the inconvenience, danger, and discredit of the existing accommodation in a pamphlet, entitled 'The Guildhall Library, its Origin and Progress; being an Appeal to the Corporation of London for its Reconstruction,' a copy of which he sent to each member of the Court of Common Council.

This enthusiastic and well-considered appeal was successful, and the author had the satisfaction of carrying in the Court of Common Council, on July 22, 1869, a motion for the erection of a new Library and Museum at a cost, exclusive of fittings, of £25,000. It was also decided that the Library should be freely opened to the public without ticket or any other formality. The work of superintending the erection of the building was entrusted to a special Committee of thirteen gentlemen, Dr. Saunders being appointed Chairman, and the public opening of the new Library took place on November 5, 1872. The opening ceremony was performed by Lord Selborne, then Lord Chancellor, in the unavoidable absence of H.R.H. the Prince of Wales. A notable collection of engravings, gems, and other works of art, and of antiquarian drawings and prints of old London, was exhibited on the occasion. Upon the close of the exhibition, the building was handed over by the Select Library Committee to the Library Committee, and was ordered by them to be opened for the admission of readers on March 10, 1873.

The straggling series of apartments in which the Library had been hidden away were avowedly only a makeshift, and yet had to suffice for over forty years. The new building, designed by the late Sir Horace Jones, consists of a Library which will accommodate 150 readers, a Newspaper Room for journals and handy-books of reference, and a Committee Room, on the upper floor; with a Museum and strong-rooms in the basement. The old building occupied the site of the corridor which now forms the approach to the present Library from the Guildhall porch. The Committee's confidence in the future development of the institution was not misplaced; the yearly attendance of readers and visitors rose at once from 14,316 in 1868, the last year of the old Library, to 173,559 in 1874, the first complete year of the new. It should be stated that the provision of the new building, as well as its foundation and maintenance to the present day, has been at the sole expense of the Corporation of London, no rate for that purpose having ever been imposed upon the citizens.

In 1873, a great addition was made to the books in the departments of commerce and fine arts out of the bequest of Alderman Sir David Salomons, which I have already mentioned. In the same year the Company of Clockmakers placed their valuable Library and Museum under the custody of the Committee. In 1877, when Sub-Librarian, I was privileged to take part in the International Conference of Librarians held in London. Many valuable papers and sug-

gestions were contributed, more particularly by the gentlemen who formed the deputation from America. One of the results for the Guildhall Library was the formation of a card catalogue, which I designed and completed between 1878 and 1883. Of this catalogue it is only necessary to say here that it is now indispensable to both officers and readers, and is much appreciated by other librarians at home and abroad, judging from the frequent applications which I have received for particulars of its construction. In 1879, a printed catalogue of the Dutch Church Library appeared.

In June 1888, I became Librarian on the death of Mr. Overall, whose health had been failing for some time past. He had been connected with the Library since 1857, and had served the Corporation altogether for forty years. In the following October, a valuable gift was received from the trustees of the British Museum of 5000 volumes and a considerable number of pamphlets from their duplicate stock, which was then being distributed to various public libraries. A bequest of 632 books was received from the executors of Mr. W. T. Wingrove in 1889. In April 1890, the Gaisford sale took place, at which were purchased 239 volumes of early plays and poems by writers of the seventeenth century; many of these books were first editions, several from the presses of early London printers, and all were beautifully bound. Purchases were also made later in the year at the sale of Mr. T. C. Noble, a well-known London antiquary. Other important additions were a selection from the library at Moor Hall, Harlow, Essex, and the presentation of collections of works upon gardening and cookery by the Worshipful Companies of Gardeners and Cooks, respectively.

The manuscript collections relating to the Aldermen of London, made by Mr. J. J. Stocken, were purchased at his death, and at the Gennadius sale important purchases were made of books on art, bibliography, and Byroniana. Valuable books were also secured at the sales of the libraries of Mr. J. Anderson Rose, Mr. R. M. Holborn, and the first and second parts of the sale of the Earl of Ashburnham's Library.

Two acquisitions of special importance must be more particularly mentioned. By the bequest of Dr. Willshire, the well-known authority upon ancient prints, the Corporation became the possessors of his library and collection of prints illustrating the history of the art of engraving from its earliest period. On the death of Mr. Alfred Cock, Q.C., his fine collection of books written by, or relating to, Sir Thomas More and his friend Erasmus, was purchased by subscription and presented to the Library. Special interest attaches to this gift as More was, as I have already mentioned, an officer of the Corporation of London, and in all probability a frequent reader in the old Library of Sir Richard Whittington.

The Guildhall Museum.

I must now turn to the history of the Museum. Within two years of the date of the resolution establishing the second Guildhall Library, the Corporation decided by a resolution on January 19, 1826, to create a Museum of London Antiquities. The time was happily chosen, for a few ensuing years were to witness the commencement of a series of great public works, such as the re-building of London Bridge, the erection of the new Royal Exchange, and the construction of railway termini and great public thoroughfares, which completely transformed ancient London, and brought to light numerous and most interesting remains of former periods. The union of the Library and Museum under one roof from their earliest days has proved a great benefit to both institutions. Works of reference in a Library, and specimens in a Museum, furnish a mutual illustration which can only be appreciated when the books and the antiquities are housed in the same building. There has, however, been a marked but quite natural difference in the development of the sister institutions. The Library, though at first limited to books relating to the City of London, has long since outgrown the restriction. The Museum, on the other hand, has, with some slight exceptions, always maintained a strictly local character. Neither the space nor the funds at the disposal of the Library Committee justified the establishment of a museum of a general character. And even if such an experiment had been made, it would have been wanting in the peculiar charm attaching to a collection which centres upon a single subject. As now arranged, the Museum displays a historic symmetry amid a wide variety of objects, illustrating the fortunes and changes of this ancient City and its people from a remote past to the living present.

The earliest objects contributed to the Museum were some specimens of pottery and other remains, principally of the Roman period, discovered in digging the foundations for the General Post Office and new London Bridge, and during the demolition of Guildhall Chapel. On March 26, 1846, the Committee reported that the ante-room of the Library had been fitted up as a museum, and that a large collection of Roman antiquities, found during the excavations for the new Royal Exchange in 1841, had been presented by the Royal Exchange and Gresham Trusts Committee. These were arranged by Mr. Thomson, Librarian of the London Institution, and a catalogue of them prepared by Mr., afterwards Sir, William Tite, was printed in 1848. In 1850, Mr. H. B. Hanbury Beaufoy, F.R.S., presented a valuable collection of seventeenth century tradesmen's tokens, relating to London, Westminster, and Southwark. The Committee engaged the services of Mr. J. H. Burn to make a catalogue.

It was printed in 1853, and a second edition was published in 1855. This catalogue abounds in anecdotes and entertaining observations on old London inns and their signs, and it was the pioneer of the present extensive literature on this popular topic.

The chief additions during the next ten years were some valuable autographs, transferred in large part from the City records, and forming the nucleus of the present collection. In April 1865, the Library Committee was authorised to purchase for £200 a private collection of Saxon, Roman, and mediæval antiquities brought to light during the fifteen previous years within the City.

The erection of the new Library in 1872, gave a great stimulus to the growth of the Museum, for which ample accommodation was furnished in the basement of the structure. It is much to be regretted that the apartment was placed so far below the level of the roadway, the result being a great deficiency of light. This arrangement was rendered necessary to provide for the use of the apartment for ceremonial purposes; but the inconvenience has, to a large extent, been obviated by the introduction of electric light. The next important acquisitions were the purchase of the extensive museum of London antiquities belonging to the late Mr. Walker Baily, the collections of Mr. J. E. Price, and a valuable series of early English keys formed by Mr. Thomas Wills. Mention must, also, be made of the extremely interesting and valuable group of Roman remains, which were discovered in 1872 on the site of the National Safe Deposit Company's premises opposite the Mansion House, and were presented to the Corporation by the late Metropolitan Board of Works. The same body had previously presented the large Roman pavement found in Bucklersbury in 1869. The demolition of a bastion of London Wall in Camomile Street, Bishopsgate, in 1876, led to the unearthing of large blocks of sculpture, consisting of figures of warriors and animals, and massive architectural fragments of the Roman period, which had been used by Londoners as building material for the repair of their wall in early times. This interesting collection also found its way to the Museum.

In 1892, the Court of Common Council placed £400 at the disposal of the Library Committee for the purchase of a remarkable collection of London antiquities from the earliest period found during the previous seven years by Mr. James Smith of Whitechapel, a working-man, whose devotion to, and knowledge of, archæology, was the subject of public recognition on more than one occasion.

In the mediæval section of the Museum, the collection of old London signs is of great interest, and includes the famous "Boar's Head" from Eastcheap, and the "Cock and Bottle" from Candlewick Street; the latter is of unique interest, being composed of Delft tiles of high artistic merit. The rearrangement of the Museum is now complete and follows the classification in the catalogue which I prepared, and which was printed in the year 1903.

Present Extent of the Library.

As a library grows, there is a great danger that its choicest acquisitions by purchase or gift may become to a certain extent buried and unknown to those to whom they would prove of special value. No catalogue, I am convinced, constitutes a sufficient help in making the special treasures of a library properly known. If the catalogue is alphabetical, collections of books on particular subjects are scattered all over its pages, and individual works of unique interest and value only appear in the general list under the author's name, distinguished by a longer title, or the addition of a special note.

A great need of the present day is the preparation of a catalogue of "notabilia" of our public and private libraries, which would place at the disposal of students much valuable information at present hidden, and for practical purposes almost non-existent. For this useful work we shall probably have to wait, but meanwhile, if librarians would give an account of the principal treasures under their charge, in prefaces to their catalogues, or in addresses on special occasions, the studious public would, I am sure, be very grateful. I make no apology therefore for bringing under your notice a few of the rarities possessed by the Corporation in their Library at the Guildhall. It has been a practice for many years at that Library, when receiving visits from literary institutions and societies, to tell them briefly the story of the Library, and to exhibit examples of its treasures most suitable to the occasion.

Besides the collections and items of special interest already alluded to, the following deserve particular mention. The Library is particularly rich in early London newspapers and directories, genealogical and heraldic works, including parish registers ; broadsides and tracts of the seventeenth and eighteenth centuries ; clock and watch-making ; archæology and costume ; sixteenth and seventeenth century Divinity and Classics ; pamphlets (mostly contemporary) on the tractarian movement ; British topography ; British history and biography ; and the publications of learned Societies. Among the manuscripts are the 'Chronica Franciae,' written in 1399, and beautifully illuminated ; it contains the history of the kings of France from the earliest times, and was known to John Stow, the historian, as "the great French book" : a Dutch Bible, dated 1360, composed of vellum and paper : a copy of the Koran, from which Sale made his English translation : 'Liber Fleetwood,' a collection of City laws and customs, compiled by William Fleetwood, Recorder of London in 1576 ; it contains the arms of the Aldermen of that time beautifully painted : a small Latin Bible on vellum, of the fourteenth century, exquisitely written : Oliver and Mills's 'Survey of London,' after the Great Fire of 1666 : 'Proceedings of the Court of Pie Powder,' held for deciding disputes and granting licences at Bartholomew Fair, 1790–1854.

Among the early printed books may be mentioned Schedel's 'Nuremberg Chronicle,' 1493; Higden's 'Polychronicon,' with illustrated title-page, 1527; and Stow's 'Survey of London,' first edition, 1598. Of additionally-illustrated books the Library possesses many of great interest. They include Lysons' 'Environs of London,' in 13 volumes, with 1000 topographical drawings, nearly 1600 prints, and 1048 illuminated coats-of-arms; Thomson's 'Chronicles of London Bridge,' in 5 volumes, with numerous manuscript, printed, and pictorial additions; 'The Recreations of Master Zigzag the Elder,' by Thomas Archer; the third, or Southwark, volume of Manning and Bray's 'History of Surrey'; Granger's 'Biographical History of England,' 66 volumes with 6000 additional portraits; Allen's 'History of London,' in 17 volumes; Brayley and Brewer's 'History of London and Middlesex,' 21 volumes; and Clarke and McArthur's 'Life of Nelson,' in 5 volumes, with many additional portraits and etchings by G. P. Harding.

Of large illustrated works only one can be mentioned here—the sumptuous illustrated volume issued as a pictorial record of the Coronation of King George IV. Among the examples of special binding are a series of miniature almanacs from 1740 to 1856, published by the Stationers' Company; and royal bindings from the libraries of King Charles II. and Queen Anne.

Books of more general interest include a file of the *Times* from 1811 to the present time, with a complete set of Palmer's index; Ashbee and Halliwell's 'Facsimiles of Shakespeare quartos,' the entire series in 48 volumes; and the illustrated description of the Albert Memorial, presented by the late Queen Victoria, with Her Majesty's autograph. The collection of maps and prints of London and the Suburbs is very large, and includes Ralph Agas's map, of which only one other copy is known to exist; Hollar's rare view of London before the Great Fire, 1647; and a fine copy of the Survey of Ogilby and Morgan in 1677.

My time has almost expired, and yet there are three important considerations which I should like to submit to your judgment, and which deserve a much fuller treatment. I can only state them now, and leave them as subjects for your individual reflection. First, then, my experience convinces me that the resources of a public library, or indeed of literature generally, are not used by the average citizen as they should be. Putting aside the learned, the professional, and the literary classes, people as a rule do not use books, and go their way, laboriously pursuing their crafts or occupations, ignorant of the great help which they might gain from those who have worked at the same problems before them, and have left the result of their labours in a form in which it can be of great benefit to later generations.

Secondly, the great success of the Guildhall Library, and the

benefit it has proved to the people of London, shows the need of similar reference libraries distributed about the suburbs of the great Metropolis. Instead of some thirty-eight library authorities, each establishing out of very limited means a reference library for its own district, a well-considered scheme might surely be devised and submitted for Parliamentary sanction by which, say, six good libraries might be formed, three north and three south of the Thames, which would amply and much more economically supply the needs of the London ratepayers.

Lastly, another want still remains to be supplied, namely, the provision of lending libraries for students, from which, under proper regulations and restrictions, valuable but necessary books of reference could be freely borrowed for home use. My views on these last two important matters were strongly urged in public addresses which I delivered in 1889 and 1894. I only venture to repeat them in your presence because in the Metropolis the rate-supported library system is still in its infancy, and much may still be done to secure its development on more economical and better-considered lines by co-operation—enforced co-operation if necessary—between the various library authorities.

[C. W.]

WEEKLY EVENING MEETING.

Friday, February 1, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Almroth E. Wright, M.D. F.R.S.

The Method of Combating the Bacteria of Disease in the Interior of the Organism.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, February 4, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Hon. George Peel,
Sir David Gill, K.C.B. LL.D. D.Sc. F.R.S.
Stephen Archigenes Ionides, Esq.
Herbert M. Phipson, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Ludwig Mond, Esq., Ph.D. D.Sc. F.R.S., for his Donation of £300, and to Professor Sir James Dewar, M.A. D.Sc. F.R.S., for his Donation of £100, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Chairman reported the decease of The Baroness Burdett-Coutts on the 30th of December, of Miss Agnes M. Clerke on the 20th of January, and of Professor Demetri Ivanovitch Mendeleeff on the 2nd of February, and the following Resolutions passed by the Managers at their Meeting held this day were read and adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record, at this, their first Meeting subsequent to her death, their sense of the loss sustained by the Institution in the decease of the Baroness Burdett-Coutts.

The Baroness Burdett-Coutts was elected a Member of the Royal Institution as far back as the year 1847, and for many years took an active interest in its welfare, being a friend of Professor Faraday and Professor Tyndall.

The Managers desire to offer to Mr. Burdett-Coutts the expression of their most sincere sympathy with him in his bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution in the decease of Miss Agnes M. Clerke, Hon. F.R.A.S.

Miss Clerke was elected a Member of the Royal Institution in 1902. She was awarded the Actonian Prize by the Managers of the Royal Institution in 1893 for her distinguished scientific contribution to Astronomical Literature; and in 1901 she was requested by the Managers to write the first Essay under the Hodgkins Trust, on "Low Temperature Research at the Royal Institution of Great Britain, 1893–1900."

The Managers desire to offer to the family the expression of their most sincere sympathy with them in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the irreparable loss to Science and to the Institution in the

decease of their Honorary Member, Professor Demetri Ivanovitch Mendeleeff, D.C.L. Ph.D. Hon. F.R.S. Hon. F.C.S., Commander of the Legion of Honour.

Professor Mendeleeff delivered the "Faraday Lecture" on "The Periodic Law of the Chemical Elements" in the Lecture Room of the Royal Institution, on the 4th of June, 1889, and on the occasion of the Faraday Centenary Celebration in 1891 he was elected an Honorary Member of the Royal Institution, and attended on that occasion to have the honour conferred upon him.

On his frequent visits to London Professor Mendeleeff invariably attended the Lectures at the Institution and visited the Laboratory.

The Managers desire to offer to the family the expression of their most sincere sympathy with them in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same viz. :—

FROM

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WEEKLY EVENING MEETING.

Friday, February 8, 1907.

The Right Hon. Earl Cathcart, D.L. J.P., Manager, in the Chair.

Professor I. Gollancz. M.A. Litt.D.
Professor of English Literature, King's College, London; Secretary of the British Academy.

Old English Poetry.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, February 15, 1907.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Joseph Jackson Lister, Esq., M.A. F.R.S.

The Foraminifera.

The lecturer proposed to begin his discourse by describing the mode of occurrence of the Foraminifera in nature and something of their way of life and their structure. He would then pass on to consider certain problems which have arisen in the course of their study, and trace the steps by which the solution of one of these problems has been attained and, in the case of another, attempt to show the direction in which the solution appears to lie.

The hollows between the ridges in a stretch of ripple-marked sand are often found to be white with multitudes of shells of minute size and exquisite shapes. If seaweed from shore pools or shallow water is shaken in water over a sieve similar shells are found in the sand which comes through.

[A lantern slide of "floatings" from sand obtained in this manner from Drake's Island, Plymouth Sound, was here shown.]

Microscopically examined these shells are usually found to be made up of separate compartments or chambers, communicating by passages and disposed in some regular plan—plani-spiral, helicoid, alternating on either side of a straight axis, or some other plan. They differ too in texture, being transparent and perforated by minute pores or porcelainous and imperforate. The shells are made of lime contained in an organic basis of "chitin," and grains of sand may be included.

The planispiral chambered shells were classified by the older naturalists in the genus *Nautilus* among Cephalopod mollusca. These were divided by d'Orbigny into *Siphonifères* and *Foraminifères*.

In sand from littoral sea-weed many of the shells will be found to contain the living animal. If slides are set on the sand overnight the animal will crawl on them and they may then be taken out and examined under the microscope; the radiating pseudopodia will be seen, attaching the animal to the substratum and its means of locomotion.

[A diagram of *Polystomella crispa* with extended pseudopodia was here exhibited.]

The hyaline substance forming the pseudopodia is protoplasm. On

dissolving the shell it will be found to be filled by protoplasm ; there are no muscles, brain, stomach, or other organs. All the functions are here, as in Protozoa in general, performed by undifferentiated protoplasm.

The simple character of the soft parts was discovered in 1835 by Dujardin—who pointed out the alliance of Foraminifera, not with Cephalopoda but with *Amœba*, and proposed the name Rhizopoda still in use.

One of the commonest littoral species is *Polystomella crispa*. The shell of this will usually be found to have the following structure.

[A lantern slide showing the shell of this species in lateral and anterior aspects was here shown.]

A biconvex shell, symmetrical about a median plane, keeled, the chambers arranged in a spiral—each convex anteriorly, i.e. towards the terminal face. The alar prolongations – each chamber set astride of the next inner whorl ; thus the last whorl hides all its predecessors. The V-shaped row of terminal pores—the main opening to the exterior. Each chamber has been in its turn the terminal chamber. As we trace them back through the spiral series the number of the canals between the chambers, which open by these pores, becomes smaller, till, as we approach the beginning, there is only a single canal. In specimens of this type a large globular chamber occupies the middle, this is followed by a second chamber of characteristic shape, to which the spiral series succeeds.

The outer walls are traversed by minute pores. There is also a canal system lying in the thickness of the walls.

In the earliest stage the organism consisted of a single chamber, and its shape, at successive stages of growth, is preserved in the interior of the shell.

Foraminifera live from shore pools to great depths, from arctic to tropical waters. A small group is pelagic, living in surface waters, down to at least 500 fms.

Their empty shells constitute the bulk of the " Globigerina ooze," which forms the floor of the ocean in many tropical and temperate regions.

[Two lantern slides were here displayed, showing the character of floatings from the ooze obtained by H.M.S. 'Challenger,' east of the Crozet Islands, in 1375 fms., and north of Ascension Islands in 2350 fms. respectively. The former mainly consisted of *Globigerina*, the latter largely of *Pulvinulina*, and attention was called to the eroded condition of the shells in the latter, a characteristic of the ooze from depths approaching 2400 fms., beyond which all calcareous organisms are dissolved.]

Foraminifera abound in many geological deposits, back to the Palæozoic period. We thus have an added dimension in which to project our ideas of their evolution.

The recent advances in our knowledge of their life history began

with the study of the fossil nummulites, from the later Eocene rocks —called nummulites from their likeness to coins. The nummulitic limestones extend from the Pyrenees to China—they are often thousands of feet thick. They form large deposits in Egypt, where the coin-like discs have attracted notice from remote antiquity.

[Some examples displayed in the library of *N. gizehensis* from the Fayum and of Algerian nummulitic limestones were here alluded to.]

The structure of a nummulite is very like that of *Polystomella*, but the whorls much more numerous, and the main space of each chamber is in the median plane, hence they readily break in this plane exposing a section of all the chambers.

It has long been recognised that in a nummulitic deposit a few specimens far exceed the others in size.

[A lantern slide was here displayed showing a specimen of nummulitic limestone from the Nile valley.]

In median sections of the small specimens the spiral series of chambers is seen to start from a large and nearly spherical chamber, readily visible to the naked eye, while in the few large specimens the spiral series is continued to the centre, where in carefully prepared sections it may be found to take its origin in a central chamber of microscopic size.

[A diagram showing the central regions of the two kinds of shell was here displayed.]

Although the two forms were found associated and to agree closely except in size and in the characters of the central chambers, they were regarded as belonging to different species, and attention was called to this puzzling "association of species of nummulites in pairs," a large and small one.

De Hantken and de la Harpe brought this phenomenon to light, the latter formulating his "Law of the Association of Species in Pairs," as follows: "Nummulites appear in couples, each couple is formed of two species of the same zoological group, and of unequal size. The large species is without a central chamber (*sic*), the small always has one." Sixteen pairs of "species" were enumerated.

In 1880 Munier-Chalmas propounded his view that the associated kinds were not of distinct species, but two forms of the same species, in fact that the species of nummulites were dimorphic. He expressed the view that this would be found to be general in the Foraminifera.

[A lantern slide showing the two forms of *N. lævigatus* from Selsey Bill was here displayed.]

Munier-Chalmas and Schlumberger, examining shells of *Miliolidæ*, found that here too the species are dimorphic, the dimorphism finding its expression in differing arrangements of the chambers, as well as in sizes of central chambers and of the whole shells.

It is, however, not always or even usually the case that the size attained by the shells of the two forms is different.

[A lantern slide showing the two forms of *N. variolarius* from the

Isle of Wight, in which they are of nearly the same size, was here displayed.]

The names *megalosphere* and *microsphere* have been given to the large and small initial chambers respectively, and the two forms are known as *megalospheric* and *microspheric*.

The examination of other groups of Foraminifera has abundantly confirmed Munier-Chalmas' view as to the general presence of dimorphism.

We now turn from fossils to the life-history of *Polystomella crispa* and inquire what light we may gain from it on the significance of the phenomenon of dimorphism.

In a large batch of *Polystomellas*, killed by a reagent which dissolves the calcareous shell, it will be found that megalospheric and microspheric individuals occur in the batch, as among the nummulites of the Eocene strata.

On staining them, another point of difference between them becomes apparent. The megalospheric form is uninucleate, the microspheric form multinucleate.

[Diagrams of the megalospheric and microspheric forms of *Polystomella crispa*, with the shell removed and nuclei stained, were here shown.]

During the growth of the microspheric form, the nuclei multiply by simple division. They also give off small irregular strands into the protoplasm. These have been called *chromidia*. Eventually, prior to reproduction, all the nuclear material appears to be resolved into chromidia.

In a culture of *Polystomella* it is common to find a mode of reproduction which on examination will be found to be that of the microspheric form. It is best followed when occurring in a specimen attached to a glass slide. Such specimens are, in the early phases, distinguished by a great increase in the number of pseudopodia issuing from the shell, so that the latter appears when seen by transmitted light to be surrounded by a milky halo. The protoplasm gradually emerges from the shell until, after some hours, the whole of it has come out and lies massed between the shell and the supporting surface and within the area formerly covered by the halo. The internal protoplasm is darkly coloured with brown granules, and the whole mass is during this time the seat of involved streaming movements. Clear spots make their appearance, and gradually the protoplasm collects about these, and separates into as many spherical masses, which remain connected by a felt of hyaline pseudopodia. Some 200 is a common number to be found. Not long after they have become distinct it may be noticed that each attains a shining coat—the indication that a shell has been formed, a small aperture being left in each for the passage of the pseudopodia. After lying in close contact for some hours, the spheres rapidly and simultaneously draw apart from one another, and within half an hour from the

beginning of the movement they are dispersed over a wide area, and each becomes the centre of a system of a pseudopodia of its own.

The whole of the protoplasm of the parent is used up in the formation of the brood of young, the shell being left empty. The process, from the first appearence of the halo to the dispersal of the young, is complete in about twelve hours.

In a short time the protoplasm which lies outside the aperture of each of the spheres secretes the wall of a second chamber of characteristic shape, and the young individual is then clearly recognisable in size and shape as the two-chambered young of the megalospheric form. Each of the spheres was, in fact, a megalosphere. The microspheric parent has given rise to, indeed it has become, a brood of megalospheric young.

[A series of 12 lantern slides was shown, illustrating the mode of reproduction of the microspheric form, and the dispersal of the megalospheric young. They were taken from a series of photographs of the successive stages of reproduction of one individual.]

As the young megalospheric form grows and the number of chambers increases, the single nucleus, which originally lay in the megalosphere, moves on through the chambers, becoming constricted as it passes from one to another. It also gives off chromidia into the protoplasm, and eventually as the reproductive phase of the megalospheric form approaches, the nucleus loses its compact shape and staining power, and finally disappears. Hosts of minute chromidia may then be found scattered through the protoplasm. These become aggregated as distinct nuclei, the protoplasm gathers about them, and they divide by karyokinesis. Then follows a second karyokinetic division, and, the protoplasm having divided correspondingly, the whole contents of the megalospheric shell emerges as a multitude of minute biflagellate zoospores some 4 μ in diameter.

This was the stage which had been reached in the study of the life-history up to about 4 years ago. The formation of the zoospores had been observed, but the fate which befell them remained a matter of inference.

The evidence pointed strongly in the direction of the view that the foraminiferal life-history consists of an alternation of generations. While the megalospheric form would, on this hypothesis, arise by a simple vegetative asexual reproduction of the microspheric parent, many considerations seemed to indicate the probability that the microsphere, the initial chamber of the microspheric form, arose by the conjugation of zoospores. In addition to the general probability of the occurrence of a sexual stage somewhere in the life-history, the sizes of zoospore and microsphere fitted in with the view that the latter might be formed by the coalescence of two of the former. Again, the fact of the rarity of the microspheric form in comparison with the megalospheric was comprehensible, on the supposition that, to be able to conjugate, the zoospores must be of different parentage.

The point remained, however, a matter of inference until Schaudinn published an account of the processes that he had observed, turning inference into certainty.

(It was only last June that we had to deplore the death of this most brilliant investigator.)

[The lecturer then read a translation of a passage from Schaudinn's paper,* in which the conjugation of zoospores, which there is good reason to believe arose from different megalospheric parents, was described. The growth of the resulting zygote (microsphere) was observed by Schaudinn, until the little microspheric individual had attained the five-chambered stage.]

We are then, at last, able to give with confidence an answer to the question, What is the significance of the phenomenon of dimorphism in the Foraminifera? The answer is, It results from the occurrence of two modes of reproduction in the life-history, sexual and asexual. The megalospheric form is the product of asexual reproduction, the microspheric form arises from the conjugation of two similar zoospores, produced by individuals of the megalospheric form. The Foraminifera thus fall into line with many other groups of Protozoa, in which a similar alternation of generations has been found.

Attention was then directed to another remarkable phenomenon presented by the shells of the Foraminifera—the *multiform* condition.

In considering *Polystomella* and the nummulites we have been dealing with forms in which the arrangement of the successive chambers is uniform throughout growth.

What is the controlling mechanism by which this uniform result is brought about? We have no evidence which will even suggest an answer. The nucleus we know is an essential factor in the construction of the chamber wall, it in some way dominates the constructive properties of the protozoan body, but the nuclear and protoplasmic characters of many groups of Foraminifera are to all appearance alike, yet they build their shells on widely different plans.

But, leaving the problem of the nature of the controlling mechanism as at present insoluble, to return to its *effects* we find that in several groups of Foraminifera, the shells are not built on the same plan throughout, but that two or even three laws of growth have controlled the building of the shell at successive periods.

[A lantern slide displaying the form of the shell in *Spiroplecta*, *Bigenerina* and *Clavulina* was here shown.]

Thus, among the Textularidæ, the genus *Spiroplecta* is characterised by a planispiral arrangement during the earlier stage of growth, but later in life the shell is formed of alternating chambers

* Untersuchungen üb. d. Fortpflanzung einiger Rhizopoden. Arb. aus Kais. Gesundheitsamte. Bd. xix., Heft 3, 1903.

disposed about a rectilinear axis. Growth is as regular as before, but the plan is changed. The genera *Bigenerina* and *Clavulina* afford similar examples. Among the *Miliolidæ* the closely connected series of genera *Peneroplis*, *Orbiculina*, *Orbitolites* furnishes an instructive instance.

[A lantern slide displaying the form and structure of *Peneroplis* was here shown.]

The test of *Peneroplis* is formed of a series of simple chambers disposed in a spiral at first but tending to straighten out in the later stages.

[A lantern slide displaying the form of several varieties of *Orbiculina* was here shown.]

In *Orbiculina* the plan is similar at first, but the chambers soon become subdivided, and also widen out laterally, either on one side of a straight axis or on both. If on one side only we have the characteristic *hooked* shape of shell from which the name *O. adunca* of this highly varying species is derived. If on both sides, the prolonged ends of the widening chambers soon meet one another, and the shell assumes in its later stages the annular arrangement. In *Orbitolites*, this latter is the characteristic mode of growth, though here too the annular plan is preceded by some traces of spiral and rectilinear arrangements.

[Two lantern slides showing the plans of growth of *Orbitolites marginalis* and *duplex* were here exhibited.]

In the more highly developed species of this genus, moreover, the chambers increase in thickness, as growth proceeds, in a direction at right angles to the plane of the flat shell, and we have the biconcave discs of *O. complanata*.

[Two lantern slides showing the plans of growth of *O. complanata* were here exhibited.]

These instances will suffice to illustrate the remarkable phenomenon of multiform tests of foraminifera, and we may now inquire—What light have we on its significance?

The assumption in the early stages of life of a type of structure different from that of the adult is a phenomenon widely met with among the higher groups of animals. We know that the throat of a chick of the third day is perforated by lateral slits—gill clefts, and that the arterial trunks between them are disposed for all the world as though the chick were to grow up not an air-, but a water-breather. What is true of the young bird holds true likewise in the Mammalia, including ourselves.

To take an instance from the Invertebrata: the appendages of a scorpion correspond limb for limb with those of the king-crab *Limulus*, except that one pair about the middle of the series is absent in the scorpion. It has been shown by Brauer that at a certain stage of development of the scorpion's egg, this pair of appendages is represented by a pair of little buds, and that there is a pair of ganglionic

centres and a body segment corresponding. These are, however, transient features, which are entirely obliterated as development proceeds.

The only view that appears to deal adequately with these phenomena is that which recognises in the appearance of these transient fish-like and limuloid features in the development respectively of bird and scorpion, the repetition of characters, which in some remotely distant age of the world were those of their adult ancestors.

We must, however, proceed with the caution. The varied groups of the decapod crustacea present in their development larval stages differing widely from one another and from the adults. Yet, if we are able in any case to form an estimate of phylogenetic relationships from adult structure, it is certain that the decapod crustacea are a natural group descended from a common ancestor. We have to conclude that the strikingly different features of the larvae are in many cases not ancestral, but adaptive features fitting them for diverse conditions in the pelagic existence through which they pass before settling down as littoral forms and acquiring the adult state.

It appears then that a stage in early life different from that of the adult may be due to the repetition of ancestral features, but that it may on the other hand be an adaptation fitting the young organism to special conditions.

To which category are we to refer the modes of growth of the young shell of the multiform foraminifera ?

With regard to the series of forms included in the genera *Peneroplis*, *Orbiculina* and *Orbitolites* Carpenter definitely held the view that the modes of growth adopted in early life by the more complex members of the series are phylogenetic repetitions of the arrangements of the simpler forms of the series, from which they had developed—that they are in fact repetitions in ontogeny of the phylogenetic history.

Carpenter's view would, the lecturer believed, more and more be found to be correct.

Another view of the significance of the multiform character of the shells of Foraminifera has been propounded of recent years by Professor Rhumbler, and a scheme of classification based on it has become current, and found its way into text-books. The early stages of the shells are here also regarded as of high phylogenetic significance, but in a sense precisely the reverse of that of Carpenter. Professor Rhumbler regards the early arrangements of the chambers as representing not an ancestral type, but one towards which the race is advancing, while later in life the organism falls back on the old bad ways of its forefathers. He thus regards the multiform shells as "phylogenetically degenerating" in the course of growth. The lecturer expressed his dissent from this view.

Two more examples of the multiform condition might be considered.

[Two lantern slides were then displayed showing the whole test of a specimen of *Orbitolites tenuissima*, and an enlarged view of its central chambers.]

Orbitolites tenuissima is a fragile shell occurring at great depths. The peripheral parts agree with those of *O. marginalis*, but the central region is built on an entirely different plan—resembling that of the Miliolid genus *Ophthalmidium*.

On the completion of the latter an astonishing change in the mode of growth occurs, the shell being henceforth built on an entirely new plan. One can hardly suppose that such a sudden abrupt mutation befell the race, as is here recorded, and it would appear that some other, though entirely unknown factor or factors, have contributed to the result.

The second case is that of *Polytrema miniaceum*, a pink encrusting arborescent form, often seen attached to corals, polyzoon stocks, and other objects from warm shallow waters.

[A diagram of *Polytrema miniaceum*, with enlarged drawings of the initial chambers, and a lantern slide exhibiting a section of *Polytrema*, passing through the group of rotaloid chambers were exhibited.]

In its mode of growth and the absence of any distinct chambers throughout the greater part of its mass *Polytrema* differs widely from most other Foraminifera, and it was referred to various groups of animals by the earlier naturalists. By Pallas and Gmelin, it was placed near the genus Millepora, which is now included, with corals and hydroids, in the Cœlenterata. Its true position was recognised by the discovery that its growth begins by the formation of a small spiral group of chambers with thick walls and coarse perforations such as are characteristic of the foraminiferan family *Rotalidæ*. In this case at any rate, there can be no question that the plan on which the first formed chambers are laid down is that of the stock from which *Polytrema* sprang.

In conclusion, the lecturer alluded to the relation in size in the genus *Nummulites* between the megalosphere, and the volume of the protoplasm of the microspheric parent, by the division of which it arose, in the manner described. He also pointed out the analogy presented by the life-history of those species of nummulites (e.g. *N. complanatus*), in which the microspheric form far exceeds the megalospheric in size, to the life-history of a fern, in which the prothallus is likewise dwarfed by the fern plant—the individual of the sexually produced sporophytic generation.

[J. J. L.]

WEEKLY EVENING MEETING,

Friday, February 22, 1907.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Dugald Clerk, Esq., M.Inst.C.E. F.C.S. *M.R.I.*

Flame in Gas and Petrol Motors.

[Experimentally Illustrated.]

Flame produced by the combustion of inflammable gas, or vapour and atmospheric air, forms the working fluid of gas or petrol engines. Such engines differ from other heat engines, as steam and hot air engines, in that the necessary heat is evolved within the working fluid by combustion, instead of added to the working fluid by conduction through metallic walls. In all practicable heat engines the source of heat is the same—the combustion of fuel; but in one type heat is evolved within the working fluid, while in the other type heat is applied externally, and passes to the working fluid by conduction.

Mechanical power can be obtained by means of flame in several different methods.

1. By filling a vessel or cylinder with a mixture of gas and air, and igniting the mixture, a slight explosion is caused, and the excess pressure blows off through a non-return valve. The temperature of the flame is very high, and so, when it cools, the pressure in the vessel is reduced below atmosphere. This reduction of pressure may be utilised by means of an engine operating by atmospheric pressure and discharging into the partly vacuous vessel, or by a piston moving into the vacuous vessel. This method may be called the explosion vacuum method, and is illustrated in Fig. 1. I have here a tubular glass vessel A, which I have filled with a mixture of coal gas and air. It is supplied with an automatic escape valve B, and it has an electric igniting device D at the lower end. The vessel is charged by means of a small gas-measuring syringe, and it is now ready for the experiment. A pipe from the model engine C on the table, which you see, connects to the vessel, and immediately on passing the spark, you observe the flash of flame through the vessel, hear the slight explosion, and notice that the little engine at once starts, driven by atmospheric air finding its way through the engine back to the partially vacuous chamber. You notice that on passing the spark

what I have described happens, and the little engine at once rotates rapidly. This method may be applied to evacuate a larger vessel than that shown on the model, by using an intermediate chamber into which the gases blow through from the explosion chamber. By this method it will be observed although flame forms the real working fluid, yet the actual operating fluid driving the piston of the small engine is atmospheric air.

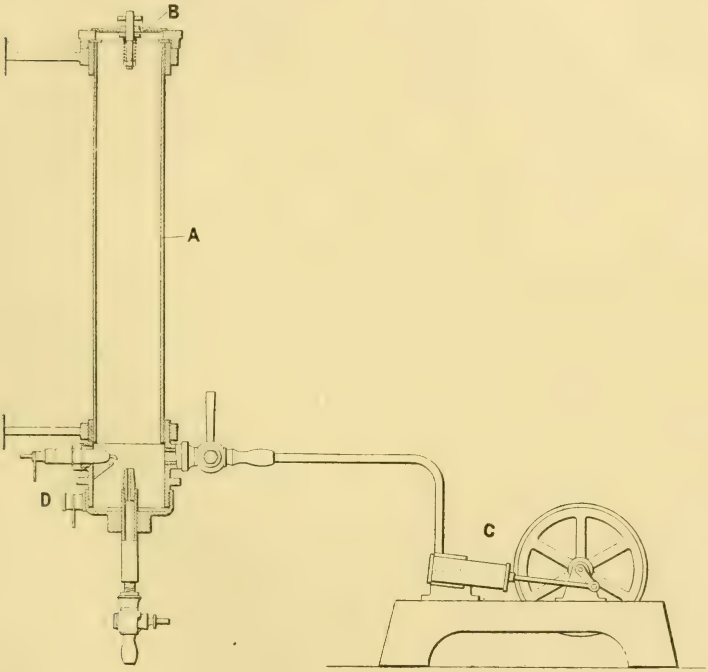

Fig. 1.

A modification of this method, shown in Fig. 2, enables a partial vacuum to be produced without the aid of preliminary explosion. I have here a small engine known as the Loan engine, which is the sole modern survivor of the vacuum method. In it a piston draws in a flame through a valve ; the flame being produced by gas, the valve closes, and the piston continues the expansion of the flame in its cylinder ; the flame cools, and on the return stroke the piston is forced back under atmospheric pressure from without. Here I have the engine. You see the flame at the upper part of the cylinder. On turning the wheel the flame is sucked into the cylinder, and you

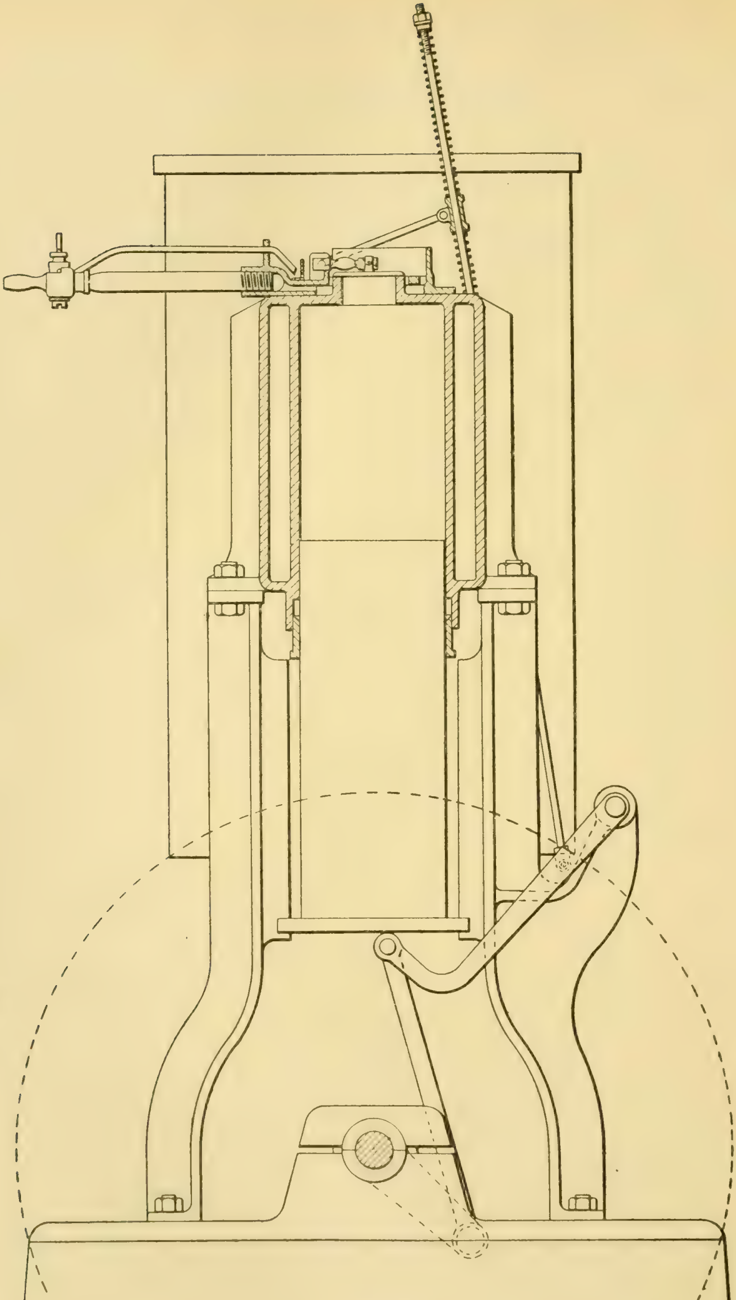

Fig. 2.

observe that the engine at once rotates. The power obtained, it is true, is small for the gas consumption; but here you have an actual example of an engine operating with flame as its working fluid at atmospheric pressure. This modification may be called the flame vacuum method.

2. By admitting a charge of atmospheric air and inflammable gas or vapour at atmospheric pressure to a cylinder containing a piston, cutting off access to the atmosphere and the gas supply, and igniting the mixed charge, a mild explosion occurs; the pressure rises in the cylinder, and the piston is driven forward to the end of its stroke. I have here an apparatus to illustrate this effect. This apparatus, shown in Fig. 3, consists of a petrol engine cylinder A fitted with a piston B in the usual way. This piston, however, carries a piston rod C, having at its upper end a long key D operating in a slot in a tubular pillar E mounted over the open end of the cylinder, and held firmly by flanges and bolts. The upward movement of the piston is buffered by rubber rings F placed within it. The downward movement of the key is buffered by rubber rings G covered by a metal plate surrounding the tubular pillar. A circular cast-iron weight H of about 100 lb., slides upon the tubular pillar, and rests upon the key in the piston rod. The weight can be hoisted up out of the way, as you see, by means of a small block and tackle. The inlet valve of the petrol engine is used to admit a mixture of air and gas. The gas is carefully measured in under the valve by means of a separate measuring pump, and electric igniting gear is arranged as in the usual petrol engine.

I have here a stop, which permits the piston to be raised through half its stroke. Raising it then through half its stroke, and taking in a charge of air and gas in measured proportions, I then place a stop K underneath the key, and lower the weight on the top of it. The mixture within the cylinder is then at atmospheric pressure, and the weight is not allowed to move the piston to compress the contents of the cylinder. On pressing down the igniter key, you observe that the 100 lb. weight is thrown up along the tubular pillar, and it rises about 10 inches, and then falls again. This 10-inch rise gives a measure of the energy of the explosion and the expansion during the half stroke of the piston. It is obvious that power can be obtained from an inflammable mixture treated in this way.

3. By supplying to a cylinder containing a piston a mixture of inflammable gas and air in a compressed state, and then igniting that mixture, motive power can be obtained. Using the same apparatus as I have described under the second method (Fig. 3), I charge the cylinder with exactly the same charge of gas and air. Instead, however, of supporting the piston by means of the stop used in the last experiment, I remove this stop and allow the 100 lb. weight to rest upon the piston rod, thus compressing the charge within the cylinder. This particular cylinder is $3\frac{3}{4}$ inches in diameter, so that the addition of

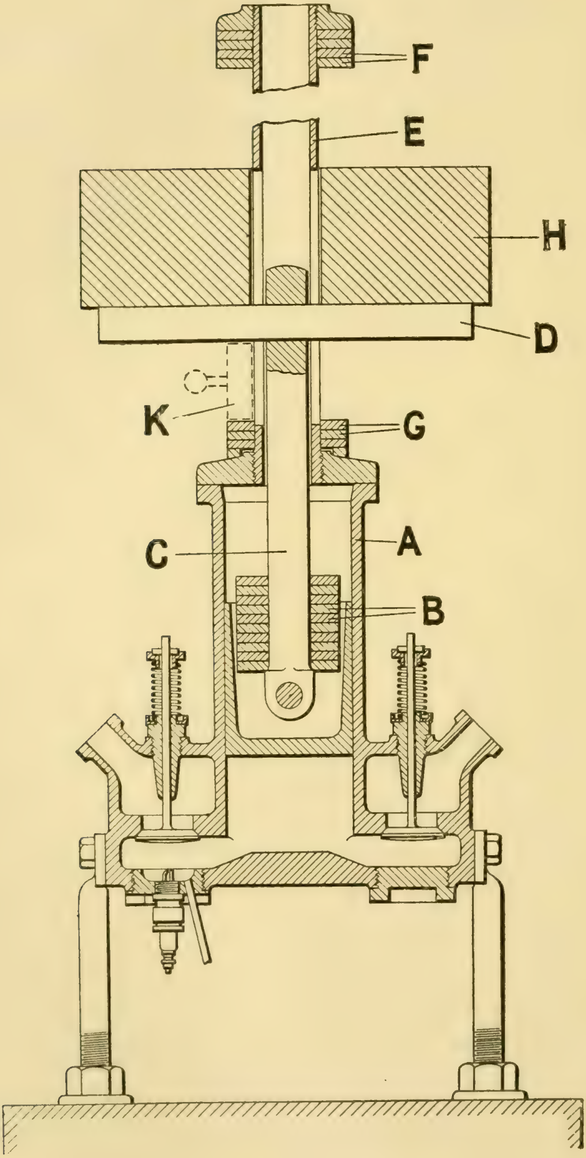

Fig. 3.

100 lb. weight only compresses the mixture to something like 10 lb. on the square inch. On pressing the igniting key, however, you observe that the explosion is much more powerful, and the weight is thrown about 18 inches, instead of 10 inches. This shows quite clearly that for the same weight of gas and air charge introduced into the same cylinder, a compressed charge gives more power than the same charge without compression. It is obvious, therefore, that this method supplies a means of obtaining greater power for a given volume and weight of gas and air. In making this experiment it is necessary to be cautious not to bring too much pressure to bear upon the mixture. Otherwise the 100 lb. weight would get too much energy, and probably damage the apparatus.

These two last methods are commonly known as the non-compression method and the compression method of operation in gas and petrol engines.

4. A cylinder is supplied with gas and air under pressure; but the mixture is ignited at a grating or gauze as it enters the cylinder, and so the pressure in the cylinder never rises above the pressure at which it is supplied. The power here is obtained without any increase in pressure, and is due to the fact that a small volume of cool mixture, when inflamed, increases in volume; so that although a pump may be used to compress mixture, the expansion in the motor side is greater, although at the same pressure as the pressure in the pump. I have here an apparatus to illustrate this action, Fig. 4. It consists of a glass cylinder A having in it a piston B, which you see. At the lower end of this glass cylinder are arranged two perforated brass discs C, and between these discs is placed wire gauze. An electric igniting plug D is placed immediately above the discs, the spark is passed continuously, and a pump E, which you see connected up to the glass cylinder, supplies mixture of gas and air through the grating, igniting at the electric spark, and expanding to force up the piston in the glass cylinder. You will observe that upon passing the spark and forcing down the pump piston, a flame appears in the glass cylinder, which pushes the piston through its range, a small movement of the pump piston resulting in a large movement of the motor piston. In this apparatus power is obtained without explosion. The flame serves the purpose of increasing the volume of the fluid supplied under pressure. Here the volume is increased, and not the pressure.

These four modes of action, and combinations or modifications of them, include all the fundamental methods used in obtaining motive power from flame, which have been attempted by mankind for the last hundred years. In the early part of last century, the vacuum methods were naturally the favourite methods of attempted operation. In the year 1820 the Rev. W. Cecil, M.A., of Cambridge, read a paper at the Cambridge Philosophical Society with the following title, "On the Application of Hydrogen Gas to Produce a Moving Power in Machinery, with a Description of an Engine which

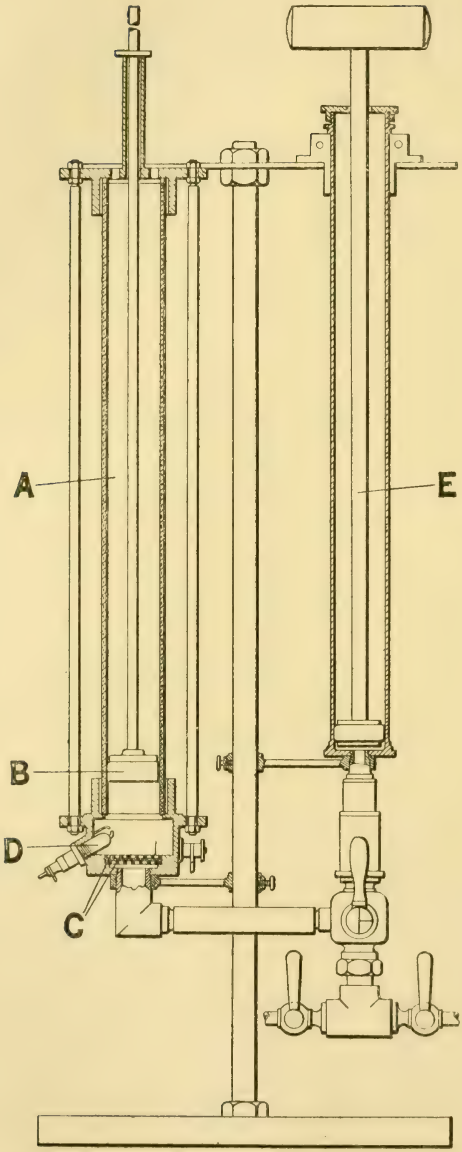

Fig. 4.

is moved by the Pressure of the Atmosphere upon a Vacuum caused by Explosions of Hydrogen Gas and Atmospheric Air." In this paper he described an engine which he had constructed, to operate according to the explosion vacuum method; and he stated that at 60 revolutions per minute the explosions take place with perfect regularity. His engine consumed, he stated, 17·6 cubic feet of hydrogen gas per hour. His hydrogen explosion appears to have been accompanied by considerable noise, because he states with regard to a proposed larger engine, ". . . to remedy the noise which is occasioned by the explosion, the lower end of the cylinder A, B, C, D may be buried in a well; or it may be enclosed in a large air-tight vessel." In this paper he also mentions an engine operated in accordance with the second method, the non-compression explosion method, and one also operated by gunpowder. This paper gives an account of the first gas engine which appears to have been worked in Britain, and, I believe, in the world.

Six years after, in the year 1826, Samuel Brown invented and built an ingenious engine, depending on the flame vacuum method, which appears to have been the earliest gas engine ever worked on any considerable scale. In an early number of the *Mechanic's Magazine*, it is stated that Brown succeeded with his engine in propelling a boat upon the Thames, and in actuating a road locomotive. This vacuum method, however, never really produced a thoroughly commercial engine, and its only survival is found in the small engine here which I have upon the table (illustrated in Fig. 2).

Many engines have been built using the atmospheric, or as it is more commonly known, the non-compression explosion principle, but the most successful was that of Lenoir. I have here a slide of a Lenoir engine of about half horse-power, which was set up at Petworth House, Petworth, over forty years ago. It was working at Petworth House when I inspected it twenty-two years ago, pumping water; but it is now replaced by three modern gas engines of about 80 horse-power each, which serve for electric light and every other power purpose required. The simplest engine of this type was one which was used in considerable numbers until a comparatively recent date—the Bischoff engine. In it a mixture of gas and air is drawn into the cylinder through suitable valves. As the piston passes an igniting aperture the flame is sucked in, the mixture ignites, and a small check valve closes the flame or touch-hole aperture. In the Lenoir engine many of the modern characteristics are found, such as the water-jacket, and ignition by the electric spark. The gas consumption, however, of all these engines was very high, rather over 90 cubic feet per indicated horse-power per hour. The power obtained for given dimensions, too, was very small.

The first and second methods, accordingly are not now used. Their disadvantages proved too great.

In all modern gas or petrol engines, the third method is used—

that is, the charge of inflammable mixture is compressed before ignition.

Many attempts to construct engines operating on the compression principle were made before success was obtained. In such attempts England had a full share. One of the very earliest feasible compression gas engines was that described by William Barnett, an Englishman, in the year 1838. This engine had many of the features of successful engines of to-day. Later proposals were made for similar engines, both in France and Germany; but the first inventor to succeed in overcoming difficulties to a sufficient extent to produce a commercial engine was the late Dr. Otto, of Deutz. To Dr. Otto belongs the honour of producing the first successful compression gas engine. The great majority of modern gas and petrol engines operate on what is now known as the Otto cycle. The combustion of a compressed charge in a motor cylinder in a safe, quiet, and economical manner, is a much more difficult problem than appears at first sight. Those of us upon whom fell the brunt of working out this problem about thirty years ago, appreciate fully the ability and knowledge displayed by the late Dr. Otto in producing his famous engine. In the Otto engine the characteristic feature is found in the alternate use of the same piston and cylinder for the purpose of pump and motor. In one complete revolution the cylinder is used as a pump, and another complete revolution as a motor. The cycle is very simple. It is so well known that I need hardly state it. On one outstroke a charge of gas and air is taken into the cylinder. On the next instroke the charge is compressed into a space at the end of the cylinder; on the second outstroke explosion and expansion occur; and on the second instroke the exhaust gases are discharged. One piston and cylinder thus serves to alternately supply compressed mixture, and use that compressed mixture for the purpose of motive power. It takes, however, four strokes, or two revolutions of the engine, to complete its cycle. I have here a section showing the operation of the original Otto engine. The engine has been greatly improved in recent years, and the Otto cycle was applied by Daimler, one of Otto's former managers, to the purpose of the petrol engine. The modern petrol engine has been developed from the original Daimler invention. I have here a Lanchester petrol engine in section, and can show you the operation of the piston and valves performing its cycle.

The Otto cycle has many great advantages. The charging and discharging of the gases is accomplished easily. The heat-flow through the sides of the cylinder is not too continuous, and consequently the cycle can be operated at very high speeds indeed. Many attempts, however, have been made to obviate the main disadvantage of the Otto cycle, that is, the necessity for two complete revolutions for every power impulse, and I have devoted much time and experiment with this end in view. In 1881, twenty-six years ago, I

invented a cycle of operations, which gave in the same cylinder, one power impulse at each revolution. This cycle is now known as the Clerk cycle, and it comes next to the Otto cycle in order of number of engines now running in the world. I have here two sections, showing the operation of the Clerk cycle. Its characteristic consists of open ports at the out end of the stroke, which are over-run by the piston. The pressure in the cylinder rapidly falls to atmosphere, and a charge is forced into the motor cylinder at low pressure, from a low pressure pump. or displacer cylinder. This displaces the exhaust products remaining in the cylinder, and furnishes the fresh charge, which is compressed on the return stroke into a space at the end of the cylinder. This charge is ignited, and in this way a power impulse is obtained for every forward stroke of the piston. A second cylinder is required, in order to supply the charge. The second cylinder, however, is very light in construction, both as to the cylinder itself, the piston, and the connecting rod and cranks driving it.

I have here a small engine, built to my designs on the Clerk cycle twenty-four years ago. The German authorities requested me to present it to the Munich Museum, to form a link in the history of the internal combustion motor, and I did so some years ago, but the authorities there have kindly lent it to me for the purpose of this lecture. The engine is sectioned, so that I can show you the movements and action of the two pistons.

In the early days of these engines, great difficulty was found in obtaining regular consecutive ignitions at a sufficiently rapid rate. Although Lenoir used the electric spark for ignition, it was not then in a sufficiently reliable form to produce a good commercial engine.

The ignition device is a most important detail, which proved too much for many of the earlier inventors. Whatever may be the skill and energy devoted to the construction and cycle of the engine, it remains a useless mass of metal, requiring power to move itself, rather than furnishing power to set other machines in motion, until an effective ignition is found. The early Otto engines had a flame igniter, of which I show two sections. That ignition depended on the principle of filling a pocket in a valve-port with coal gas, igniting a lower surface of that gas in contact with air in the lower part of the port, and carrying the flickering flame thus produced quickly from communication with the external atmosphere, where it was lighted, to communication with the interior of the cylinder. I have here a little experiment, which shows at once the nature of the Otto flame igniter. Here is a spirit lamp, and here a gas jet. I light the spirit lamp, take the glass cap, place it over the jet, allow the gas to stream in to fill the cap, move the cap over, ignite it at the spirit lamp, and pass it back, when you see that the gas jet can be ignited by the flame carried by the cap, although I have placed the lamp and the jet nearly twelve inches apart. The Otto flame

igniter served admirably for the Otto cycle engine, igniting in usual cases about 80 to 100 times per minute. Where, however, ignition has to be more frequent, as in engines constructed in accordance with the Clerk cycle, the Otto flame was too slow. It required too much time for operation. Accordingly, I produced a modified flame igniter, which I show in section. This igniter depended upon supplying a port with inflammable mixture from the cylinder itself; the gases, being already mixed, contained within themselves oxygen sufficient to support combustion, and accordingly the flame filling the port in this way can be very rapidly ignited, and is quite equal to at least 300 ignitions per minute. This slide was much used in Clerk cycle engines for many years.

These flame igniters gave way to the hot tube igniter about twenty years ago, and now electric igniters have been rendered relatively so perfect that they are rapidly displacing all hot tube devices, even in stationary engines. Petrol engines, as you know, very soon passed from tube ignition to electric ignition.

The regularity and certainty of the various operations: charging, compressing, igniting, expanding and exhausting, in an ordinary stationary gas engine, is marvellous, when the difficulties which have been faced and overcome are considered. The ordinary gas engine of medium size rotates at about 180 revolutions per minute, that is, three revolutions per second. The forward stroke is thus completed in a sixth of a second, that is, the intensely hot gases are produced in the cylinder, the pressure attains its maximum, and expansion occurs for the whole power operation in one-sixth of a second. Naturally, ignition must take less time, and in these engines we find the ignition of the mixture from the beginning of the explosion to the attainment of maximum pressure takes from one-twentieth to one-thirtieth of a second. It is surprising enough to be able to accomplish these operations with such certainty at these speeds, but the experience with the petrol engine is still more surprising. A petrol engine, running at the very common speed of 1200 revolutions per minute, takes but one-twentieth of a second per revolution, that is, one-fortieth of a second for the power stroke. In these engines, maximum pressure is attained very quickly, the explosion, or pressure rise period taking from one-eightieth to one-hundredth of a second. It is interesting to consider that in an ordinary four cylinder car, with the engine running at 1200 revolutions per minute, we get 2400 explosions per minute, the whole of the operations of a complete revolution being performed in the infinitesimal time of one-twentieth of a second.

These considerations show what an advance has been made in handling flame as the working fluid during the last thirty years. It has taken more than a hundred years to attain our present knowledge of the means of handling and controlling flame as the working fluid in these engines; but the last thirty years have seen great develop-

ment, so far as practical matters are concerned; so that now, over two million horse-power of stationary gas engines operated by flame are in use in the world. It is difficult to form an estimate of the power of motor car engines in use, but probably it now exceeds a million horse-power.

Although great progress has been made in the practical control and utilisation of flame and gaseous explosions, for the purpose of producing motive power, little is as yet known as to the actual properties of the flame working fluid so utilised. In this branch of work science so far has made but little progress, and our knowledge of the properties of air and other gases at high temperatures is of a somewhat fragmentary nature. Science still requires to investigate the properties of gases at high temperatures, in order to fill the gap in our knowledge at the upper end of the scale, which Sir James Dewar has so ably filled at the lower end. Accordingly, for the present, it is not possible to formulate a complete theory of the internal combustion motor, in the absence of knowledge of the properties of the gases constituting its working fluid at the very high temperatures attained. The subject is a difficult one, and involves not only the statical properties of these gases, but it requires a knowledge of the conditions and rate of chemical combinations occurring in minute fractions of a second, and the conditions of dissociation of compounds such as carbonic acid and steam at high temperatures under varying temperatures and pressures. Our knowledge, for example, of the dissociation curves proper to steam or carbonic acid, is in the vague qualitative state. It is known that under certain circumstances some dissociation occurs; but no accurate quantitative knowledge exists as to the amount of dissociation under any given conditions of temperature and pressure, either alone or in mixture with other gases. Notwithstanding our present ignorance on this subject, many distinguished investigators have given it some attention. Bunsen attacked the problem in 1866. Fig. 5 is a drawing of the apparatus used by him. Here is arranged a small glass tube A which you see with a valve B, weights C to apply pressure to the valve, platinum points D D between which the electric spark could be passed the whole length of the tubular vessel. The vessel was filled with various explosive mixtures, and ignited by the spark. The valve was loaded until it just blew off. This blow-off pressure was considered to be the maximum pressure produced by the explosion. Bunsen made many experiments in this way. He considered his explosive mixture to be an air thermometer, with a varying volume, and after allowing for the variation due to chemical combination, he was able to calculate from his pressures the maximum mean temperature of the mixture. As a result of his experiments, Bunsen discovered that in none of the gaseous explosions investigated by him could he obtain the maximum pressures which he expected from the total heat of the mixture present, and the known specific heat of the products of combustion

at low temperatures. He generally found a deficit of about 50 per cent., that is, the pressures were only half those which he expected. He accounted for this by assuming that upon gaseous explosion the dissociation temperature of steam and carbonic acid was exceeded, and consequently chemical combination was arrested until temperature fell. Bunsen's apparatus was very crude, and could not have been expected to give accurate results. The maximum pressures must have far exceeded the pressures registered by his apparatus. To register pressures so instantaneous as those given by hydrogen and air or oxygen in such a small tube as Bunsen's, very light and delicate apparatus would be required; whereas he used long levers with heavy weights.

Messrs. Mallard and Le Chatelier, and Berthelot and Vieille, took

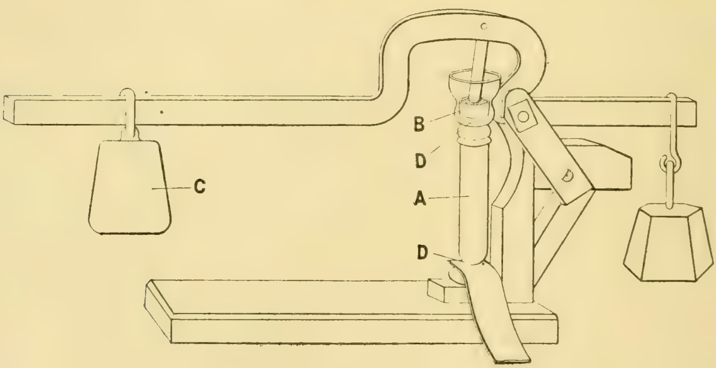

Fig. 5.

up the subject of gaseous explosions, and made experiments also with numerous gases and oxygen, and coal gas and air. They used an indicator instrument of a different type; but even with their instrument the records examined by me appear subject to very great oscillations of pressure, apparently depending upon the period of the indicators used.

A few years later a series of experiments were made by me, in 1883, with the apparatus which I show in section. Here I used a Richards indicator of the best construction known at that date, and secured indications which were fairly reliable. I show a slide from which you will see curves of explosion and cooling with coal gas obtained by means of this apparatus. These experiments also showed clearly that the whole of the heat present was not evolved at maximum temperature, assuming the gases to have their ordinary specific heat at the high temperatures as well as low. Messrs. Mallard and Le Chatelier, and Berthelot and Vieille, came to the conclusion that

the specific heat of the gases had been changed, and they considered combustion to be complete at the maximum temperature, or nearly so. My experience with engine indicator cards, supplementing the experiments made with gas and air mixtures in a closed vessel, led me to conclude that combustion was not complete, and that therefore it was not safe to draw deductions as to varying specific heat, in the absence of definite knowledge that chemical combination was completed before determinations were made of specific heat value. In engine experiments, it was found that the same phenomena occurred, that is, maximum pressure never reached that to be expected on the constant specific heat theory. My view, then, was in opposition to that of the French physicists, as I considered that no proof had been furnished by them, either of the absence of continued combustion, or the absence of dissociation at the very high temperatures of the flame. The absence of definite knowledge as to specific heats at high temperatures, dissociation, or rates of continued combustion, made it impossible to develop any complete theory of the internal combustion motor.

To enable some investigation, however, to be made on different engine cycles, it appeared desirable to consider the gas engine as an air engine pure and simple, operated with air of constant specific heat, the air being assumed as a perfect gas, and the chemical action considered as merely a means of heating the air through the desired temperature range. Calculating on this simplified theory, it became evident that the efficiency to be obtained in an air engine without heat losses was dependent upon compression mainly. Working out this theory showed that while the utmost thermal efficiency that could be theoretically expected from a non-compression engine of the Lenoir type was 22 per cent., compression supplied means of getting theoretical efficiencies of as high as 60 per cent., with practicable ranges of pressures and temperatures. Considering, then, gas and petrol engines as air engines, the theory is very simple. There are three symmetrical cycles of compression air engines. They may be called constant temperature, constant volume, and constant pressure engines, from the method of adding heat. In the constant temperature engines, the heat is added on an isothermal expanding line, but in the constant volume engines the heat is added while the volume is constant; and in the constant pressure engines the heat is added at constant pressure, while the volume varies. I have four diagrams on the wall illustrating these different engines, calculated as air engines. The first cycle you will recognise to be the Carnot cycle applied to an air engine. It is a perfect engine, between the limits of temperature. It will be seen, however, from the diagram, that assuming 500 lb. per square inch to be the maximum pressure allowable, although the efficiency is great, being equal indeed to 0·64, yet the mean pressure is very small—only 6 lb. per square inch for a maximum pressure of 500 lb. absolute. In the constant

volume engine shown on the other diagram with the same maximum pressure the efficiency is less, being 0·48; but the mean pressure is 105 lb. per square inch, with 500 maximum. The constant pressure engine shown in another figure shows still better results. Here the efficiency is 0·56, and the mean pressure is 117 lb. The Carnot cycle is obviously impossible from a practical point of view. It is interesting to note, however, that for equal compressions, it does not matter whether the Carnot cycle, constant volume, or constant pressure engines be used; the theoretical efficiency is the same. In the constant volume engine, which you will recognise as an engine operating on the modern compression system, high theoretical efficiencies are possible, and it is interesting to note that for any given compression ratio, the efficiency is the same, whatever be the maximum temperature above the temperature of compression—that is, the cycle is a cycle of constant efficiency, given certain conditions. It has been found in practice that a first-class modern engine will give in indicated power 0·7 of the heat which a perfect air engine would give under the same conditions of compression, proportions, etc. Thus, an engine having an air engine efficiency of 0·5, will give $0{\cdot}5 \times 0{\cdot}7 = 35$ per cent. of all the heat given to it in the form of indicated work.

The air standard has proved its utility as a guide to the engineer for twenty-five years now, and in a recent report of a committee appointed by the Institution of Civil Engineers on "The Standards of Efficiency in Internal Combustion Engines," it has been definitely adopted as the official standard, after exhaustive tests of engines of different sizes. This simplified theory of the internal combustion motor has been most useful in pointing out the way to better efficiencies, and, with its use, internal combustion motor efficiencies have risen from 16 per cent. in 1882 to a maximum of 37 per cent. in 1906. To enable further progress to be made, however, it is now necessary to know more of the actual properties of the working fluid. Unfortunately no method of investigation so far applied was able to determine those properties. During the past two years, however, considerable progress has been made in the invention and development of new means of studying the actual working fluid within the gas engine cylinder, and the gases composing it outside the cylinder in separate vessels. In the early experiments made by me, showing the rising and falling curves for gaseous mixtures, and in subsequent experiments made by Oliver in America, and by Messrs. Bairstow and Alexander in this country, the knowledge acquired of the rising and falling curves was only in strictness applicable to the behaviour of highly heated gases in a closed vessel. No means of obtaining a cooling curve in an engine cylinder had been proposed.

At the beginning of 1905 I proposed a new method, and made a considerable number of experiments on a 50 horse-power gas engine. A section of this engine is shown on the screen. Such engines give

indicator diagrams resembling the constant volume diagram shown upon the wall. I show an actual diagram from an engine like this upon the screen. These diagrams give information as to the time of ignition, the work done, and the compression and expansion lines followed by the charge within the cylinder. Such diagrams, however, do not give any information as to the rate of heat loss through the sides of the cylinder, or the specific heat of the high temperature charge undergoing expansion. By modifying the action of the engine, however, it is possible to get information of this nature. By altering the valve arrangements, so that when desired both charge inlet valve and exhaust valve can be held closed, I was able to get diagrams from which a cooling curve could be calculated. One of these diagrams is shown at Fig. 6. It will be seen that when

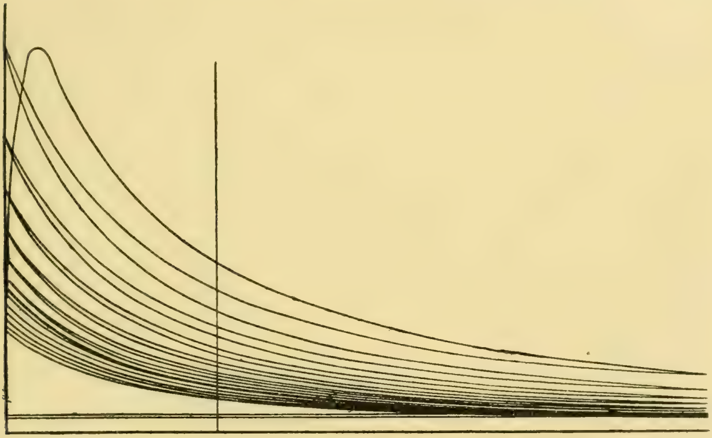

FIG. 6.

the exhaust period approaches, instead of exhaust discharge at the proper point, no gases escape from the cylinder. The piston accordingly compresses the whole contents of the cylinder into the compression space, and the temperature which has fallen by expansion, rises by compression. A point is touched on a vertical line from the end of the card. On expanding, a line below the first compression line is traced, then another compression line is obtained, and so on, a series of compression and expansion lines are obtained, each terminating under compression at certain specific points. It will be observed that before the ordinary compression line of the engine is reached in the diagram shown, there are six of those points. If no cooling existed in the cylinder, obviously whenever the volume was restored to the original point, no fall of temperature would be visible.

The fall, as you see, is gradually decreasing revolution by revolution. This fall, however, is not entirely due to heat loss. It is partly due to work done. There is a certain amount of heat converted into work at each reciprocation. This, however, can be allowed for, and then a cooling curve is obtained which shows the real temperature drop due to cooling up on the expanding and compressing lines. From this curve, by somewhat troublesome calculations into which I need not enter here, the apparent specific heat of the charge can be obtained for each expansion line.

I show upon the screen a curve giving the apparent specific heat of the particular working fluid in this engine at temperatures of from 70° C. up to 1500° C. Seven points are marked. These are the points of observations, and each point is the mean taken from twenty-one separate diagrams.

This method of obtaining specific heat appeared to me likely to give the true specific heat of the charge, without fear of continued combustion. In these particular experiments the engine was run at 120 revolutions per minute, so that the completion of the first compression after ignition occurred half a second after the explosion. In that half second it seemed to me probable that the greater part, if not all, of the combustion would be completed. Calculations made, however, from the expansion curves shown, proved that for some reason the lower end of each expansion line was disturbed in such a way as to make it highly probable that some combustion was continuing within the cylinder, even two seconds or so after the beginning of the explosion. I have, therefore, used the term apparent specific heat to characterise the values given by the curve I have shown.

Two tables have been calculated, giving the apparent instantaneous specific heats and the apparent mean specific heats for the different temperatures, as obtained in this manner. Assuming a certain proportion of continued combustion, these numbers give a very fair indication of the heat loss incurred in the cylinder, and curves have been prepared illustrating the heat loss incurred during the whole stroke, and a portion of the stroke, of this engine. These curves give an interesting indication of the probable mean temperatures of the cylinder walls under certain conditions. The curves show that for the whole stroke the mean temperature of the whole enclosing walls is about 70° C., when the water-jacket is cold, and about 200° C. when the water-jacket is hot; but for the inner part of the stroke, the first three-tenths of the stroke, the mean temperature is much higher—170° C. when cold, and 400° C. when hot.

This method of investigation gives a more accurate knowledge of the properties of the working fluid, so far as the thermodynamics of the engine are concerned, and it enables us to make an entire heat balance sheet from the diagram only. I have calculated full load diagrams taken from the engine by this method, and accounted for

106 thermal units, when the calorimeter showed 105 thermal units to be present. The method appears capable of very considerable accuracy.

This method, then, enables us at present to determine the apparent specific heat, and the heat flow to the cylinder in any engine under its working conditions. The indicator, however, requires to be of a very accurate type ; and to distinguish between a small amount of continued combustion, and a variable specific heat, it was necessary to design another type of indicator which did not depend upon mechanical levers for its marking upon the card. I accordingly constructed an optical indicator of a new type, which gives extremely accurate results.

Before leaving this new method of research, I would like to say a few words as to some valuable work which has recently been done by Professor Hopkinson, of Cambridge. Professor Hopkinson has attacked the problem of heat loss incurred within a closed vessel. The apparatus consists of a calorimeter capable of dividing up the heat flow to the sides of a closed vessel into portions incurred in minute fractions of a second. In this vessel there is a thin wood backing, and a copper strip is wound close against the backing, in such a way as to insulate the edges. If an electric current be passed through this strip, the temperature of the strip at any moment may be determined by the resistance. Assume a galvanometer to be in circuit, so arranged that its readings correspond to temperature. If this galvanometer be in circuit, and a gaseous explosion occurs within the cylinder, one diagram can be taken to show the effects of the explosion so far as pressure is concerned ; a simultaneous diagram can be taken showing the temperature of the copper strip. Determinations can be made of the heat loss to the backing, and other corrections, and in this manner the exact amount of heat which has left the hot gases, and passed into the walls can be determined at any given moment. This arrangement promises to give important information as to the rate of loss in gaseous explosions, from which observations some deductions may be drawn as to specific heat, and as to time of termination of combustion.

Returning to my own experiments with the optical indicator, my next slide shows an indicator card taken with the instrument, and operating to produce the new alternate compression and expansion diagram. The appearance of this indicator card is most interesting. You will observe slight discontinuity in the rising line, and just as maximum pressure is approached, the indicator begins to oscillate rapidly through a small distance. These oscillations, as you see, continue all down the expansion stroke, and die out gradually and do not terminate until the end of the compression stroke. The oscillations are about 600 to the second. The amplitude of the oscillations, as you see, gradually falls, until it has practically ceased

at the end of the first compression. The engine was running while this diagram was taken at the rate of 180 revolutions per minute—that is, each single stroke was performed in one-sixth of a second. From this it is evident that the oscillation in this particular engine lasted during a third of a second. The ordinary indicator very rarely shows oscillations of this kind. The mechanism is too rough, and is too much damped by the friction of the pencil to follow such rapid changes. I have tested the period of the indicator, and find that it is about 200 to the second, so far as ordinary piston displacement is concerned. From this it follows that considerable pressure disturbances within the cylinder must have occasioned the oscillation. In this particular engine, the explosion was always accompanied by a peculiar whistling sound. This is very common in many types of engines. This whistling sound seems to start just about the time the diagrams show the beginning of the oscillations—that is, immediately after ignition. It is somewhat difficult to account for this peculiar action, but it appears to have some connection with the discontinuous nature of combustion of a mixture of inflammable gas, or vapour with air. An experiment in this open glass tube will show clearly what I mean. Here I have a mixture of gas and air which is slowly being filled into the tube by means of a Bunsen burner at its bottom end. A flame is placed at the upper end. After a time you will notice that the mixture ignites at the open end; the flame travels back to the glass tube, accompanied at first by a low roaring sound, which increases in intensity as the end of the tube is reached, terminating in a loud snap, sometimes a sort of whistle. When this occurs, the flame flashes back along the tube again, and there is obvious oscillation of some kind proceeding. It is not known why the mixture burns in this way, but this particular roaring or whistling seems to occur only when combustion is going on. It is noticed in all pressure flames in the open air. Such flames, for instance, as gas and air mixture flames, make a loud roaring sound, and sometimes a loud whistling sound. In experimenting with flames for the igniter I described some time ago, and for a gas furnace using such mixture, I found loud whistling noises very often amounting to a shriek. It appears highly probable then, that wherever this oscillation goes on, as shown by the diagram, combustion is still proceeding. The combustion may be small in amount, but the oscillation appears to be an indication that combustion is continuing.

After taking a diagram with my optical indicator, I mentioned the matter to Professor Hopkinson, and he stated that he had the same experience in a large Otto engine. He was good enough to send me a diagram, taken with his optical indicator, on this large engine. Here you see the diagram, and in it the whole expansion line is obviously occupied by rapid oscillations of the indicator. Many diagrams have been taken, one on the top of the other, so

that the indications are not quite clear, but the oscillation is undoubted. This oscillation is very different from the usual indicator oscillations which vitiated the earlier experiments.

Professor Hopkinson has also made another interesting experiment bearing on the same point. He has ignited a mixture of gas and air at the top end of a long vertical tube, 6 feet long, by about 7 inches diameter. The indicator was placed at the bottom end, and the diagram was produced which I show you. This diagram shows a smooth, rising line until maximum pressure is approached. Then the indicator gets into violent movement, and it will be observed that on the cooling line the oscillation lasts, with gradually diminishing intensity, for quite a long period—not far short of a second. Combustion is evidently proceeding during this particular oscillation, because just at the terminating point of the oscillation it will be observed that the cooling curve has a break in it, a sort of hump. At that hump, as I understand it, the ordinary laws of cooling begin to have effect ; but up to that point combustion appears to be proceeding in the long tube.

At present, I have a number of experiments in hand on engines of different sizes, with the new optical indicator, and hope to get accurate figures, both as to specific heat, and continued combustion and heat flow through the cylinder sides.

Experiments have been made by Messrs. Holborn and Austen on the specific heat of air and carbonic acid by another method entirely, and there is reason to hope now, that, between the various experiments which, to my knowledge, are now progressing in this country and on the Continent, the whole question will be cleared up in the next few years in a satisfactory manner.

As engineers, we are vividly interested in the progress of knowledge as to the working fluid, because, without that knowledge, it is not possible to attain the ultimate practicable heat conversion by means of the internal combustion motor. At present, however, the internal combustion motor is by far the most efficient means of converting heat into mechanical work. 37 per cent. conversion has been achieved, and so far as present indications point, it will be possible to make an engine converting over 50 per cent. of the whole heat given to it into mechanical work. This has been made possible only by the study of the properties of flame, not only on the physical, but on the chemical side, and on the mechanical side, using devices of sufficient delicacy and rapidity to deal accurately with large volumes of a very difficult working fluid. The mean temperatures attained by the working fluid in many engines is as high as 1800° C. It has been found that attempts to measure the temperatures in the cylinders by platinum thermometers always result in the melting up of the platinum wire.

Flame, as I have already said, has required over one hundred years' effort by engineers and scientific men to enable it to be handled

as it is to-day. The results so far, are most satisfactory, as is shown by the many thousands of engines continually in operation, both fixed and locomotive. Much, however, remains to be done, and the immediate problem to be faced is the application of such engines to marine work. Although my sympathies are thoroughly with the abstract scientist, and I appreciate the abstract scientific interest in such problems as I have discussed to-night, yet the practical side absorbs most of the time of the engine designer and inventor dealing with flame as the working fluid; so that there is plenty of room for investigators, both physicists and chemists, in this promising field.

As one who has given thirty years' study to the practical and scientific problems involved in this matter, it is exceedingly gratifying to find a great and increasing interest in the subject, which will lead to the complete investigation of the complex properties of the working fluid.

[D. C.]

WEEKLY EVENING MEETING,

Friday, March 1, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Count A. de Bosdari, Councillor, Italian Embassy.

Dante in the Critical and Poetical Works of Carducci.

When I first accepted the invitation of the Managers of this old and illustrious Institution to hold this lecture (a great honour bestowed upon me for which I cannot adequately express my gratification), I could not foresee that what was intended to be an eulogy of the greatest of our living poets would turn into a commemoration of one who had just departed to join his great forerunners in the pantheon of the purest Italian glory. It is not without great emotion that I think that when I revisit my country I shall not see again *la cara e buona imagine paterna* of him whom I had adored as a genius and loved as a father.

In the year 1865 the then very young Kingdom of Italy celebrated one of its first National Festivities—the Fifth Centenary of the Birth of Dante. Among the crowd which lingered along the streets of Florence there was a young poet, who at the time could have said of himself—

"il nome mio ancor molto non suona."

Giosue Carducci, for he it was, had stopped awhile before the statue of St. George in the Loggia dell' Orgagna, and in a sort of soliloquy was urging the saint to descend from his pedestal and disperse with the broad sword he holds in his hands the idle multitude, and was calling him *fratello.* A purple-faced *romagnolo* heard the word, and taking it as meant for himself, kissed Carducci on both cheeks, and exclaimed, "Viva l'Italia, il Poeta divino e il Veltro ghibellino." "The poor man," says Carducci, relating this amusing episode, "did not intend to make poetry; but the poetry of the period was all more or less of that kind." Further on Carducci describes how good Professor Giuliani used to finish his weekly lessons on Dante by way of an allegorical meeting between Dante and Victor Emanuel "*in the region of the sparrows*," which never failed to excite the loudest applause of the fashionable ladies who used to gather around him. Critics of so many centuries had tried in vain to find out who

the real Beatrice was. How had they not perceived at once that Beatrice was the Italia Una? Did they not remember that in the purgatory she appears garbed with the three national colours? Many years after the preface of the "Levia Gravia" from which I have taken the few fragments above, had been written, the official world of Italy desired to use our great national poet as a weapon against the invading clericalism, and they instituted in Rome a chair, the main object of which was to expound whatever the most Christian and most Catholic of all poets had written against the Roman Church. They wanted to have for this chair a celebrated man, and the very first they thought of was Carducci, then at the summit of his fame; but Carducci refused this rather doubtful honour, and expressed the reasons of his doing so in a celebrated letter to Adriano Lemmi, from which I am going to quote to you a passage, because I think that it is a sort of compendium of the principles of historical sincerity, and, so to say, of political self-denial, with which our Master has taught us to approach Dante: "The aims and intentions of those who have instituted the chair seem to be such as to require in the man who will occupy it opinions on the political and religious ideas of Dante, which I must frankly acknowledge I do not possess. For me the greatness of Dante does not go beyond the circle of middle-age and of strict catholicism; the reform which, according to Ugo Foscolo, he had the intention to effect in the Church, if this intention existed at all, did not affect the dogmas; he aimed at a catholicism more strict, more ascetic, and more despotic. No one more than the Alighieri dreamed or would have more approved of a conciliation between the Pope and the Empire. But the conciliation is an old Italian utopia, and we need not be afraid of it. However, let us avoid politics. I only mean that I may be mistaken in these ideas of mine, and would be willing to be persuaded to the contrary, but I know that they are expressed in some books which are now in the hands of many, and it would be unbecoming for me to go on the new Chair and contradict them." It was so that, avoiding all the prejudices of his time and only trusting long and laborious historical researches, he set very early to work to build for himself and for his pupils of that school of Bologna, which he never wished to leave, an image of Dante, which might be truthful; and his very first cares were given to the rhymes of the poet, that marvellous early work so unknown to many, and still so necessary to prepare one's mind to the study of the Divine Comedy. In the essay on the rhymes of Dante he first of all points out the enormous debt of Italian literature, acknowledged by Dante, to the French and Provençal predecessors. He then goes on describing how the rhymes can very appropriately be divided into three periods, the first of which may be termed a period of transformation. It is known that Dante despised his contemporary Tuscan poets, and that he much more praised those of the school of Bologna. But though he was shortly to become infinitely

superior to all, he at the beginning was but one of them, and he had to start his poetical career by some of those competitions which were the fashion of the day. Some of his early productions are unpoetical, and some of them unmistakably ugly, and on this ground have been repudiated by many critics; but the tendency of the critic of Carducci is to accept as authentic as many as possible, as he very rightly thinks that no great man becomes such in a moment. Moreover, much more conversant than the greatest part of his predecessors with the mysteries and intricacies of the ancient Italian poetical technicalities, he is able to find arguments for the authenticity of such poems, which had entirely escaped the notice of men like Fraticelli and Giuliani. Dante did not belong more than a few years to this period of transformation. He was trying to find a new way, and it was not long before he discovered it. All of you remember the passage of the purgatory, where he meets Bonagiunta de Lucca, a poet of the period. Bonagiunta asks Dante how is it that the sweet new style he now hears is so different from what he and his companions had been able to create. You know what Dante answers: "I am one who writes when love inspires me, and I set down what it dictates in my innermost soul." Let me read to you those celebrated lines which give us a key and an explanation of the second period of the rhymes of Dante, which according to our critic is to be styled Mystic:—

(Dante. Purgatorio, xxiv., 49–63.)

Mystic, because our poet soon resorted to religion as a refuge from the torment and unrest of earthly love. "In the souls," says Carducci, "in which wrath is more powerful, love is usually deeper and more thoughtful, and even sensuality seems to have something ideal about it. Those lyons appear to feel as a need of rest, and, leaning their heads on love, to dream, and to find in the dream of the beloved eyes a sort of refuge from the desert and the hell which is round them in the world." With such a soul this man who, as Byron said, "*loved before knowing the name of love*," who fed his love with its own strength without any exterior satisfaction, must have found a sort of exaltation in what was denied to him. Add to this the presentiment of the near end of Beatrice, on which he so very often dwells, as her weakness and the paleness of her cheeks seemed to afford a sufficient reason for his fears. You would have said that he was perceiving the wings of an angel appearing on the handsome shoulders of the Florentine, and that he watched her as she slowly abandoned the earth to ascend to the heavens, so that Dante never ceased since to look heavenwards. The Beatrice of the rhymes of this period is not yet the symbol of divine wisdom, but already a creature sent by God into the world to show it a miracle—"di cielo in terra a miracol mostrare"—and come for its salvation—"e venne in terra per nostra salute." I have here quoted the very

words of Carducci himself, because I think that only a poet can seize certain shades which, besides historical reasons, can show us how the poetry of love of mediæval troubadours became through religion worthy of the loftiest minds.

The third period of the rhymes of Dante symbolic and philosophical, according to a very clever conjecture of our critic dates back to the time after the death of Beatrice, when the poet met a mysterious lady, as every one of you will remember to have read in the "Vita Nuova." Then began a series of passing loves which the poet expiates in his purgatory, before ascending to his paradise. Because our critic quite rightly thinks that the symbol and the allegory is not all in this last part of the lyric works of the Alighieri. It had been a dogma of his, repeatedly affirmed in the "Vulgare Eloquio," that vulgar rhymes could only serve the purpose of expressing the thoughts of love. Depicting human wisdom under the shadow of an allegorical woman could not, in the long run, be sufficient to a man who felt something more in love, and to whom, according to his first biographer, even sensuality was not altogether without attraction. *Ricordati, ricordati*, as Virgil tells him in the purgatory; and our critic (remember he was writing this essay in the prime of his youth) cannot but be indulgent for the warmth of the amorous passions which appear through "*il velame delli versi strani*." But the results of those sensual sins, which the great human poet was not successful in concealing under a philosophical allegory, were the tremendous reproach addressed to him by Beatrice at the end of the purgatory. As Carducci quotes it at length in his essay, I hope you will allow me to read it as a conclusion of this hurried summary of it.

(Dante, Purgatorio xxx., 121–137; xxxi., 28–30 and 49–60.)

The two discourses on "la varia fortuna di Dante" are an attempt, somewhat incomplete and unsuccessful, to write the story of the fame attained by Dante through the various ages of the Italian literature. The critical portion of this writing is quite imperfect, and, accordingly, was omitted from the edition of the selected prose works of Carducci, revised and approved by the author himself. But, out of the whole, I have still to mention two chapters, which have been inserted therein, and are really worth studying.

The first is an account of the opinions held on Dante by his contemporaries till the end of the 15th century, such as Giovanni da Virgilio the Latin poet of Bologna, Cecco d'Ascoli Dante's fierce detractor, Cardinal del Poggetto, who wanted the Inquisition to burn the book of the "Monarchia," and finally the poet's own sons Pietro and Jacopo, the first editors and explainers of the Divine Comedy.

In the other chapter Carducci brings together the three giant figures of the Italian middle age, Dante, Petrarca, and Boccaccio, and from their being near each other a new light arises to make clearer their images to us. The touching modesty and unreserved admiration of Boccaccio

for Dante is known to us through all his works, but what is less known is that Boccaccio became as a sort of intermediary between the two, whom he considered so much greater than himself, but whom posterity has declared to be his equal. Both Ugo Foscolo and Cesare Cantù have contended that Petrarca was jealous of Dante. You will find in the essay I am alluding to a luminous translation and a penetrating criticism of the celebrated "Lettera Senile," where Petrarca explains how absurd and inconsistent that rumour had been, and, besides, you will be able, through many appropriate quotations, to persuade yourself that the later poet paid to the earlier one of the greatest possible compliments by imitating him.

I would be tempted to give a fuller account of the celebrated discourse, "L'Opera di Dante," which was the only one ever held on the chair of which I have spoken before; but who could ever sum up a writing so bristling with thoughts without reading it from beginning to end? Let me just remind you of the preamble in which he describes how the remembrances of the four greatest poetical names of Italy surround the ruins of Canossa, where, he says, the dissents between Church and Empire assumed the appearance of a fatal drama, and of a passage where, though more moderately, are repeated and confirmed the ideas contained in the letter to Adriano Lemmi on the futility to try to constrain Dante into the mould of modern thought:—

"It is not the case to attempt to find in the Imperial principles of the Alighieri the foundation of the unity of Italy, unless this is included in the wider unity of Christianity. The love for the national idea is shown in the deep feeling of the poet for the glories and misfortunes of Italy, and in the fact that he considered the empire as a Roman institution and as an Italian right. The book of the 'Monarchia' is the last expression of mediæval political classicism, and it would be injurious to Dante, according to his ideas, to try to discover therein what is called to-day the pagan State or the atheistic State. But we can derive enough praise by remembering that Dante is our master and father in keeping the Roman tradition of Italy, and that he was the purest and most severe judge of the misgovernment of Church people and of the necessity of getting rid of it." After perusing the special works which Carducci consecrated to the study, *del Vicin suo grande*, as he calls him, it will be of very great interest to you to follow his steps in studying the influence and the efficiency of the poet through more than three centuries of the history of Italian literature. I could not too warmly advise you to very carefully read the four discourses, entitled "Dello svolgimento della letteratura nazionale." Carducci was always averse, and taught us to avoid in the study of literature the general ideas, and to apply to the great masters of the poetical art the systems and the proceedings so usefully employed in our days in natural history. But he sometimes indulged himself in what he disapproved of in young

people, and we must admit that no one better than he was entitled to make an exception in his own favour. These discourses are the finest specimens of this sort of achievement. You will see there how the Italian language, as soon as it was fit for literature, arose through Dante to a greatness unequalled since. The birth of Dante and the death of Ariosto are called there the sunshine and the sunset of the glorious day of Italian revival; and Dante is described as looking on the Italian culture like a giant arising between St. Thomas and St. Bonaventura, at the time when all the beautiful cathedrals of Italy were being erected (never to be completed afterwards), and when the timid colours of art were still waiting for a great painter. Further on we shall be taught why such a great difference exists between the poetry of Dante and his successors; and how the Divine Comedy remained as a solitary monument in which no one dares to inhabit, or build other edifices on it. According to our Master, if Dante had lived a little earlier, perhaps a century earlier, he would have created an ideal religious literature more civil than the one which in later days belonged to Catholic Spain, and more practical than the one Germany has seen in our days. Coming down to the Renaissance, Carducci points out that the Popes of this time were unworthy of the invective of St. Peter in the Paradise; and, reminding us that the great poem had been begun in Latin verses (a hard task which was abandoned at once), he wonders how, a few centuries later, a movement could be started again towards the classical antiquity which Dante could have been thought to have stopped for ever. This study of the great models of antiquity was instrumental to transform into a national literature what was in Dante strictly Tuscan. And what could be the efficiency of the religious principles in Italy, if the efficiency of the Divine Comedy had been nil? Carducci gives us a formula of the Italian thoughts which were developed for centuries around the Roman Catholic religion, and which are quite identical in writers so deeply different from Dante.

As for the Italian language, Dante had already noticed and described the tendencies that came into effect only much later, by distinguishing the noble language from the vulgar; but only in the 16th century the rules of the language which Dante had created could be established, and with the realisation of all his theories the edifice of the national literature was completed. It was then that Italy appeared for the last time in its character of the intellectual capital of Europe; and as she had given to middle age the foremost Christian poet, she gave at the end of the Renaissance its only real heir in Torquato Tasso.

He, too, thinks and believes through his philosophy; he loves and explains his love through his doctrine. He is an artist, and writes dialogues of scholastic speculations, which aspire at being platonic. And, as Dante himself, he has always something to

repent of in his Catholic conscience. His poem, so essentially religious and chivalrous, is subdued by him to a moral allegory; but, even so, seems to him to be profane. He begins it again on purer lines, but this new poem is still far from satisfying him, and he finishes by writing the *poem of the creation*. I could conclude my sketch of the critical work of Carducci on Dante by this rather fragmentary compendium of his four discourses, but I have still to tell you that valuable ideas and precious teachings on Dante are scattered all over the immense literary work of our critic, as when he compares Goffredo Mameli to Dante in those poems where that hero of our *risorgimento* imitates too closely some of the Canzoni of the Convito in the excess of the *doctrinal* poetry. Or when he compares Manzoni to Chiabrera, who had been successful in substituting a popular form of songs for the individual, and rather too courtly canzoni of Dante. Or, finally, as when speaking on the unveiling of a memorial of his beloved Giacomo Leopardi, the last great character of slave Italy, he proudly exclaims: "If a foreigner will malignantly come forward to reproach us that we have served too long a time, we will always be able to answer with a single word, 'Dante.'"

I will not speak at great length of the inspirations from Dante, in the poetical work of our author. First of all, because my official duties call me elsewhere, and secondly, because in such a matter it is much better to allow the poet to speak of himself. Moreover, the direct inspirations are not very numerous, and, generally speaking, the poetical style of Carducci much more shows the influence of Horace or Petrarca than that of Dante. You would say that he was loth to try to imitate what is inimitable. But as early as 1855, being little more than 20 years old, he had already given to the world his hymn to Dante, following in this the example of Giuseppe Giusti, Giacomo Leopardi, and of nearly every great poet of the 19th century. In this hymn, rather too grandiloquous, and replete with youthful superabundance, we read the following lines:—

Divin surse il Poeta; e disdegnando,
La triste Italia, e per mancar d'obietto,
Pargoleggiante il gran vigor natio
Te salutò in desio—Alma Italia novella,
Una d'armi di leggi e di favella,

to which he later on appended the following note: "It was allright to say that in 1859, but now, having studied a little better the times, the men, and the poem, I would not say that, not even in a dithyramb. These are news which we must leave to those who make all efforts to flatter the *Veltro*." This hymn is still to be read in those Juvenilia, on which the poet himself passed such a severe censure. From the festivities of the fifth centenary of Dante Carducci brought back three very powerful sonnets, in which Dante is again introduced to prophesy the liberation of Rome. I am sorry not to have time to

read them, but I must hasten to the more perfect work of the "Rime Nuove," where finds its place the celebrated sonnet, in which the poet asks Dante why, when everything he cherished, Church and Empire, has disappeared from the world, he (Carducci) still spends days and nights on these poems, though he has no hope that Beatrice will pray for him, nor that Matilda will wash out his sins; and the other sonnet, "Giustizia di Poeta," where Dante is represented as punishing with hell all his enemies, and calmly looking down on them from heaven.

(Carducci, Poesie, pp. 558, 559.)

I will rapidly point out some satiric passages which seem to be a discordant note in his great veneration for Dante, as where in "Le Nozze" he alludes to the sterile Beatrice, who must give up to "l'immenso intendimento della vita umana." Or where extolling the pagan and serene happiness of the life of Ariosto, he says that the price of his songs was not the favour of the princes, nor an ever-changing popularity, nor "di teologal donna l'amore." In fact the irony and the opposition between what he actually praises and exalts, and what he, for the moment, has ceased to adore, or has never adored at all, is one of the characteristics of Carducci's poetry, and, according to the opinion of a very acute critic Enrico Panzacchi, sometimes spoils his very best inspirations. I could quote to you a good many more fragments in his work, but I think that you will more appreciate the reading of two "Ode Barbare," which are in some measure inspired by Dante. The first is entitled "In una chiesa gotica." You remember that I have shown to you how Dante introduced the religious element in the poetry of human love:—

In the canzoni of that period (he says, and I cannot do better than quoting his prose as a comment to his poetry) *there are some stanzas which can only have been conceived between the austere columns of the great cathedrals, under the light of a glorious sunset in April reflected on the painted church windows, and contrasted by the reddish light of the chandlers, whereas the smell of the incense wraps up the altar of the Virgin, the organ sounds, and the argentine voices of the women fill the dark vaults with a melancholic hymn. Then Dante must have seen in an odoriferous cloud, brightened on the white forehead by the uncertain light of the setting sun and of the wax tapers, the girl of the Portinaris; there he must have heard the voice of her, knelt down, go up to God in a sound of lamentation and desire; then the time and the space must have faded away before his mind, and he must have seen the vision of paradise and inferno: the paradise which called her, and the inferno which waited for him.*

The second ode barbara, which I am going to read to you, and which will conclude this lecture, is mainly consecrated to the praise of Sirmione. This gem of the peninsulas, full of the memories of Catullus, who used to seat himself on its shores,

lamenting his lost love of Lesbya, with his soul deeply wounded by the Roman orgies, is not far from Verona. Lalage, the companion of the poet, is made to turn from the enchanting sight of the lake to the frowning tower of the Scaligeri, and to watch, on her knees, the Alighieri who appears there, to utter the famous words :—

Suso in Italia bella giace un lago,
Appiè dell' Alpe che serra Lamagna
Sopra Tiralli che ha nome Benaco.

(Carducci, Poesie, pp. 813–816 and 833–836.)

[A. de B.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Mrs. Ayrton, M.Inst.E.E.
Sidney George Brown, Esq.
William Duddell, Esq., M.Inst.E.E.
Horace Duncan, Esq., M.B.
William George Kirkaldy, Esq.
John Edward Mounsey, Esq.
Walter Peacock, Esq.
Edwin H. Rayner, Esq.
Edward Swash, Esq.
Hugh Weguelin, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to "A Member" for a Donation of £60, and to Mr. E. R. Merton, *M.R.I.*, for his Donation of £50, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Chairman reported the decease of Professor Henri Moissan, *Hon.Mem.R.I.*, on February 20, and of Mr. Lachlan Mackintosh Rate, M.A. *M.R.I.*, on February 28, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the irreparable loss to Science and to the Institution in the decease of their Honorary Member, Professor Henri Moissan, D.C.L. D.Sc. Hon.F.R.S. Hon.F.C.S. Membre de l'Institut, Commandeur de la Légion d'Honneur, Professeur de Chimie Minérale à l'Université de Paris.

Professor Moissan was elected an Honorary Member of the Royal Institution on the occasion of the Centenary celebrations in 1899, and was presented with his Diploma of Honorary Membership by H.R.H. The Prince of Wales, the Vice-Patron.

Professor Moissan delivered a Discourse at the Royal Institution on "Le Fluor," on Friday Evening, May 28, 1897. In a series of conjoint experiments on the following day with Professor Sir James Dewar, they succeeded in liquefying Fluorine. And in the year 1903, gaseous Fluorine was actually sent by Professor Moissan from Paris to the Royal Institution, where it was solidified by the use of liquid hydrogen, and proved even at this low temperature that Fluorine combined with liquid hydrogen with explosive violence.

Professor Moissan's last Friday Evening Discourse at the Royal Institution was given so recently as June 1, 1906 on "L'Ebullition des Corps Simples," when he illustrated, by use of his electric furnace, the volatilisation of the metals.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of their most sincere sympathy and heartfelt condolence with Madame Moissan and the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained in the decease of their late Honorary Secretary to the Committee of Visitors, Lachlan Mackintosh Rate, Esq., M.A.

Mr. Rate was elected a Member of the Royal Institution in 1859. He always took an active interest in the welfare of the Institution, and as a benefactor contributed to its scientific work. His tact and business qualifications enabled him to render invaluable services to the cause of Science, and the advancement of the Institution, and for twenty-one years he discharged the laborious duties of Honorary Secretary to the Committee of Visitors.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of their most sincere sympathy and heartfelt condolence with the family in their bereavement.

The following letters were read :—

1 STRATTON STREET, W.
February 9th, 1907.

DEAR SIR,

I have the honour to acknowledge the receipt of your letter of the 8th inst. enclosing a Resolution of the Managers of the Royal Institution of Great Britain with reference to the death of the Baroness Burdett-Coutts.

I beg you will assure the Managers of my appreciation of the terms of the Resolution, and my gratitude for the expression of their personal sympathy in the great loss I have sustained.

I hope I may be permitted to add a few remarks pertinent to the special bearing of the Resolution, and possibly not uninteresting to the Managers and Members of the Royal Institution. I am perhaps the more entitled to such a privilege in dealing with this important tribute to the Baroness's memory, not so much because I am left by her express desire and act in the position of her sole representative, as by reason of my long and close association with a public life which in its inner aspects was otherwise a singularly solitary one.

The Baroness's social relationships to Science, her friendships with many, her acquaintance with nearly all, of its eminent professors for the past 70 years, are well known ; her efforts to encourage and aid those who were struggling in the same path, not so well : as was her custom. But it probably is not known—at least to this generation—that, without the equipment of a scientific training, she had the keenest personal interest in Science ; she delighted to hear its problems discussed, its discoveries explained ; and she never failed to follow, with an attention jealous of apathy or interruption, their learned exposition to those around her. I cannot remember the time when the "man of Science," even one little known at the moment or to the company, had not a special place at her table ; or the occasion when her own attention, compelling that of others, did not give him full opportunity.

The Baroness was a great educationist. At a time before the State assumed that obligation, she naturally devoted her efforts in that respect to the poorest classes. The later phase of the question still found her in the same field, specialising those efforts in directions which the State has only recently begun to follow.

But all the while she was mindful of the claims of science, less popular then than now ; and in many ways little known, or not remembered, she brought her ever-watchful eye, her discrimination, and her means, to bear on their advancement.

If a scholarship was to be established at Oxford, not classics or history, or even theology, but Science claimed her aid. The great Dr. Routh, notoriously difficult of access in his old age, received her at Magdalen as a young girl, bent on the adventure from a party at Nuneham, and became her friend till he died. Yet even that memory, rich in patristic learning, did not avail to place the Bodleian Library as high in her affections as the University Museum. I would venture the opinion that she believed the Pengelley collection of fossils, rightly studied, would have more practical bearing on future human happiness than a gift of old manuscripts.

Such things were not done at haphazard. She would spare no trouble to search out both the need and the means. With a touch of characteristic humour she inquired of Sir Wm. Hooker whether Kew Gardens, so far up the Thames, was not poor in Seaweeds. She had already found out the fact, and had secured the Griffiths collection, so rare and extensive that, without impairing the central completeness, it provided duplicates for six other Botanical establishments. She probably had not read Schimper's monograph on the genus Sphagnum, and did not know the details of the muscological collection of Bruch; but she found out that Kew also wanted Mosses, and that Schimper's great herbarium could be acquired. Thus, not so much by wealth as by thoughtful insight, special departments of British Science were enriched at her hands.

I could multiply such instances, if time served, in more local spheres; and extend them to branches of science with which the Royal Institution is more directly concerned. It is almost superfluous to say they would touch the careers of many students of Science, young and old, experimentalists, discoverers, halting in their progress for want of the help that came from an unknown hand.

It is not my purpose to examine the motives of this marked and consistent attitude of the Baroness towards Science; rather to suggest, as I believe, that it was in the first place natural, and without motive, satisfying the instincts of a mind peculiarly analytical, suggestive, and acute. But she was also deep-thinking and far-seeing; while she worked at the daily round, she was looking always to the horizon. She knew the constant and munificent aids that Science had brought, and must continue to bring, to the great human problem to which she devoted her life. And it was inherent in her nature, in days gone by, when Science appealed to a more limited and less generous circle than now, to select it, simply on that account, for such encouragement as her means and position, her personal sympathy and interest, could often afford.

I have felt it a grateful duty, to the Baroness's memory, and to the honour paid it by the Royal Institution, to trespass on the Managers' attention at this length, for which I hope the reasons apparent will be adequate excuse.

I am, dear Sir,
Yours faithfully,
(Signed) W. BURDETT-COUTTS.

SIR WILLIAM CROOKES, F.R.S.
Honorary Secretary, Royal Institution.

68 REDCLIFFE SQUARE, LONDON, S.W.
February 9, 1907.

DEAR SIR,

As the only surviving member of the family of the late Miss Agnes M. Clerke, I have to thank you for forwarding to me the Resolution passed at the General Meeting of the Royal Institution on the 4th inst.

I am deeply grateful for the honour that the Institution has thus done to the memory of my sister, and for the graceful appreciation of her services which the Resolution records.

I am, dear Sir, yours truly,
(Signed) AUBREY ST. JOHN CLERKE.

[*Translation.*]

PROFESSOR SIR JAMES DEWAR, F.R.S. ST. PETERSBURG.

SIR,

I am profoundly touched by your letter of condolence. Amongst all the letters of sympathy for my husband, those which I have received from England have touched me to the quick.

I will always remember the profound esteem of my husband for his English friends, and I cherish the best memories of my sojourn in England and of your family.

With kindest regards, yours very truly,
(Signed) ANNA MENDELEEFF.

The Chairman reported that the following Letters had been received from the Honorary Members who were elected at the General Meeting on December 3, 1906.

UPSALA, *December* 11, 1906.

SIR,

I beg you to convey to the President and the Members of the Royal Institution of Great Britain my most respectful and sincere thanks on account of my having been elected an Honorary Member of their illustrious Institution.

I am keenly and humbly sensible of the great honour hereby conferred on me, which I feel to be a most precious encouragement to continue, to the best of my powers, my endeavours in the service of science.

Yours very sincerely,
(Signed) H. H. HILDEBRANDSSON.

CAMBRIDGE, U.S.A.
December 13, 1906.

DEAR SIR,

Your favour of December 4, announcing my election to Honorary Membership in the Royal Institution, has just come.

Permit me to express my profound appreciation of this very great honour, and my gratitude to the Members for their kindness and interest in my work. It is needless to say that I accept the Honorary Membership with the greatest pleasure. To have one's name entered upon a roll containing those of so many men of highest eminence is a great privilege and encouragement.

I am, Sir, your obedient servant,
(Signed) THEODORE WM. RICHARDS.

KIEL, *December* 16, 1906.

SIR,

Allow me to transfer to you my sincere thanks, due to the Members of the Royal Institution of Great Britain, for the very great honour they have done me by electing me—unanimously, I am informed—an Honorary Member of the Institution.

Every philosopher knows so well the history of the Royal Institution—knows Faraday—that it is certainly needless to say how much I value this Membership.

Not being able to come over presently to England, I try to apply to your kindness in helping me forwarding any due expression of my thanks to all Members of the Institution.

I am, yours very truly,
(Signed) P. LENARD.

[*Translation.*]

MUNICH, *December* 16, 1906

DEAR SIR,

Having received your kind letters of the 4th and 11th December, as well as the printed matter and the Diploma as "Honorary Member of the Royal Institution of Great Britain," I have the honour to express to you my thanks for your trouble, and to beg you to tell the Committee how deeply honoured I feel by this exceptional and high distinction.

To be an Honorary Member of an institution whose task is the "Promotion, Diffusion and Extension of Science and of Useful Knowledge," and to which tbe most brilliant stars of our scientific firmament, such as Davy, Faraday, Lord Rayleigh, J. J. Thomson, Dewar, and many others, have belonged, is certainly the highest guerdon for a long life devoted to the teaching and pursuit of chemical knowledge.

I am, with especial respect, your devoted
ADOLF VON BÄYER.

UPSALA, *December* 17, 1906.

DEAR SIR,

Some days since I had the honour to receive your information of my election as Honorary Member of the Royal Institution, and yesterday I got the Diploma.

I feel sure that I shall always look back on this great and unexpected appreciation with especial pleasure. and I beg you to receive, and to the President and to the Members of the Royal Institution present, the expression of my hearty appreciation of the great honour conferred upon me.

Yours most faithfully,
(Signed) KNUT ANGSTROM.

To His Grace the Duke of Northumberland,
President of the Royal Institution.

Senate of the Kingdom (of Italy),
December 22, 1906.

Illustrious Sir,

I have now received the Diploma of Honorary Membership of the Royal Institution of Great Britain, and it is with the greatest expansion of the heart that I fulfil the pleasing duty of offering to you, Illustrious Sir, the expression of my keenest gratification. If this unexpected honour fills me on the one hand with joy, on the other hand it admonishes me to persevere in those experimental studies which, in a degree far greater than my scanty merits, have called forth this fresh evidence of good will on the part of English scientists.

Will your Grace be good enough to transmit my gratitude to all the Members of the celebrated Royal Institution, and at the same time to accept personally the expression of my highest consideration.

Your most devoted,
(Signed) AUGUSTO RIGHI.

Berlin, *December* 27, 1906.

Sir,

I was gladly surprised by your letter announcing me the extreme distinction of being elected an Honorary Member of the Royal Institution—the more, as this Society is closely allied to some of the discoverers I admire most, and who are reproduced on the Diploma I received this day.

With the expression of my most cordial thanks for both,

I am, yours truly,
(Signed) J. H. van't HOFF.

To His Grace the Duke of Northumberland,
President of the Royal Institution.

Davos, *January* 5, 1907.

Your Grace,

The Royal Institution, at their Meeting of 3rd December last, did me the honour of electing me as an Honorary Member. May I beg your Grace, as President of the Institution, to receive my sincerest and warmest thanks for this distinction, and to rest assured that I esteem most highly the importance of the fact that an institution which has reckoned among its Members such great masters as Young, Davy, Faraday, etc., and such, and to which many of the most eminent inquirers of modern times belong, should have accorded such recognition of my modest performances. This recognition will be to me a powerful incentive to further efforts.

I regret that illness has prevented me sending you a letter of thanks before.

Assuring your Grace of my especial respect,

I am your Grace's obedient servant,
(Signed) W. C. RÖNTGEN.

The following Lecture Arrangements were announced :—

Professor G. H. Bryan, M.A. Sc.D. F.R.S. Two Lectures on Wings and Aeroplanes. On *Tuesdays*, April 9, 16.

Professor W. Stirling, M.D. LL.D. D.Sc. Three Lectures on Stimulation, Luminous and Chemical. On *Tuesdays*, April 23, 30, May 7.

D. S. MacColl, Esq., LL.D. Two Lectures on Alfred Stevens (The English Sculptor and Painter). On *Tuesdays*, May 14, 21.

PROFESSOR G. H. F. NUTTALL, M.A. M.D. Ph.D. Sc.D. F.R.S. Two Lectures on MALARIA, SLEEPING SICKNESS, TICK FEVER, AND ALLIED DISEASES. On *Tuesdays*, May 28, June 4.

PROFESSOR H. A. MIERS, M.A. D.Sc. F.R.S. Two Lectures on THE BIRTH AND AFFINITIES OF CRYSTALS. On *Thursdays*, April 11, 18.

A. W. VERRALL, Esq., Litt.D. Two Lectures on (I.) EURIPIDES AND HIS AGE, (II.) THE BACCHANTS OF EURIPIDES. On *Thursdays*, April 25, May 2.

H. F. NEWALL, Esq., M.A. F.R.S. Pres.R.A.S. Two Lectures on SPECTROSCOPIC PHENOMENA IN STARS: (I.) CHEMISTRY, (II.) MOTION. On *Thursdays*, May 9, 16.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.* Three Lectures on CHEMICAL PROGRESS: WORK OF BERTHELOT, MENDELEEFF, AND MOISSAN. On *Thursdays*, May 23, 30, June 6.

PROFESSOR S. P. THOMPSON, B.A. D.Sc. F.R.S. *M.R.I.* Three Lectures on STUDIES IN MAGNETISM. On *Saturdays*, April 13, 20, 27.

PROFESSOR W. C. McINTOSH, M.D. LL.D. F.R.S. Two Lectures on SCIENTIFIC WORK IN THE SEA FISHERIES. On *Saturdays*, May 4, 11.

ARTHUR BOURCHIER, Esq., M.A. Two Lectures on THE LIMITS OF THE DRAMATIC ART. On *Saturdays*, May 18, 25.

SIR WILLIAM H. WHITE, K.C.B. LL.D. D.Sc. F.R.S. *M.R.I.* Two Lectures on THE CONTEST BETWEEN GUNS AND ARMOUR. On *Saturdays*, June 1, 8.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

British Museum (Natural History) Trustees—History of the Natural History Collections, Vol. II. 8vo. 1906.
British Bloodsucking Flies. 8vo. 1906.
Catalogue of Lepidoptera Phalænæ, Vol. VI. (and Plates). 8vo. 1906.
Catalogue of Madreporarian Corals, Vol. VI. 4to. 1906.
List of Casts of Fossils. 8vo. 1906.
Guide to Exhibition of Old Natural History Books. 8vo. 1905.

Secretary of State for India—Geological Survey: Records, Vol. XXXIV. Part 3. 8vo. 1906.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVI. 1° Semestre, Fasc. 2. 8vo. 1907.
Classe di Filologiche, Vol. XV. Fasc. 7–10. 1906.

American Geographical Society—Bulletin, Vol. XXXIX. No. 1. 8vo. 1907.

Asiatic Society of Bengal—Journal and Proceedings, Vol. II. 1906, Nos. 4–9. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXVII. No. 3. 8vo. 1907.

Automobile Club—Journal for Feb. 1907.

Bankers Institute—Journal, Vol. XXVIII. Part 3. 8vo. 1907.

Bashforth, F., Esq. (the Author)—Ballistic Experiments from 1864–1880. 8vo. 1906.

Batavia, Royal Magnetical and Meteorological Observatory—Regenwaarnemingen in Nederlandsch-Indie, 1905. 8vo. 1906.

Birmingham and Midland Institute—Report for 1906. 8vo. 1907.

Boston Public Library—Monthly Bulletin for Feb. 1907. 8vo.
Annual List of Books, 1905–6. 8vo. 1907.

British Architects, Royal Institute of—Journal, Third Series, Vol. XIV. Nos. 7–8. 4to. 1907.

British Astronomical Association—Journal, Vol. XVII. No. 4. 8vo. 1907.
Guide to History of Plant Classification. 8vo. 1906.

Brooklyn Institute—Science Bulletin, Vol. I. No. 4. 8vo. 1904.
Canada, Commissioner for Emigration—Canadian Life and Resources, Nov. 1906. 4to.
Canada, Geological Survey—Summary Reports, 1905–6. 8vo. 1906.
Reports on Rossland, B.C., and Chibougaman Mining Districts. 8vo. 1906.
Section of Mines Report, 1904. 8vo. 1906.
Cape Colony, Government of—Records of South-Eastern Africa. By F. M. Theal. 9 vol. 8vo. 1898–1903.
Carnegie Foundation for the Advancement of Teaching—First Annual Report of the President, 1906. 8vo.
Carnegie Institution—Contributions from the Solar Observatory, Mt. Wilson, Nos. 13–14. 8vo. 1907.
Report of the Director, 1907. 8vo.
Chemical Industry, Society of—Journal, Vol. XXVI. No. 3. 8vo. 1907.
Chemical Society—Proceedings, Vol. XXIII. Nos. 320–321. 8vo. 1907.
Journal for Feb. 1907. 8vo.
Church of England League—Gazette for Feb. 1907. 8vo.
Editors—American Journal of Science for Feb. 1907. 8vo.
Analyst for Feb. 1907. 8vo.
Astrophysical Journal for Jan. 1907. 8vo.
Athenæum for Feb. 1907. 4to.
Author for Feb. 1907. 8vo.
Chemical News for Feb. 1907. 4to.
Chemist and Druggist for Feb. 1907. 8vo.
Dyer and Calico Printer for Feb. 1907. 4to.
Electrical Contractor for Feb. 1907. 8vo.
Electrical Engineer for Feb. 1907. 4to.
Electrical Engineering for Feb. 1907. 4to.
Electrical Review for Feb. 1907. 4to.
Electrical Times for Feb. 1907. 4to.
Electricity for Feb. 1907. 8vo.
Engineer for Feb. 1907. fol.
Engineer-in-Charge for Feb. 1907. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 8, 1907.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Professor David James Hamilton, M.B. LL.D. F.R.C.S.E. F.R.S.E., Professor of Pathology, University of Aberdeen.

Certain Seasonal Diseases of the Sheep, and the Means of Preventing Them.

Introductory.—Perhaps a word of apology may be thought necessary by some members of my audience for the introduction of a subject which, on the face of it, may seem to be suited for a medical rather than for a general audience. While admitting that this might be so were the subject treated from a purely technical point of view, yet, in extenuation of my appearance here this evening, let me premise what I have to say by reminding you that the diseases in question are of great general scientific interest, and their study, owing to the pecuniary loss sustained by the country from the mortality caused by them, is one of the utmost economic utility. Further, the bearing they have upon the pathology and prevention of many human diseases is so intimate, that one cannot fail to see how the one may shed much light upon the other and lead to the study of many diseases of man, from a wide comparative, rather than from a narrow, exclusively human, point of view. The study of the cause of disease is simply one of the numerous sub-studies in the domain of biology, and one of enormous importance, and the more widely the subject is approached, the more likely will it be to lead to results of permanent value. Hence, from these various points of view, the theme I have chosen for my lecture may perhaps be looked upon as worthy of passing consideration by an audience so sympathetic with matters scientific, in the broadest sense of the term, as that of the Royal Institution always proves to be.

The above diseases have claimed my attention for many years past, first in a private capacity, thereafter under the auspices of the Highland and Agricultural Society of Scotland, and, lastly, since the year 1901, under the patronage of the Board of Agriculture and Fisheries. In the year 1901, the Board of Agriculture appointed a Departmental Committee to inquire into two of the diseases, namely, those known as Braxy (*Morbus subitarius ovis*) and Louping-ill (*Chorea paralytica ovis*), and our Report on these was issued in June

of last year. It gave an account of the work, so far as it had gone up to the date of publication. (" Report on the two Diseases of the Sheep, known as Braxy and Louping-ill." Board of Agriculture and Fisheries, 1906.)

Diseases in question form a Group.—The sheep is peculiar in respect of the many contagious diseases to which it is liable, and it is curious that heretofore they have not claimed more attention than has been awarded to them. Those with which we are more immediately concerned form a large group, the members of which are closely related, in so far as they are each caused by a specific organism having certain mutual affinities, and, apparently, of the same type as that of Tetanus. Several of the group have never, up till now, been recognised, and those whose characteristics have claimed attention have been investigated only in a perfunctory manner. Previous to the work of the Board of Agriculture Committee, little was known of most of them which could serve to explain their pathology and aetiology, or lead to their prevention.

So far as my own observations have demonstrated, the members of the group are comprised in the following :—Braxy (*Morbus subitarius ovis*), Louping-ill or Trembling (*Chorea paralytica ovis*), Malignant Œdema of the sheep, Blackquarter or Quarter Evil, the disease known as " Struck," and two diseases which, provisionally, I have named Disease " A " and Disease " B."

Each due to an Anaërobic Bacillus.—Some of them, such as Braxy, appear to be peculiar to the sheep, while others, such as Blackquarter, are common to it and to cattle. Louping-ill, although pre-eminently a disease of the sheep, is said to affect other animals, such as the calf, the pig, and the goat, but only on rare occasions. Each of them is caused by an anaërobic bacillus, having a great tendency to spore, and whose natural habitat is the intestine.

Their Periodicity.—One remarkable feature of these diseases of the sheep is that they occur periodically, that is to say, at stated times of the year. Certain of them, such as Braxy, Disease " A," Disease " B," and Malignant Œdema, prevail in the autumn and winter months, while others, and more particularly Louping-ill, are diseases of the spring ; all of them tend to vanish during the summer. They show themselves, almost to a day, each in its season, and vanish quite as regularly and mysteriously.

Areas Affected.—They prevail only in certain districts, and mainly along the west coast and southern counties of Scotland, and the northern counties of England, while the east coast of the whole of Great Britain may be said to be almost exempt from their ravages. Draw a straight line from the north of Scotland down to the south of England, and you practically separate the infected districts from the non-infected.

Braxy is a most destructive disease in Iceland, the Faröes, and the west coast of Norway ; in fact, it may be asserted that wherever

the waters of the Gulf Stream impinge upon the littoral of a country, there Braxy will be found to prevail. It is quite likely that other diseases of the group, as well as Braxy, infest such countries, although nothing is known of the matter.

Pecuniary Loss.—The pecuniary loss entailed upon sheep-farming districts afflicted by these diseases, directly and indirectly, is enormous. It has been calculated that our loss in Great Britain must amount to something like half-a-million yearly, and this, I fear, is really an under-statement of the case. So dreadful is the mortality in certain areas of Scotland that sheep-farming, as a profitable industry, is ceasing to exist.

It would be too large an undertaking to attempt even to outline the features of each of the diseases I have enumerated in a lecture of the present scope. I shall select two of them for the purpose of illustration, namely, Braxy (*Morbus subitarius ovis*), and what is popularly known as "Louping-ill," or the leaping disease (*Chorea paralytica ovis*).

Braxy—Symptoms.

Under natural circumstances the animal dies so rapidly that opportunity is seldom afforded of studying the disease from its commencement until its termination. All accounts, however, seem to agree that a short, quick, step is, perhaps, the first sign noticeable. The animal is off its feed, and is restless, with a tendency to lie down and get up suddenly, as if expressive of a certain uneasiness. Quite likely it is noticed that it does not rise so readily to the dog as others do. When the disease has been conferred experimentally, by inoculation upon a hind limb, I have found that the limb invariably hangs down in a paretic condition, the ankle is flexed, and the animal continues to roam about in a half-dazed condition, trailing the inoculated limb after it. The pulse varies between 30 and 35 per minute, and is often imperceptible in the extremities, the breathing is somewhat laboured and from 40 to 42 per minute, while the temperature runs from 105° to 108° F. Rumination is entirely suspended, and a crunching noise is sometimes emitted. The belly usually begins to swell, the back rises, the head is depressed, and the animal roams about in a listless manner ; then probably, if not enclosed, it will crawl away from its mates, take refuge in a cranny or nook, and finally fall over on its side. When this stage is reached, my experience leads me to conjecture that the fatal issue is not far off. Probably, within an hour or two, the animal is dead. The blood is said to be very dark and thick, and does not flow easily, but I think that undue emphasis has been laid on this as a sign of the disease. I have known a subject of natural Braxy bleed to death into its own stomach. When once the animal falls over, it passes into a semi-comatose state, and makes no further effort

to escape. It is often said that it seems to suffer from cramping pain in the abdomen, but my own observation seems to point to the symptoms of uneasiness being due, quite as much at least, to feverishness, and to the animal being in a half-delirious state. The swelling of the abdomen is often not at all marked until after death, when it ensues with great rapidity, a matter of a couple of hours being sufficient to render the abdomen tense and tympanitic. When inoculated experimentally, the subcutaneous areolar tissue of the thighs and abdomen can be felt to crackle on pressure at the time of death, and this also increases immediately after the animal has died. In some cases, there is evidence of diarrhœa—in fact, from the empty condition of the bowel after death, I am inclined to believe that diarrhœa or, at least, copious evacuation of the bowel, must be of common occurrence. The urine is said to be scanty, and dark-coloured, but I have not noticed, in cases where there was an absence of hæmorrhage into the muscles, or elsewhere, that the urine contained in the bladder after death presented any abnormality. Hogg relates that an animal, which he was carrying home on his shoulders, vomited, but this must be a rare symptom, as the paunch is invariably filled with food.

The disease when inoculated usually runs a course of from five to eighteen hours after the symptoms have declared themselves. The most rapid case I have noticed was one in which the animal lived for nineteen hours, dating from the time the virus was introduced. Some natural cases are said to linger for a few days, but I doubt if these are instances of Braxy. There is no more constant sign of the disease than the extreme rapidity with which the fatal issue ensues, and in this respect the malady has often reminded me of Asiatic cholera.

The incubation period cannot be determined in natural Braxy, but in inoculation experiments I have found it to be generally from forty-eight to sixty hours, often very much shorter. When the virus is sporing, and is injected simultaneously with acetic acid, not only does the attack prove more severe, but the incubation period is diminished.

The Organism which is the Cause of the Disease.—Throughout the carcase of an animal dying from Braxy a vegetable micro-organism is found usually in great abundance. The effusions into the various serous cavities of the body literally swarm with it, but that poured into the peritoneal sac always contains it in greatest abundance.

Not only, however, does it prevail in the effusions into serous cavities, but it is also found, at least after death, in the blood, a fact which is in a manner corroborated by the heart's chambers containing gas. That the presence of the bacillus in the blood is not merely a post-mortem phenomenon, is borne out by the observation that, in cases where the animal had been dead only a very short time previous to the making of the autopsy, and where as yet the carcase was quite warm, gas escaped from the interior of the heart. In respect of the

organism being found in the blood, the disease is at variance with Louping-ill, in which, almost without exception, the organism is absent from the blood, even though it may be present in myriads within the peritoneal effusion.

The kidney and the liver contain the organism, the kidney, as so often happens in bacillary diseases, sifting it out from the blood through the winnowing action of the glomerular capillaries.

It abounds in the blood-stained mucus found in the fourth stomach, and in the intestine it is present in overwhelming numbers. The part of the intestine in which it will be found in greatest quantity is probably the ileum, and more particularly in a portion in which there is not any fæcal matter. If a scraping be taken from the surface of an empty loop of this part of the bowel it will be found composed in great part of bacillus and shed epithelium. The epithelium desquamates in microscopic flakes, and becomes mixed up with mucus and the bacillus, these together constituting the thick half-liquid discharge which is found on the surface of the mucosa. In this respect the disease seems to correspond with Louping-ill, bovine Blackquarter, Struck, etc., which, as shown elsewhere, all appear to be intestinal in their origin. The intestinal mucus seems to be the natural habitat of the organism in this and other diseases of the same class; it grows upon it more readily perhaps than upon any other secretion of the body. Even in cases where Braxy has been induced experimentally by subcutaneous inoculation the intestine will be found swarming with the organism, clearly showing that the intestine and its contents afford peculiar facilities for its propagation.

The first description of the organism seems to have been given by Ivar Nielsen. In his work on the subject,* published in the year 1888, he described an organism which he found in the hæmorrhagic areas of the digestive tract, as well as in the capillaries of the various organs, and which he regarded as the cause of the disease. There seems little reason to doubt that the organism seen and described by him was that of Braxy.

The organism is an anaërobic bacillus, and is distinguished from all the others of this class (Louping-ill, Blackquarter, Struck, Malignant Œdema, etc.) by its comparatively small size and delicacy of contour.

The following measurements were taken from bacilli and spores growing in the peritoneal liquid. They were made immediately after removal of the liquid from the body and in the perfectly fresh state, without any drying, staining, or clarifying. The dimensions varied considerably, the largest rods being found among those which were not sporing:—

> Those not sporing: 2·84, 4·26, 5·68 and 7·1μ long by 0·7 to 1μ broad; those sporing, 4·26 to 5·68μ long; free spores, 1μ long.

* Bradsot hos Faret (Gastromycosis ovis), Tidsskrift for Veterinaerer, 1888.

It will thus be seen that, on an average, the rod comes up to something like half the long measurement of a human coloured blood-corpuscle, sometimes longer, at other times shorter. As found in the natural liquids of the body, it exhibits an extraordinary aptitude for sporing. The spore is smaller than that of any of the other anaërobic organisms leading a parasitical existence upon the sheep, and can often be distinguished from them by this feature. It is brown-coloured, highly refractile, and located towards the end of the rod, giving to the latter a somewhat lanceolate contour.

The rod is usually quite immobile, both when in the natural serous effusions of the body and in culture on artificial media. It may happen, however, that, under certain circumstances, it develops some amount of motility. The ends are rounded, but when the organism is stained, as with Loeffler's blue, certain chromatic points make their appearance, and if one of these does not happen to be at the extreme end, the unstained capsule gives the rod a somewhat tapering appearance. Owing to the presence of these chromatic granules the bacillus may present an irregular or half-digested appearance, even in an unstained preparation.

It may chance that nearly every rod is sporing, and not only may the spore be located towards one extremity, but exquisite drum-stick forms are met with, the delicacy of the rod under such circumstances in contrast with the well defined spore being a notable characteristic. When the spore is just beginning to show, it can be recognised as a little clear point; this enlarges, becomes more highly refractile, and eventually may protrude at one of the poles. It must be remarked, however, that such a drum-stick configuration is by no means diagnostic of Braxy; it is a morphological feature common to almost all the members of the group.

Two bacilli may often be encountered still united, either at an angle or in a straight line, but it is seldom that the rods are strung in chains, and chains with a zig-zag conformation are almost prohibitive of the organism being that of Braxy. Two or more bacilli may lie side by side and closely adherent, possibly overlapping, in which case they may resemble a bacillus of unusually great length.

The organism when grown on glucose-beef-tea has a great tendency to run into dense masses, in fact to clump, so that when it settles down on the sides of the culture tube the deposit has a distinctly granular appearance. On spreading out a drop of the culture deposit on a slide the same fine particles are visible with the naked eye, or with a pocket lens. On microscopic examination, they are seen to be not merely loosely held together colonies, but to constitute a dense feltwork not easily dissociated.

Both kinds of blood-corpuscle are invariably absent from the peritoneal liquid even when much blood-stained, and when the liquid is allowed to remain quiescent for a few days, the particulate matter settles down and leaves the liquid above clear but deeply laked.

When the peritoneal or other serous effusion is incubated at a temperature of 38° C., nearly the whole of the rods will be found to have vanished within forty-eight hours, while the number of free spores has correspondingly increased. In giving birth to a spore, under these circumstances, the mother bacillus seems to perish and is bacteriolysed.

Thus incubated, the spores may be retained for a matter of years. I cannot say for how many years, but I may affirm that the earliest samples of peritoneal liquid in my possession will still reproduce the disease with almost perfect certainty. The liquid does not undergo ordinary putrefaction, and the vitality of the spores is not influenced injuriously by the moisture. I generally keep the liquid in sealed tubes, but it may be preserved in sterile bottles equally well.

The reaction of the peritoneal liquid is alkaline as a rule, and abundant triple phosphate crystals are usually found in it. This fact probably accounts for the organism growing to such excess in this liquid within the living body.

Boiling of the spores for a matter of five minutes usually kills them, but they are able to retain their vitality without fail at a temperature of 80° C. continued for twenty minutes. Jensen* found that after the stomach of a sheep had been kept in dilute spirit for a period of seven weeks, he was still able to start a growth from it.

The bacillus stains with gentian-violet, perhaps more intensely than with any other of the aniline dyes, and for photographic purposes this reagent is possibly the most serviceable ; but fuchsin and Loeffler's methylene-blue give excellent results. The latter stains the chromatic granules very delicately, and, like the others, leaves the spore quite uncoloured. I would emphasise, however, that the view obtained of the unstained organism in its living state and suspended in the serous liquid in which it has been growing, is incomparably truer to nature than that of any stained and clarified preparation.

The bacillus when taken fresh from the peritoneal liquid sometimes gives a reaction by Gram's method, but a positive result is uncertain, and reliance should not be placed on this reaction as a point in the diagnosis. More frequently than otherwise the reaction fails completely, and I have never obtained it in beef-tea cultures.

Its Culture.—When cultivated on glucose-beef-tea under oil, all germination should be over at the end of thirty-six hours' incubation, and the organism in great part have settled down on the bottom and the sides of the tube in fine particulate, sand-like masses. If the tube has been retained in a sloping position, the deposit takes place in a fine line all along its dependent aspect, and at the bottom. The part of the deposit at the bottom of the tube has a slimy appearance

* Deut. Ztschr. für Thiermedicin und vergl. Path., Bd. viii., 1896, p. 265.

with these fine granular particles dispersed throughout its midst. The whole constitutes a somewhat viscid mass, which can be stirred up only with difficulty, rising in a central column with serpiginous coils, and settling down again rapidly when the medium is brought to rest.

When cultivated by the puncture- or stab-method on glucose-agar medium under anaërobic conditions, and at 37–38° C., a luxuriant growth shows itself rapidly along the track of the needle; copious gas-bells are liberated, which soon tear the medium in pieces and force it up against the cotton-wool plug. If the disengagement of gas is slight, mere slits or tiny rents may be formed in the medium, along which the bacillus tends to propagate. Owing to the amount of gas evolved and the consequent laceration of the medium, the growth is seldom characteristic.

By far the most distinctive culture is that made anaërobically, under oil, upon glucose-gelatin at a temperature of 21° C. As might be expected, the organism under these circumstances germinates slowly. From a week to ten days must intervene before the culture can be said to be diagnostic, but when this time has elapsed, there is no method by which the character of the growth can be seen to such advantage. The peritoneal liquid, after being engrafted upon glucose-beef-tea, is purified by subjecting the medium to a temperature of 80° C. for twenty minutes; thereafter, the mixture is incubated at 38° C. for thirty-six hours. The organism thus generated is next inoculated upon the glucose-gelatin by the puncture- or stab-method. The oil which overlies the surface of the gelatin is no barrier to accomplishing this, for, in passing through it, the organism adheres quite well to the platinum needle and is carried down with it to the depths of the medium.

Retained at 21° C., signs of germination can be recognised by the third day. The needle-track becomes more evident, and soon, along its course, colonies begin to show which slowly increase in size. From the eighth to the tenth day, the growth is at its best, and has the following appearance: It is most copious from two to three centimetres below the surface, but extends along the whole track. Little cup-shaped areas of liquefaction come to be arranged at intervals of about a centimetre, one above the other, concave and hollowed-out above, convex below. They closely resemble diminutive cups filled with the liquefied medium. From the hollow upper surface of each of these, coarse arms are thrown upwards and outwards, almost as if something solid had been dropped into the cup and had occasioned a splash. The arms are comparatively disjointed in their course, and have none of the continuous thread-like character of some other members of the group. I do not know of any other organism which grows in the same fashion. It differs from a culture similarly treated of Louping-ill, Black-quarter, Malignant Œdema, Disease "A," or that of Disease "B."

Louping-Ill.

The disease is one which, in contrast to Braxy, prevails chiefly during the spring months, although sporadic cases occur in the autumn or early winter. The period extending from the middle of April to the middle of June may be said to limit its occurrence in an epidemic form, the middle of May marking the zenith of its intensity. The valley of the North Tyne is one of the most severely smitten areas, and it was greatly through the interest shown in the matter by the Duke of Northumberland that the Board of Agriculture Inquiry was undertaken. The loss from the disease in the West Highlands of Scotland is tremendous, while all over the southern counties of Scotland its ravages are well known. On the east of Scotland, however, from the extreme north down to the Lothians, the disease is so rare that some of those engaged in sheep farming hardly know what it is.

Symptomatology.—The symptoms can be divided into three distinct stages. In the first, the animal, as in others of these contagious diseases of the sheep, is noticed to be somewhat dull. It may separate from the rest of the flock, stand apart in a listless fashion with drooping head, and be off its feed. It assumes subsequently a reeling gait as if intoxicated, and will lean against a dyke or fence for support. A dazed expression is often noticed in this stage as if the animal were in the initial stage of a fever.

These symptoms may last for a period of from two to three days, when the sheep falls over, quite unable to support itself, or to regain, even temporarily, the upright position. The limbs are now spasmodically convulsed at intervals of perhaps a minute to a couple of minutes. The movements are mostly of a galloping character, and so incessant that the turf becomes worn in the area of their excursion. During the intervals, quivering or trembling movements are perceptible, hence the name "Trembling Disease" sometimes applied to it in the Western Highlands. The neck is drawn back as a rule, but the muscles of the neck are not intermittently contracted as in the case of the limbs. The temperature in this, the second stage, may go up to 105° to 108° F., and the pulse-rate and respirations are increased in number. The muscles of the jaws and those concerned in swallowing are not usually involved, nor is there any squint, and the intelligence of the animal appears to be little if at all impaired. It will nibble grass, and it swallows milk with avidity and without impediment.

There are cases, however, which assume quite a tetanic character. In these the muscles are in a state of rigid spasm, while, it is said, although I have never seen an instance of this in the natural disease, the muscles of mastication are in a like rigid condition, and in most respects the phenomena resemble those of idiopathic tetanus. Shep-

herds in Louping-ill districts will tell you of the occurrence of what they term "lockjaw" among their sheep during the spring months, and from the examination of the carcases of those dying with such symptoms, and for other reasons, I have come to the conclusion that this disease is simply a severe variety of Louping-ill.

The animal may succumb in this second or convulsive stage, apparently from acute toxic poisoning. It passes into a semi-comatose state, the temperature sinks, and the convulsive spasms become weaker and weaker, preliminary to the fatal termination.

Should the disease not prove fatal within a matter of a week or less, then there is every likelihood of its passing into the third stage characterised by the following phenomena: The convulsive spasms of the limbs, so notable in the second stage, now give place to a condition of more or less complete motor paralysis; the limbs are outstretched and limp, while, if the sheep be held up by the fleece, they hang down relaxed and listless, and the animal is quite unable to use them for purposes of support. When the feet touch the ground the fetlocks are knuckled under in a perfectly helpless manner.

Painful sensation, so far as one can judge, does not seem to be affected to any appreciable extent even in this stage, but the reflexes from the limbs appear to be blunted, and in certain cases annulled.

The whole appearance of the animal closely resembles that of a person suffering from post-diphtheritic paralysis, the nature of the paralysis being essentially motor, and affecting the limbs by preference, although it seems to differ from the diphtheritic form in the fact of the palatal muscles being spared. During the course of the disease the animal is able to swallow liquid nourishment without impediment. The intelligence in this third or paretic stage often remains uninfluenced. The animal recognises objects about it, and will bleat when a companion sheep is removed into a neighbouring pen. It will eat fodder if offered to it, but is quite helpless to seek for such of its own accord, and, as a consequence, in many instances, seems to die from starvation as much as from any other cause.

In this highly paretic state it may live for weeks, recovery seldom, if ever, taking place, even though the animal may have been fed artificially. Some subjects of the disease are said to make a partial recovery, probably only one limb remaining permanently crippled. I have always been somewhat doubtful, however, of the diagnosis in such cases; the symptoms look to me more like the effects of spinal abscess, which is very common, at least among lambs, during the spring months.

Morbid Anatomy.—On examining the carcase in this disease one of the most notable features is the absence of lesion which might serve to localise the peccant agent in any particular organ. Gas begins to develop in the abdomen very soon after death, and within a few hours, in certain instances, the wall of the abdomen may assume a greenish tint.

The abdominal cavity, as a rule, contains an excess of serous liquid, but this is not always the case. Sometimes the liquid is thick, muddy-looking, and, it may be, tinged with blood, while at other times it is quite clear and limpid, or, at the most, a delicate coagulum separates from it. In no case have I seen peritonitis or pleurisy accompany the disease, and hence the conclusion seems inevitable that the organism which causes it is not possessed of inflammatory tendencies.

A few punctiform hæmorrhages may be met with along the course of the intestine, but with this exception all the viscera may seem to be quite healthy. Nor have I seen any evidence of meningitis or other disease of the central nervous system.

The microscopic examination of the natural liquids and of the organs of the body proves equally disappointing. Thus the blood is free from any micro-organism which can be detected microscopically, and when cultivated aërobically or anaërobically remains equally barren. The cerebro-spinal liquid and nerve centres are devoid of any parasite which might be taxed with a casual relationship, and, for these reasons, the pathology of the disease for long remained to me a problem fraught with obscurity.

During the first season (1902), in which we conducted our observations at Kielder, in Northumberland, we noticed, however, that two kinds of case were brought in to us. In the one there was an excess of peritoneal liquid, which was also turbid and sometimes slightly stained with blood. In the other the peritoneal liquid was perhaps not in excess, or, if so, it was quite clear and limpid.

On microscopic examination of the turbid liquid it was found to be teeming with a large coarse-looking rod-organism having a great tendency to spore, while in the case of that which was clear and limpid not a bacillus was to be detected. The rod in question had a close resemblance to that of Blackquarter, and, at first, we supposed that we had to do with two diseases running side by side, namely, Blackquarter and true Louping-ill.

On incubating the clear peritoneal liquid, however, in sealed tubes, I found invariably that, in the space of twenty-four hours, it became turbid, and when the tubes were opened, a whiff of gas escaped with a small explosion. On examination of the liquid microscopically, it was now found to be swarming with the same large sporing rod present in the liquid which was turbid.

This threw a new light on the whole pathology of the disease. There were evidently two forms, one in which the peritoneal liquid was so full of a notable micro-organism that it could be readily detected by microscopic examination, the other in which the organism was so sparsely distributed in the liquid that it could not at first be detected microscopically, but in which the same specific rod developed abundantly on the liquid being incubated at a body temperature.

The turbid liquid was found in those animals which died within

a few days, and with acute toxic symptoms, the clear liquid in those which lived longer and in which the disease went through all its three stages.

When the animal was slaughtered during the height of the malady the peritoneal liquid was always of the clear variety; the turbid variety was found only where the animal died a natural death.

Description of the Organism.—The organism (*Bacillus choreæ paralyticæ*, Hamilton) possesses the following characteristics: It is a large coarse-looking rod, sometimes elongating into a thread, or, it may be, a chain of rods. The actual measurements, as taken directly from the organism in the peritoneal liquid, I have found to be: In one case, when not sporing, $5 \cdot 6 \times 1 \cdot 4\mu$, $7 \cdot 0 \times 1 \cdot 4\mu$; and, when sporing, $4 \cdot 2 \times 1 \cdot 4\mu$. In another case, when not sporing, $2 \cdot 8\mu$, $4 \cdot 2$, $5 \cdot 6$, $7 \cdot 0$, $9 \cdot 8$, and $11 \cdot 2\mu \times 1 \cdot 05$ to $1 \cdot 4\mu$. The spores in the latter case measured $1 \cdot 05$ to $1 \cdot 4\mu$ in length. Its dimensions, therefore, like all the members of the group, vary considerably, chiefly accounted for by the fact that involution forms are almost always present. The ends are rounded, and it is possessed of feeble motility. It has a considerable tendency to spore; the spore is located at its centre or at one end; and occasionally, especially after incubation in its native liquid, it assumes a drumstick configuration, indistinguishable from that of the *Bacillus Tetani*. Most of the usual aniline dyes stain it readily, and the colour is not discharged by Gram's process. These staining reactions hold good of the organism both when taken directly from the carcase and when in culture.

It is a strict anaërobe, and grows on various media, but most characteristically on alkaline glucose beef-tea and glucose-gelatin, each covered with olive oil. The glucose beef-tea becomes turbid after four to five hours' incubation at 38° C., and continues so for days while incubation is proceeding. If removed from the incubator after, say, four days' growth, the culture begins to settle down slowly at the bottom of the tube in a fine precipitate of greyish colour. It does not tend to agglomerate in a granular form, as in the case of Braxy, nor to become attached to the sides of the tube. During the process of germination much gas is evolved which possesses a distinctly putrefactive odour.

Examined microscopically, the culture is found to be composed of thick stout rods, somewhat longer possibly than in the original peritoneal liquid, slightly motile when first removed from the incubator, losing this characteristic later on. Their actual measurements were found to be: $4 \cdot 2 \times 1 \cdot 4\mu$, $5 \cdot 6 \times 1\mu$, $7 \cdot 0 \times 1 \cdot 4\mu$, and $14 \cdot 0 \times 1 \cdot 4\mu$.

The ends are rounded, but usually the growth is free from spores. Even when the medium has been strongly alkaline to begin with, the Braxy organism will render it acid after a few hours' growth. The Louping-ill bacillus acts in a like fashion, but slower, and this probably accounts for germination being more protracted in the latter than in the former case.

Surface cultures on glucose agar grow luxuriantly; along the central streak formed by the inoculating wire, and from each side of this, somewhat arborescent processes extend outwards, rendering the sides of the central streak very irregular. Such cultures are not particularly diagnostic.

The stab-culture on glucose-gelatin, grown at 21° C., however, is very characteristic. In order to observe it, the gelatin should be covered with olive oil, and the inoculation made with the platinum wire through this. Quite a week will elapse before the growth reaches its best.

From the surface there passes downwards a grey-coloured streak for a distance of a centimetre or so, but underneath this the culture becomes more expanded and often flattened out in a single lamella whose borders are occupied with a series of loop-like festoons. Brush-like arms are subsequently thrown out from the central streak. It liquefies the gelatin in course of time, but slowly, and when liquefaction around the culture is complete, the organism falls down in a greyish-coloured deposit. Gas-bells may or may not be liberated; sometimes there is a single gas-bell at the deepest part.

The Intestine the Portal of Entrance in the case of all the members of the Group.—Not only in the case of Louping-ill, but in that of all the other members of the group, the intestine seems to be the portal through which the organism gains entrance, and the fact that the peritoneal cavity contains it, more than any other cavity, or any organ in the body, is thus readily enough explained. The peritoneal cavity is evidently the great lymph-sac of the body. Not only do its walls contain the lymph-vessels in connection with the intestine, but those lymph-vessels returning from the hind limbs have evidently a free, although perhaps circuitous, connection with its interior. The organism of Louping-ill fails to propagate on the blood during life, but grows freely enough on the secretion of the peritoneal membrane, hence the large quantity of bacillus usually found in the liquid.

When the organism of the disease is inoculated subcutaneously in the sheep, death takes place so suddenly, evidently from acute toxic poisoning, that time and opportunity are not afforded for the development of the nervous phenomena. Where the organism, on the contrary, is introduced into the alimentary canal, and where the animal takes the disease, but lives over, it may be, several weeks, the nervous symptoms are well developed.

Production of Immunity to the Disease.—It would be apart from the object of this lecture to enter into the explanation of the production of immunity to the disease too minutely, and those of my audience interested in the matter are referred to the Board of Agriculture Report (*loc. cit.*) for a full statement of the facts.

The bacillus of Louping-ill can be administered to sheep by the mouth with impunity throughout the greater part of the year. As the susceptible months are approached, however, namely, March,

April, and May, the danger of doing so is extreme, and a fair proportion of the animals so treated will die with all the classical symptoms. At other times of the year, the organism may, and does, pass along the intestine of the sheep without exerting any harmful manifestation. Indeed, at these times, it exerts a most beneficial influence in rendering the animal immune. My whole method of preventive treatment, as will be seen from the Report, is founded upon this principle, namely, the administration of the organism by the mouth at a time of year when the sheep is not susceptible to the disease. The organism multiplies in the intestine, but apparently, at these times, is prevented by some means from crossing the barrier afforded by the intestinal wall, and so does not find access to the peritoneal cavity. Nevertheless, it undoubtedly immunises the animal, and protects it from an attack of the natural malady. Out of a total of 1340 sheep treated by us according to this method during the year 1904–5, in the very worst districts of Scotland, and where we often shifted the animals deliberately from "clean" to "foul" pasture, we had not a single death from Louping-ill. A culture was administered to the animals mostly during the month of January, and not in a single instance did we find that it had, when administered thus early in the year, a baneful influence, and yet it acted as a most effectual protective against the natural disease.

The subject of immunisation through the intestine, in the case of contagious diseases of man which are of intestinal origin, has not, it seems to me, met with that attention which its importance claims.

How the immunisation in the sheep is effected, I will not at present venture to explain. It may be that the epithelium of the intestinal mucosa becomes resistant to the passage of the organism and thus prevents its gaining access to the peritoneal cavity, or it may be otherwise. To elucidate the problem will require much patient investigation.

Strangers visiting a foreign country and residing in towns where typhoid prevails endemically, are more likely to contract the disease than the regular inhabitants. May the explanation of this not be that the latter are habitually drinking typhoid, and have become immune to the fever, without actually suffering from the malady? We know, as a fact, that immunity to the intensely poisonous substances, ricin and abrin, may be brought about by administering graduated doses by the mouth. It seems, therefore, rational enough to suppose that in the case of several diseases of man, especially those in which the intestine is primarily concerned, a like immunity may be established through the alimentary canal.

Whether the dead bacillus given in this way has a like effect I have not as yet determined, but I think it probable that, if employed in sufficient quantity, such may be the case. The immunising principle is evidently contained in the protoplasm of the bacillus, and there does not seem any very evident reason for believing that it may

not exert its beneficial influence if administered by the mouth, and in a condition fit to be absorbed.

During the months in which the disease is rampant, however, the protective influence of the intestinal wall is lost in a large proportion of cases; the organism gets over into the peritoneal sac, fructifies within it, and kills the animal.

The spores being voided with the dejecta are taken up from the pasture by a fresh host, and the result seems to depend very much on the season of the year at which this happens. Should it occur during the susceptible months the danger is extreme, while, at other periods, it is practically nil. Nevertheless, the younger the animal the more liable is it to the disease, and hence we may suppose that the ingestion of the organism at an early period must have effected its immunisation.

Cause of Periodicity of Louping-Ill.—The insusceptibility of the sheep to the disease at certain times of the year seems to depend directly or indirectly on the condition of its blood. The blood of the sheep during the spring months of the year usually constitutes an excellent medium of culture, while at other times it, as a rule, is not only inimical to the growth of the bacillus of Looping-ill, but is intensely bacteriolytic to it. So that if, say, the blood of the sheep during the month of July, be mixed *in vitro* with a culture of the bacillus, the mixture covered with olive oil, and the whole incubated at a body temperature for twenty-four hours, probably every bacillus will be found to have vanished. During the susceptible months, however, the organism multiplies and spores on the blood of the sheep perhaps better than upon any other medium. It is evidently this inhibitive action on the part of the blood during most of the year which prevents the organism growing upon it or upon the peritoneal liquid, for there is no reason for believing that the organism gets into the alimentary canal with more facility during the months in which the sheep is susceptible than at other times. The peritoneal liquid apparently does not possess solvent powers to anything like the same extent as the blood, and where the bactericidal action of the blood is lessened, as during the months of susceptibility, the organism is enabled to pass the wall of the intestine, to fructify on the peritoneal liquid, and to kill the animal acutely with all the symptoms of toxic poisoning. The peritoneal liquid in such instances is thick and turbid, and contains the organism in abundance.

Cause of the Toxic Phenomena.—It may happen, however, that in other instances the blood still retains sufficient bacteriolytic properties to dissolve any of the organism which gets into it, although it is not sufficiently inhibitive to prevent a certain exodus of the bacillus from the intestine. Those bacilli which enter the blood stream, under these circumstances, are still bacteriolysed, and apparently the same thing happens, but to a minor extent, within the peritoneum. The blood in such animals will be found free from bacilli and the

peritoneal liquid may be clear and limpid, and not show any of the organism until after being incubated. These cases run a chronic course, and in them the nervous phenomena are most marked. The difference between the acute and the chronic case seems to depend upon the rapidity and volume with which the bacillus gets through the intestinal wall. In the chronic case the number is evidently small, and can be dealt with by the solvent action of the blood, while in the acute case the number is so great that the animal dies from rapid toxic poisoning. All seems to depend upon the condition of the blood. If it be in its usual state of antagonism to the growth of the bacillus, such as prevails during the greater part of the year, apparently the bacillus cannot leave the channel of the bowel. If, on the other hand, this salutary property be weakened but not annulled, a certain leakage as it were takes place, but still the number of bacteria which find entrance to the blood-stream or peritoneal cavity is small, and they can be got rid of by the bacteriolytic action of the blood-plasma or peritoneal liquid respectively.

Through the toxines set free from the bacteriolysed organism, in the latter case, a condition of chronic toxic poisoning of the animal ensues. If, again, the blood-plasma has lost its bacteriolytic protective action completely, the organism not only passes the barrier constituted by the wall of the bowel, but fructifies on the peritoneal liquid, killing the animal with symptoms of acute toxic poisoning.

It would thus appear that the sheep is a remarkable animal in that its blood is highly protective at certain times of the year, while at other times this protective influence is more or less completely lost.

Whether such a relationship exists in a modified form in the human blood with regard to certain pathogenic bacteria may be a matter of question. No one has inquired into the subject, but, judging from analogy, there seems little reason to suppose that such an outstanding quality occurring in one mammal is not at least represented in another. The fall of the leaf and the spring of the year have always been held to be seasons of great susceptibility in man to certain contagious and infectious diseases. May it not be that the natural bactericidal qualities of the blood, as in the case of the sheep, are lessened at these particular seasons ? various pathogenic organisms being thus encouraged to grow upon the tissues or it may be upon the surface of the various mucous membranes.

Prevention.—When any one of the before-mentioned diseases is thoroughly established, remedial measures are of little avail. In the case of Braxy the course of the malady is so rapid that opportunity is seldom afforded for the administration of what might be regarded as curative agents, and in that of the others nothing in my experience has the slightest effect in staying the course of the disease.

All efforts at amelioration of these scourges, therefore, must be directed to prevention, must be of a prophylactic nature. From

the fact that Braxy occurs almost exclusively in one-year-old animals, it would seem that by some means nature renders the animal immune to the disease during its first year of existence, and, manifestly, if we could imitate the means adopted by nature we might have every hope of saving the lives of a large number of animals which otherwise would be doomed. It is evidently those animals which have not been rendered immune before the advent of the Braxy season which fall victims to the disease, and hence all the more reason why we should endeavour to forestall nature and insure that this much-to-be-envied condition of immunity is established in every instance before the susceptible period commences. The same line of argument holds good of the other diseases of the class.

How to bring about this condition of immunity is accordingly the prime factor to be considered by way of treatment. The favourite means in practice, in the case of other contagious diseases, is that of injecting subcutaneously some of the blood-serum obtained from an animal rendered artificially immune. The serum taken from the immune animal has the property of conferring immunity on the fresh host. This method, however, is impracticable in sheep-farming operations. It would require an enormous amount of immune serum to satisfy the demands of even a limited district, and, besides, it necessitates the use of a subcutaneous injection syringe, an instrument constantly liable to go wrong in unskilled hands.

Then there is the method of injecting the organism subcutaneously which is the cause of the disease, but this again requires the use of a subcutaneous syringe, and, besides, is most uncertain and dangerous in its application.

What has proved in my experience the most practicable and successful method is that of administration of the organism by the mouth. Nature evidently brings about immunisation by the organism being taken up from the pasture, passing into the stomach and intestine, fructifying in these organs, and throwing off a poison which, getting into the blood, has the effect of acting upon it and rendering the animal proof against an attack of the natural disease.

Now, it is evident that this means of protecting the animal might quite well be imitated artificially, and that, if practised at a season when the diseases in question do not prevail, might be employed with little risk of killing the animal by communicating a fatal attack of the malady. Such is the system I have been pursuing for the last two years, and, as time goes on and I am getting more experience from the experiments which are being made, I feel justified in expressing the conviction that we are at least on the right track. It must, however, be clearly understood that all our trials of the system as yet are purely experimental. The lay mind, and especially the sheep-farmer mind, is inclined to regard all such investigations from a hard-and-fast point of view, quite oblivious of the fact that sub-

stantial knowledge on matters of this kind is not to be acquired in a day, but requires much patient observation over a long period before any reliable outcome can be obtained. It is my urgent plea, therefore, with all those concerned not to take these preliminary experiments as samples of the ultimate success or failure of the treatment, but rather as an index of what the best method of administration may come to be. The subject, it must be remembered, is one which is practically new to Science, and is one of the most involved in the whole range of biology. In any such inquiry certain experiments will succeed and others will fail, and it ill becomes us to sit down and weep over our failures.

Preparation of Drench.—The method of preparing the drench is as follows: The particular organism which is the cause of the disease is isolated in a pure state from the abdominal, or peritoneal liquid, as it is termed. This is afterwards grown artificially on a liquid medium, and a certain quantity of the culture administered to each animal. Whether a single administration is sufficient, or whether the preventive is more effective when administered twice, remains as yet only partially determined, but my experiments seem to indicate that a single dosing is sufficient.

In carrying out any treatment of this kind, one of the first points is to discover what the animals in a particular district are suffering from. Braxy is a common term, employed by those in the sheep industry to indicate practically any disease from which sheep die in the autumn and winter months, and although there is less liability to error in the case of Louping-ill, yet there is again the same tendency to include all those diseases which occur during the spring months under this designation. It is obvious, therefore, that, unless opportunity has been afforded of ascertaining, with certainty, what the mortality in a particular district is due to, we are working quite in the dark in recommending a remedy. This has been forced in upon me by the experience of the last few years, and accordingly, before preparing the drench for any particular locality or farm, I make it a custom, if possible, to procure some of the peritoneal liquid from some of the animals which have died, in order to make certain what the disease or diseases are with which we are dealing. By the examination of the peritoneal liquid, an excellent guide is afforded to the diagnosis, and in order to avoid any source of error, I always endeavour to prepare the drench from animals which have died on the particular farm where the experiment is to be carried out.

Results of Treatment.—Our results have, accordingly, gone on improving, from year to year. The last experiment with Louping-ill was made during the spring of 1906. Over the worst districts in Scotland, and under the most trying conditions, we administered the drench to 1340 first year's sheep, or "hoggs," as they are termed, and out of these we had not a single death from Louping-ill.

Our latest experiments with Braxy commenced last August, and were spread over Scotland, England and Ireland. As sheep die from several diseases of the class under consideration, our endeavours this last season were centred on testing whether they could be immunised to several of these simultaneously. I may say that this is a most important question from many points of view, and one which as yet is quite undetermined. Were it possible to accomplish this in one operation great trouble would be saved in gathering the sheep and administering the remedy. From a scientific point of view as well this question of multiple immunisation is of the most extreme interest and importance.

The drench in these latest experiments was made up from five different kinds of bacilli, all found in the peritoneal cavity of sheep dying in infected districts, namely, Braxy, Louping-ill, Malignant Œdema, and what I have named, for the time being, Disease "A" and Disease "B." The drench was administered mostly during the latter half of August, and a careful record has been kept by me of all the deaths which have occurred.

This winter, as we all know, has been one of unusual severity, perhaps the most severe we have had for the last fifty years, and a certain proportion of the mortality which has ensued may be accounted for by exposure to its inclement influence, and to the diseases, such as pleurisy, pneumonia, peritonitis, etc., which are incidental to such severe climatic conditions. The mortality in Scotland among undrenched sheep in the same districts as those in which our experiments were conducted, has been truly enormous. In accounts of it which have been sent to me I hear that 50 to 80 per cent. is common enough, indeed more than it has been for many years past.

So far, I may say, our experiments in Scotland and Ireland have been wonderfully successful. The mortality from all causes will not, I think, come up to more than 3 to 4 per cent. This is a great improvement upon anything we have had as yet, especially when the character of the weather is taken into consideration. What is still more remarkable, however, is the fact that Braxy and the other diseases, against which the animals were protected, have virtually disappeared from those treated by us. the mortality being accounted for by accidental causes or diseases beyond our scope. Among animals leading an outdoor existence and fed in the wildest and bleakest districts in our country, there will always be a certain mortality, but I am in hopes of reducing the present low figures to an even lower level next season.

It is always to be remembered, moreover, that shepherds usually place to our credit deaths from accidental causes which are quite beyond our control.

It is only in England, on the Duke of Northumberland's property, that the mortality has been at all excessive, and here, I must confess, the method of prevention, on the face of it, looked as

if it had been a failure. This is the more to be regretted as his Grace has all along manifested the liveliest interest in the investigation, and last year had planned an experiment which might be considered to be of a crucial and conclusive nature. A matter of sixty sheep were bought in the market: they were treated twice with our drench, and were placed subsequently on three farms with a most unenviable notoriety for these diseases of the sheep, at Kielder in Northumberland. Nevertheless, the mortality among them has been large, or rather was large at first, but for the last three months or so it has almost ceased.

On the same three farms and on a fourth immediately adjacent, we also treated a fairly large number of the ordinary or native sheep, and again there was a large mortality.

This result is remarkable when contrasted with the treatment in Scotland and Ireland, and must be capable of explanation. I may say, to begin with, that I had not had an opportunity of ascertaining what disease or diseases prevailed in this particular district of Northumberland during the autumn and winter months. Our Committee had made a thorough study of those which prove so fatal in the spring, but our knowledge was restricted to these. We were working, therefore, in the dark so far as knowing what disease we had mainly to contend with. The farmers themselves are not quite sure about it. They call it sometimes "Braxy," but more frequently "Sickness."

I have gone into the matter so far very thoroughly, and intend bringing it to an issue later on, the main result of the inquiry at present being that the animals which succumbed on these four farms have nearly all died from the same disease, and that this is certainly not Braxy. Indeed, I am sceptical of Braxy prevailing in this district to any great extent.

What, then, is the disease? My own impression—although I should like to be guarded in this opinion until an opportunity of further investigation is afforded—is that the disease which accounted for the mortality in this experiment is what is known as "Black-quarter," a disease belonging to the same class as all the others.

If this supposition be correct, it shows, as I before remarked, what necessity there is for thorough inquiry into the diseases prevailing in a district before attempting to prevent them. It will not be until land-owners and sheep-farmers unite to further the aims of such investigations that the rational means of preventing these diseases will be perfected. An inquiry of this kind cannot be carried on without funds, and surely where such a large commercial undertaking is concerned, it behoves the country to do all it can to ameliorate a state of matters which is bringing ruination upon the best sheep-farming districts, and which is one of the chief causes of their depopulation.

[D. J. H.]

WEEKLY EVENING MEETING,

Friday, March 15, 1907.

Sir William Crookes, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor George Lunge, *Hon. M.R.I.*, of Zurich.

Problems of Applied Chemistry.

It is one of the greatest honours I could covet to stand up in a room hallowed by the shades of some of the heroes of Natural Science, and to be privileged to speak on the theme I have selected. I feel that I ought to justify my claim to do so by the interest and importance of the subject. But I think it right to say at once that the title of this discourse, "*Problems of Applied Chemistry*," is in effect much too comprehensive, and should not be pressed home. A mere enumeration of such problems would be both tedious and useless; and how could anyone think of treating all of the more important of these problems within the space of sixty minutes? I shall have to confine myself to merely a few of those matters which might be fairly brought within the compass of the title of this lecture.

"Applied," in the narrower sense of "Industrial," Chemistry—means the pressing into service of chemical principles for practical purposes, such as the extraction of valuable matters from the three realms of Nature, and the conversion of such matters (if they cannot be put to any direct use) into numerous other useful substances. The science and art of the engineer are intimately interlaced with those of the practical chemist, so that it is difficult to say which of the two has to borrow most from the other in order to carry out his objects in the most efficient manner. So much is certain—an industrial chemist cannot exist, as such, without availing himself of the resources of the constructive arts. Even the laboratory of the purely scientific chemist in these days contains numerous mechanical appliances, without which (to quote only one instance) it would have been impossible to effect the glorious discoveries made in the laboratory of the Royal Institution, in the past generally, and more particularly in quite recent times. But what could the chemical *manufacturer* nowadays do without such assistance? The practical, as distinguished from the scientific, chemist possesses, or is supposed to possess, sufficient knowledge and experience to see to the working of machines and to minor repairs without calling in an engineer, save

in difficult or complicated cases, or where the work is carried out on such a scale that the services of a specialist are required, and that it pays to employ one. If a chemist lacks such knowledge, or possesses it in too limited a degree, he will have to be content to remain in the laboratory and to do the ordinary "testing," which is both a tedious and a badly-paid occupation. I am told that in this country this is far more frequently the case than either in America or on the Continent; but I must refrain from pronouncing an opinion as to how far this is due to the ordinary training of the chemists, and how far to the jealous reluctance of many unstudied managers to afford the chemist access to the real factory work, lest he acquire sufficient routine in that work to supplant themselves.

Well, granting the opportunity, how can a chemist gain experience in conducting operations on a large scale? Various ways are open for this purpose. In former times the chemical manufacturer (who only in very exceptional cases deserved to be called a real chemist) learned his trade, both on the chemical and the engineering side, as far as it was indispensable; but he learned it simply "by rote," as the saying goes. He would enter a factory as apprentice or volunteer, and there he had an opportunity of witnessing, not merely all the chemical operations, but also the building of sheds, the setting of pans, and of steam boilers, the erection of the simple machinery of those days, the construction of furnaces for chemical purposes, and similar matters, and he was expected to use his hands like any ordinary workman. If he was clever and industrious, he learned in the course of years, not merely to direct the various operations at the works, but also to make improvements in details and, perhaps, if all went well, in more important matters. To be sure, it is notorious that this never took place without large sums of money being thrown away, either in the form of misshapen or faulty apparatus and machinery, or of spoilt chemicals, and so on. And this happened to the unstudied "practical man," who, through family connexions or by mere chance, had stumbled into chemical manufacturing, as well as to men who had studied the science of chemistry, and who desired to apply the knowledge thus gained to the execution of some well-known process, or to the working of some laboratory invention on a large scale. Those men who possessed a scientific foundation, were, in their turn, compelled to learn the technical side of their profession by dint of practice, just as the tailor has to learn the art of making clothes and the barber the art of shaving. A man of scientific attainments had certainly, even in the olden times, a clear advantage over the mere "practical man." He was able to make a chemical examination of his first materials, of the intermediate products, and of the finished merchandise. Sometimes, although by no means invariably, he could more easily manage to ascertain the causes of disturbances in the manufacturing processes, and to put these right again; and often he was able to effect savings and improvements in these processes. But

this advantage only held good in such cases where the chemical reactions, as such, came into play; whilst in those cases where the mechanical side prevailed, the studied chemist, for the most part, showed less quick insight and resource than the unstudied foreman or manager, with the ultimate result that he wasted quite as much money over failures as the others did. And it is not difficult to understand that such failures happened even to eminent scientific chemists who attempted to carry out their ideas on a large scale. Their ignorance of the proper ways for translating a laboratory method into a manufacturing process was at that time remedied only in exceptional cases by the advice of an engineering expert. If such an expert was actually consulted he often did not succeed in his task, because chemical operations were outside his province, and because he tried to apply certain means, suitable to cases with which he was familiar, in cases of an entirely different nature. On the whole, the chemical manufacturer of those days felt at every turn the pinch of a mere routine experience, gathered piecemeal during the course of his daily work.

This state of affairs continued until about the middle of last century, or a little later—to be sure with marked differences in details, both in respect of local conditions and in the nature of the manufacture. To begin with the latter, we perceive that then, as indeed from the very first, several branches of chemical manufacture had their full share in the progress of mechanical engineering. To name only a few of these, we may refer to the vast field of metallurgy, to the manufacture of coal-gas, and to the extraction of sugar from beet-roots. But precisely in these cases we cannot find much proof of co-operation between the chemist and the engineer, for the former had very little to say in these industries at that time, and in most of these establishments his services were dispensed with, whilst all the resources of mechanical engineering were fully applied. The managers of such works did not come from the ranks of the chemists, but from those of the mechanical engineers; and even now that chemistry has established its proper influence upon those industries, and has produced great revolutions in them, that state of things has remained very much as before. Many other branches of manufacturing, which undoubtedly have a chemical basis, and in which to-day a large number of chemists are actually employed, were in those days carried on in a purely empirical manner, like any handicraft. I instance soap-making, tanning, brewing—indeed, all those industries which are connected with food—and above all, dyeing and tissue-printing. But towards the end of the period which we have so far had in view, we perceive the commencement of a scientific treatment of those industries. Even before then, the genius of Chevreul had thrown a flood of light on the chemical behaviour of fatty substances, and Persoz followed in the domain of dyeing fabrics. On the other hand, the more intelligent manufacturer of chemicals

gradually ceased to entrust the construction of apparatus to ordinary tradesmen, and began to seek the assistance of trained engineers, so as to obtain apparatus constructed in the most rational way, and so to effect a saving in space, time, fuel and labour. This co-operation of the various arts and sciences was distinctly promoted by the technical high schools in France and Germany, more especially the Ecole Centrale at Paris, founded in 1830, and the Polytechnics at Karlsruhe, Vienna, Hanover, and Zürich, which sprang into being during the ensuing twenty-five years.

In Great Britain matters took a somewhat different course. Here the chemical industries had from the first taken their full share in the astounding development of all branches of industry which in this country has for several centuries enjoyed an uninterrupted peace, whilst continental Europe was lacerated by frequent wars, above all by the storms raging during a quarter of a century in the wake of the French Revolulion of 1789. Napoleon's attempt to create industrial progress by special legislation, and by hermetically shutting off the Continent from intercourse with England, was not crowned with success; and the industries artificially nursed by his policy mostly collapsed when, on the conclusion of peace, English merchandise was once more freely imported into France, Germany, and other countries. Thus Great Britain had a long lead in all the fields of commerce and industry.

Some of the most important of the chemical industries have indeed altogether originated in this country, especially that of sulphuric acid and that of chloride of lime, both of which date back as far as the eighteenth century. But it is only fair to remember that some of the most important improvements in these manufactures are due to French inventors and French scientists. Not only that, but we must bear in mind that to France we owe the invention of the Leblanc process, which for three-quarters of a century has enjoyed a practical monopoly in the manufacture of alkali. It is curious to notice how, in this case, England has reciprocated the services rendered to her by France by the development of the other chemical industries just named. The Leblanc process, invented in 1791, and carried out on a large scale in France a few years later, could not be at once introduced into this country, owing to the fact that its first material, common salt, was burdened with an absolutely prohibitive excise duty. The abolition of this tax in 1823 acted like the wave of a magic wand, not merely in calling into life the manufacture of alkali itself, but by giving a strong impetus to all the chemical industries connected therewith, viz., those of sulphuric, hydrochloric and nitric acid. Almost immediately the tide of inventions and improvements set in, and a few decades later we find Great Britain absolutely dominant, not merely in the branches just mentioned, but generally in the field of inorganic chemical industries. For many years, up to 1870 about, this predominance was not seriously called into question.

This splendid achievement Great Britain owes, in the first place, to the energy, business capacity and general practical ability of her sons; and, in the second place, to her very much greater freedom from the leading-strings of government, in comparison with Continental states; but, thirdly, also to the special talent possessed by many Britons in the mechanical direction, to their aptitude for seizing the essence of any machinery, and to their genius for inventing new mechanical contrivances and adapting them for purposes of manufacturing and locomotion. The nation to which mankind owes the inventions of the steam-engine, of railways and steam-boats, and the displacement of manual labour by machinery in spinning and weaving, and countless other industries, such as the substitution of the Bessemer process for hand-made wrought-iron, this same nation has, up to the third quarter of last century, also manifested the greatest progress in applied chemistry. The British manufacturer, although in those times he had frequently not studied the science of chemistry, as such, has nevertheless always shown special aptitude for creating, so to say, intuitively, the most suitable apparatus for operating chemical processes, the principle of which may have been discovered elsewhere.

In this manner inorganic chemical industry was developed in Great Britain up to the middle of last century to a greater extent than in any other country, by men like the Muspratts, Tennant, Gossage, Dunlop, Chance, and many others. Most of them were neither studied chemists nor engineers, but in their school any theoretically educated chemist could immensely profit for the work of factory-manager.

In close connection with this state of matters we find in England among the greatest inventors men who, at the outset, did not even possess a routine knowledge of the field in which they achieved their later successes, and who were altogether "outside the profession." Walter Weldon, the reformer of the industry of chlorine, was a journalist of high literary culture, but originally of a very slight amateur knowledge of chemistry, and not at all acquainted with practical manufacturing. Henry Bessemer was a brass-founder who, during the earlier part of his life, had nothing to do with iron. Sidney Gilchrist Thomas was a clerk in the War Department, who had never seen an ironworks when he made his epoch-making invention, and who had acquired his knowledge of chemistry and metallurgy in his spare time after office hours, which most of his colleagues in England spend in the pursuit of sport, and many of his Continental colleagues in the beer-house or the wine-tavern.

Peculiar to England is also the following case, very different from those just quoted, but also illustrative of the ways of British inventors. William Henry Perkin, whose jubilee was celebrated last year amid the concourse of all civilised nations, had, at the early age of sixteen, entered Hofmann's laboratory in London. Already, two years afterwards, whilst working at the synthesis of quinine (a task

not accomplished up to date), he discovered the colouring matter called "mauve," the forerunner of all colours produced from coal-tar: and only a year later, at an age when, on the Continent, the great majority of young men are either still at school, or at least entirely innocent of any taste for practical life, and when they only enter upon their theoretical studies, he built a factory for producing his mauve, which at once proved a success and laid the foundation for his splendid work in after life. This is not the place to enlarge upon his later career, which is almost unique in combining inventive genius with the true spirit of pure science.

We need not be surprised to find that sometimes even the most eminent practical men threw away much time and capital on inventions which, in the end, turned out failures. Sometimes this was due to the fact that, owing to their ignorance of the scientific principles of the case, they hit upon a wrong idea and pertinaciously clung to it, unmoved by constant mishaps which they hoped to overcome by patience and perseverance. But not unfrequently failures occurred, even where the original idea was a good one, which, in the end, was carried to a successful issue. The cause of this may be that the inventor had overlooked some difficulty, apparently unessential, but in effect fatal to success. Or else the proper mechanical means for carrying out the idea were not discovered by the inventor nor the experts consulted by him. I beg leave to illustrate this by a remarkable instance. One of the great problems presented to applied chemistry in the last century, at which many inventors in all industrial countries have been working, was the utilisation of "alkali-waste," that is the residue resulting from the extraction of crude soda by the Leblanc alkali process, which occupies a large space, and which for generations caused an unbearable nuisance for miles around the works by contaminating air and water. The first partial success in this direction was scored in 1861 by Ludwig Mond at Cassel—later on at Widnes, and by Max Schaffner at Aussig. But the endeavours to solve that problem are much older. One of the first patents referring to it was taken out in 1837 by Gossage, one of those great captains of English chemical industry whose name I have already cited. Already, at that date, he had conceived the idea of decomposing and utilising the calcium sulphide, which is the principal constituent of alkali-waste, and which, at the same time, is the cause of the nuisance produced by that waste. The idea was to treat the waste with moist carbonic acid, which interacts with the calcium sulphide to form calcium carbonate and hydrogen sulphide. Gossage quite rightly recognised a number of the conditions necessary for realising that reaction, but unfortunately not all of them. To mention only one thing in which he failed: neither Gossage himself, nor anybody else for many years afterwards, hit upon a practicable method of dealing with the dilute hydrogen sulphide formed. After working incessantly at this problem for seventeen years, Gossage

believed success within his grasp. Most unfortunately, he was not merely mistaken, but committed a serious financial miscalculation as well. In his over-confidence he launched out into a big speculation by contracting with all alkali manufacturers of Widnes (then the greatest centre of that industry in the whole world) for the treatment of their waste during a number of years. But it soon became manifest that there were unforeseen difficulties not yet overcome. The expense of the treatment, moreover, exceeded the value of the products. Still, Gossage's good faith was recognised on all sides, and all the alkali makers but one released him from his ruinous contract. That single exception, however, proved sufficient to cripple the inventor financially, though his name was even then one of the most honoured in his profession, not merely in England, but also abroad, and though it was common knowledge that he had spent the best years of his life on that thankless task. As late as 1861 he firmly proclaimed his conviction that he was right after all, in a discourse delivered before the British Association. Well, he *was* right, but the missing links in the process were only discovered in 1883 and 1887, and led to the application of that process at all the Leblanc works. This final success is connected with the names of Carl Friedrich Claus and of Alexander Chance.

Just about the time of which I have been speaking, I tried, after absolving my University studies in my native country, Germany, to make my way there in the chemical industry which, at that time, was certainly in a very backward state. Accordingly, in 1864, I resolved to try my luck in this country, which occupied the first place in this respect as well as in many others. One of my first steps, after arriving in England, was to call at the Royal Institution with a letter from my teacher, the immortal Robert Bunsen, introducing me to the Professor of Chemistry, Edward Frankland, who received me with the utmost kindness, and whose powerful word greatly smoothed my progress. In coming to England for my practical education, I followed the example of many German chemists, of whom I will only quote a few of the most eminent names—Caro, Pauli, Martius, Peter Griess and Ludwig Mond. The two last-named have permanently associated themselves with this country; whilst the three first-named, as well as many other German chemists who had found a temporary home in England, returned later on to their own country; and these very men have been in the forefront of those to whom is due the remarkable development of German chemical industry, which took place almost at the same time as its political rise, a coincidence which is probably not at all accidental. I myself remained twelve years in England, and only left when called to my professorial chair in Switzerland.

The reason for the sudden and intense blossoming out of the chemical industry in Germany, after being long dormant, and for its firm and healthy rooting in a soil long prepared by the general spread

of scientific education, is perfectly clear. Up to that time the German professor, as well as his students, had been frequently held up to ridicule, not merely abroad but at home as well, as idealistic dreamers, unsuited to the wants of real life and to the requirements of trade and manufacture, and in this there was only too much truth, so long as they were not in intimate touch with men of practice. But at last an amalgamation between these two classes of men took place, and was greatly furthered by the World's Exhibitions, chiefly that held in London in 1862, and, moreover, by the very lessons learned by the German students during their stay in England. Without laying aside their scientific armour, they profited by what they had seen of the co-operation of chemistry and engineering, and, generally speaking, of the interaction of science and practical life. Within a very few years there arose those enormous establishments at Ludwigshafen, Höchst, Elberfeld, Berlin, Darmstadt, and elsewhere, which are conducted on a scientific basis, but with the most extensive utilisation of all the attainments of manufacturing experience. Several of these have now a staff of a couple of hundred chemists and of dozens of fully trained engineers. But although at present Germany certainly holds the foremost position in many branches of chemical industry, it would be unfair to award to her the only palm for progress in this branch of human work. Austria, France, Switzerland, and Belgium have all made immense strides in that direction; of America we shall speak anon. And what of Great Britain? Well, she has certainly not stood still, and she is still one of the greatest homes of industrial chemistry, but one cannot blind oneself to the fact that her progress in this line has not been quite so rapid as that of the other countries just named, to say nothing of the United States, whose chemical industry was hardly in existence at the beginning of that new epoch, but which now is second to none but England and Germany. To some extent the comparative slowness of development in this country is not absolute, but merely relative. It is clear that those who are already well ahead cannot, within a given time, cover as much ground as those who start late, since there is so much less leeway to make up; hence the quicker progress made by the late starters does not mean any real superiority, provided always that the former class are not actually outstripped in the race. The question whether this has really come to pass in this or in that field of chemical industry in the struggle for life between the English chemical manufacturers and their Continental and American rivals, is too wide and too delicate a topic to be touched upon now. But so much you will allow me to say—if we confine ourselves to a comparison of English and German chemical industries, I have surely laid sufficient emphasis upon the fact that the Germans have immensely profited from the English in the matter of practical manipulation and in the co-operation between chemists and engineers. Why should not the English, then, turn the tables on the Germans, and profit, in

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their turn, by giving a wider scope to the scientific treatment of industrial problems in their factories than has hitherto been the rule? Seeing that in pure science the people of Great Britain have never lagged behind *any* other nation, and that, on the contrary, the land of Newton and Faraday has been a beacon to all others at more than one epoch, there is absolutely no valid reason why she should now, or at any other time, be behind any other in the combination of science with practice.

Before turning to another chapter, I beg leave to mention a very good instance of the way in which Englishmen have understood how to combine engineering with chemical manufacture. I have already spoken of the introduction into this country of the Leblanc process for the manufacture of one of the most important chemical products, carbonate of soda, or "alkali," in the parlance of trade. This process came over from France; but it had not been long employed in England before it was thoroughly modified in all particulars, and thus rendered more efficient and more remunerative. In no instance have the excellence of English methods, and their superiority over those used at that time on the Continent, been better proved than here. My own treatise on "Sulphuric Acid and Alkali" is, in its technological part, based mainly on what I had learned and practised during the twelve years of my residence in this country; and if that treatise has met with a favourable reception, as having proved useful to both British and non-British alkali makers, it is due to the circumstance just mentioned. But those methods, worked out during the second and third quarter of the last century, have had their day. A new process came up, which sapped the economical foundation of the Leblanc process. The history of this, the ammonia-soda process, has, strange to say, been directly contrary to so many others. It was invented by two Englishmen, Dyar and Hemming, who patented it in 1838, and who established the (very simple) chemical part of it in such manner that nothing really essential has been added since their time. But Dyar and Hemming did not succeed in the *practical* application of their invention, nor did their numerous successors meet with any better fortune, either in this country or elsewhere. It was reserved to a Belgian engineer, Ernest Solvay, to find the first economical solution of that problem, and this he achieved only after many years of patient work, and after sacrificing nearly all his means. Once he had gained his point, however, the economical superiority of the ammonia over the Leblanc process soon became evident. This was brought home to English manufacturers by the success of the firm of Brunner, Mond and Co., which had acquired Solvay's English patent rights. The Leblanc process, and the enormous sums of money invested in it, seemed even then doomed to speedy extinction. But for a time, at least, this calamity was averted by the perseverance with which the British alkali makers kept making improvements in the

Leblanc process, all calculated to cheapen it and lessen the nuisance connected with it. I cannot go into details on this point, but I wish to mention at least two things. One of these I have already spoken of in another connection—I mean the recovery of the sulphur from the alkali waste, where it had been the cause of an intolerable nuisance, and which now became the source of considerable profit. The second point is the substitution of machinery for manual labour in all stages of the process. It is only by means of these, along with a number of other improvements, that it has been made possible for the Leblanc process to survive to a certain extent. How much longer that is likely to continue I will not pause to speculate on. The prolongation of its life is due to the fact that in the first stage of the process an important acid is produced, which is not furnished by the ammonia process, viz. hydrochloric acid. Most of this is immediately converted into chlorine, which gas is used up for preparing very important articles of trade, viz. bleaching powder, bleach liquors, and chlorates. Of these, bleaching powder is a British invention, made by the Glasgow chemist, Tennant; but, apart from this, the manufacture of chlorine and of all chlorine products has been put on its practical basis almost entirely by English inventors, and has been developed more extensively in this country than anywhere else in the world. The processes worked out by Tennant and Dunlop at Glasgow, as well as by a host of others —amongst whom the names of Weldon and Deacon, in Lancashire, stand out prominently—have been copied for generations all the world over. The manufacture of chlorine products, which is not possible in the ammonia-soda process, has naturally given a new lease of life to the Leblanc process, at least in its first stage, which is the manufacture of salt-cake and hydrochloric acid. But, on the other hand, this last entrenchment of the Leblanc process is being vigorously assaulted from another quarter—by the electrolytic processes, which split up the alkaline chlorides directly, and in the simplest possible manner, into free chlorine and caustic alkali.

We are, in these days, so much accustomed to deal with electricity in its innumerable applications, that we are apt to forget how recent is the introduction of that force of Nature into practical chemistry. One of the finest heads among English alkali makers, whose grasp of the principles of science far surpassed that of most ordinary technical chemists, Dr. Ferdinand Hurter, pronounced himself, as late as 1888, decidedly against the commercial possibility of introducing electricity as an agent for manufacturing the cheaper class of chemicals. But within a very few years of that date the contrary had become an established and well-known fact, even in his own domain of alkali. True, in hardly any field have there been more failures to translate the results of science into economical manufacturing processes than in that of electricity; and even now it is only quite exceptionally that, wherever the electrical current has to be produced

by means of *steam*, electro-chemical methods can compete with the older ones for the manufacture of what is called "heavy chemicals." This is easily understood when we remember that about 90 per cent. of the heat-value of coal, or its equivalent of energy, is lost in the circuitous routes of steam boiler, steam engine, and dynamo. But there are several ways in which the problem of obtaining cheaper electricity is being grappled with; and, if most of these have to be dismissed for the present, as belonging to the "music of the future," we have at least one which is a hard fact, and that is the generation of electricity by water-power. Unfortunately, in the British Isles the amount of available water-power is very limited in comparison with many other countries. It is a curious coincidence that those two European countries which are the greatest producers of coal, Great Britain and Germany, should be less favoured by Nature in respect of water-power than other countries which possess little or no stores of mineral fuel, as Sweden, Norway, Switzerland, France, Italy, and Spain. A very different condition of affairs obtains in the United States, where we find the greatest coal-fields combined with the greatest amount of water-power existing in any civilised country. It is impossible to shut one's eyes to the fact that the day will inevitably come when the coal-fields will be so far exhausted that all those industries which consume large amounts of mechanical energy will be forced to emigrate to countries where water-power is abundant.

No other substitute has, as yet, been found for generating force, and, indirectly, electricity. True, the energy given out by the descent of water in rivers is but a small fraction of that which is radiated upon the earth from the sun, or of that which is developed by the play of the tides and the force of the wind, but no way has yet been found of utilising these other sources of energy, except to the slenderest extent. The harnessing of these natural agents belongs, so far as we can see, to the class of problems which will hardly be solved by our own generation, whatever developments the remoter future may bring. But of the water power existing on this planet there is a large proportion which has never yet been touched, and this, as well as the water power which has been already forced into the bonds of man, runs on for ever. This is, of course, an incalculable advantage over coal, which, by its use as fuel, is dissipated into the atmosphere in the shape of carbon dioxide, and thus altogether destroyed as a source of energy, since from carbon dioxide fresh fuel can only be generated by the intervention of solar energy, and this takes place at such a very slow rate that it cannot be taken into account in our economical consideration.

We, who have been born to see the ascendency of coal as the principal producer of energy in bulk, can hardly realise what a short epoch in the past and future history of mankind belongs to the age of coal. It has taken many thousands of years to form the beds of coal

which exist in the earth's crust, and which have preserved to us a tiny portion of the solar energy radiated upon our planet during that period, millions of years ago. At that period, for various reasons, the production of living matter must have been incomparably more rapid than is the case at present. During untold ages this stored-up energy was lying idle, hidden under the accumulations of the more recent geological formations, not merely up to the advent of man, but through nearly the whole of his history. Leaving aside the tens of thousands or (according to some) hundreds of thousands of years during which man existed before the dawn of history, we must remember that historical documents exist in Egypt, Babylon, India, and elsewhere, taking us back at least 8000 years, and that the most glorious times of Greek and Roman civilisation are about 2000 years behind us. How modern, in view of these figures, is the use of the coal, and over what a short time it will extend ! In these isles the use of coal is much older than in any other country, but even here its serious exploitation is comparatively recent, dating barely 150 years back ; whilst its future (even if we disregard the more pessimistic estimates) is not likely to exceed some 200, or at most, 300 years. Germany and the United States will probably hold out 200 or 300 years longer, but in all other countries the chances are all the other way.

Well, what is to happen then ? Those countries where water power is abundant may possibly substitute electrical heating for that produced by the burning of coal, but what about England and Germany, which are so poorly off in that respect ? Even in those countries which are more favoured, the amount of water power is by no means infinite ; and, if it had to be drawn upon, not merely for motive purposes, but for the production of electricity for heating purposes, it would be found insufficient in most places. Here we are faced by one of the greatest problems of applied science, both in chemistry and in physics, a problem which will give plenty of occupation to generations of future inventors. At present we can only surmise that some solution will present itself in the shape of a direct conversion of the sun's rays into other forms of energy ; but the means by which this would be practically accomplished are at present quite uncertain.

The Age of Coal, in the midst of which we are living, short as it is evidently doomed to be in the long history of mankind, has been of incalculable service. For our purposes we may dismiss the earlier part of it, and look back only a hundred years. In all branches of industry, in locomotion, in the means of communication, and in innumerable matters ministering to the comforts of life, the progress since that time has been going on at a geometrical ratio. The present state of all these factors of civilisation in Europe (to say nothing of America) differs from that obtaining a hundred years ago far more than the latter differed from the Roman era, or even from the age of the Egyptian kings. And this miracle has been brought

about solely by *coal*, without the aid of which it is simply impossible to imagine the revolution which has taken place since then. "Railways!" That single word, to give only one instance, will bring this home to anyone who ponders over this matter. And it is equally impossible for us to imagine that, during the past century, there could have been any other invention, based upon the utilisation of the other supplies of energy of which we have spoken, which could have replaced the untold services of coal, that accumulator of solar energy, which alone has enabled the human mind to work out the thousand and one channels through which modern civilised life is flowing. We may say this with all confidence, for how otherwise could we account for the fact that such inventions have not been made in former times, when there were certainly quite as many ingenious minds in the world as during the coal-consuming age?

Let us now come down to considerations of a more modest, but more practical nature than those in which we have just been indulging. Seeing that the stock of mineral fuel upon this earth is so very limited, cannot we find means of husbanding it more than this has been done hitherto? It is only too notorious that the way in which coal is at present consumed, is most wasteful. Of the energy residing in coal, most ordinary steam-engines utilise less than 10 per cent. by converting it into mechanical motion; and even the most perfect steam-engines devised utilise hardly more than 15 per cent. Improvements in this direction may possibly swell this proportion a little, but there is no prospect of gaining much in that direction. Enormous wastages are also incurred in other ways. The conversion of pig-iron into steel, the manufacture of glass, and many other industries consumes from four to twenty times, and even more, of the quantity of coal required by theory. Many descriptions of coal are too poor to be used at all except in the immediate vicinity of the spot where they occur; and in burning our fuel, whether it be for industrial or for technical purposes, we invariably send its nitrogen into the atmosphere, which surely contains quite enough of that commodity; the only exception being the manufacture of coal-gas, to which we shall refer later on. Here some of the grandest problems of applied chemistry present themselves to us—how to stop that fearful waste of fuel; and how to recover the nitrogen of the coal, if that be possible.

It is certain that we must look for the solution of these questions in the direction of converting coal into gaseous fuel. It is true that much has been done in that field in past years, and more especially will the name "Siemens" occur to every one in this connexion, but much more remains to be accomplished. Another great stride ahead lies in the better utilisation of the waste gases from blast furnaces, in which respect the last few years have witnessed some very important improvements. All this refers merely to a better utilisation of the heating power of coal, but not to that other great task, the recovery

of its nitrogen in a useful shape. This, together with the question how coal of poor quality is to be turned to a better account, has been tackled by the equally indefatigable and intelligently directed energy of Dr. Ludwig Mond, one of the benefactors of the Royal Institution. His invention, the "power-gas," has already attained a large measure of success, as is proved by the extent of the plants erected and designed. Mond's process belongs to that class by which we approach one of the greatest problems, for the time being, of applied chemistry; I mean the conversion of nitrogen from sources not yet opened out into ammonia and nitrates.

The immense importance of this latter problem lies in the fact that it touches our most urgent want, our supply of food. The soil of most countries, if tilled in the old manner, would not nearly suffice for the production of the requisite amount of food for men and cattle, while the limits of its producing capacity are being gradually narrowed down by exhaustion. Sir William Crookes, in his address to the British Association in 1898, has most forcibly drawn attention to this. The importation of food-stuffs from other less thickly populated countries can only modify, but not altogether extinguish, the danger of ultimate shortness of food at some future date, possibly not so very remote. It is certainly a great comfort to know that, with suitable manuring, the soil may be forced to yield even better crops than it would give in the virgin state, let alone in a condition impoverished by centuries of tilling. But stable manure is nothing like sufficient to attain that object, and we must turn to mineral fertilisers, principally phosphates, potassium salts, and nitrogen compounds. The two former classes of fertilisers are found in abundance in nature, and there is no danger, apparently, of their being exhausted during the next thousand years.

But the case is very different with the mineral forms of nitrogenous manures, i.e. ammonium salts and nitrates. For agricultural purposes it does not make much difference whether we apply the nitrogen in one or the other of these forms. The ammonia, apart from insignificant quantities otherwise obtained, all comes from the nitrogen of the coal, but up to about twenty years ago only that coal which was used in the manufacture of gas was made to yield ammonia, and only one-sixth of its nitrogen was obtained in this form. In all other uses of coal, where at least twenty times as much is consumed as in the manufacture of gas, the nitrogen was simply sent into the air.

Quite recently, some progress has been made in the way of utilising some of this nitrogen as well. I have already mentioned the Mond process, where some of the nitrogen is recovered in the shape of ammonia; but this covers only one corner of the field. In another section a good deal has been already achieved. In the manufacture of coke, which is also a process of destructive distillation, and entirely analogous to gas making, very much larger quantities

of coal are consumed than for the latter, since coke is indispensable for the smelting of iron and for other metallurgical purposes. Up to about twenty years ago all the volatile by-products in the manufacture of coke were lost—that is to say, tar, gas, and ammonia. The recovery of these by-products was first carried through in one or two French coke-works, about 1861, but nowhere else for a number of years, although in 1879 the late Dr. R. Angus Smith had earnestly recommended to the English coke-works the adoption of that system. Even now, both in France and England as well as in America, the recovery coke-ovens have found only a very limited adoption; in England perhaps 5 per cent. of the coke is made in this way, against upwards of 50 per cent. in Germany. In consequence of this, whilst twenty years ago Germany imported nearly all ammonium sulphate required for its agriculture from this country, she now imports none, and has, on the contrary, become a large exporter of that commodity. The reasons for this wonderful change are various. One of them is undoubtedly the revival of that spirit of push and enterprise which, after lying dormant for centuries in consequence of the ravages of the Thirty Years' War, caught up the German people, and enlivened German industry in all directions. Without going into details on this matter, we may take it that a considerable reserve of ammoniacal nitrogen exists in the quarter indicated, and that the present production of about half a million tons of ammonium sulphate might be greatly increased in that manner.

But that reserve is, after all, nothing like sufficient to cover the requirements of agriculture in the future; and it is quite likely that in the long run all the really available nitrogen of the coal would not suffice for the wants of man. And what about the time when coal itself will be exhausted? Well, there is an eternal and inexhaustible source of nitrogen to which we must turn, and that is the atmospheric air. Four-fifths of this consists of nitrogen, calculated to amount to 4000 billions of tons, mixed with a quarter of that weight of oxygen. More than 100 years ago, in 1785, Cavendish discovered the fundamental fact that, by the action of the electric arc, the nitrogen of the air combines with oxygen to form nitric acid. The formation of ammonia from atmospheric nitrogen has also been effected, both by electricity and (which is more important) in other ways as well, as we shall see anon. But until a very few years ago these facts had never been put to any practical use, and the problem of turning the atmospheric nitrogen into ammonia, or nitric acid, although frequently approached in a purely scientific or, experimentally, in a technical way, had not been solved. Our days have seen the realisation of that most important task.

Let us first speak of ammonia. We are led up to this by what is, verily, a long and circuitous path. We must start from the discovery of calcium carbide (announced in 1862 by the celebrated Woehler), the technical preparation of which substance was first effected by

Willson in 1892, and about the same time by Moissan. True, the expectations that were entertained in various quarters in connection with this remarkable chemical product have not been fully realised to the extent anticipated by the inventors; but, on the other hand, an entirely novel use has been discovered for it by Professor Adolf Frank and Dr. Caro, of Berlin. They found that when nitrogen is passed over red-hot calcium carbide it is absorbed with formation of calcium cyanamide. This latter, when treated with water under high pressure, is made to yield ammonia; but it is not necessary to do this, since the crude product, which they have called "lime-nitrogen," can serve directly as nitrogenous fertiliser, and is in that respect equivalent to its own weight of ammonium sulphate. This is, indeed, its principal use for the present and the near future; but, as a matter of fact, the discoverers go much further. From the lime-nitrogen they prepare cyanogen derivatives of various kinds, some of which are valuable as constituents of explosives, and they are earnestly trying to employ it in the manufacture of nitric acid. They have also brought in several other industries—the manufacture of pure graphite, of pure hydrogen, of urea, and so forth. The pure nitrogen required for all this was at first produced by passing atmospheric air over red-hot copper; but it is now made by liquefying air and distilling off the oxygen, which is thus obtained as a valuable by-product. The inventors expressly recognise the invaluable aid which they have in this respect derived from the world-renowned researches of Sir James Dewar, carried out in the Royal Institution. The works already in operation, or in course of construction, will by the end of this year utilise water-power to the extent of some 55,000 horse-power, and will produce lime-nitrogen equivalent to 100,000 tons of nitrate of soda, and this with an expenditure of force less than one-third of that required for the process of Birkeland and Eyde, of which I shall speak directly.

I must, however, first say a word about the strenuous efforts made by Professor Frank and Dr. Caro, this time in connection with Dr. Ludwig Mond, to extract from *peat* both power and ammonia. Enormous, but hitherto almost worthless, deposits of peat exist in Ireland and North Germany; and the ultimate success of these endeavours, which we have every reason to hope for, will prove an incalculable boon to these countries. At the same time, all fears of a scarcity of ammonia for agricultural purposes would be thus removed for generations to come.

Important as ammonia is as a fertiliser, it ranks after the nitrates in that respect; and, unlike ammonia, the nitrogen of the nitrates is of immense importance for other purposes as well, viz. the manufacture of nitric acid and of explosives. The very limited quantities of nitrates required in former times, amounting to a few tens of thousands of tons per annum, were furnished by Indian saltpetre, that is, crude potassium nitrate. A far more abundant supply was opened

out a little more than half a century ago, when the exploitation of the beds of nitrate of soda in South America was begun. The crude nitrate found there is refined on the spot, and comes to us as "Chilian saltpetre," which is almost pure sodium nitrate, to the tune of a million and a half tons per annum. About four-fifths of this is taken up by agriculture, the remainder serving, in the first place, for the preparation of nitric acid. As for that acid, it is impossible to imagine how we could do without it. Apart from minor, but quite indispensable uses, one of which is in the manufacture of sulphuric acid by the lead-chamber process, the greater part of nitric acid is consumed in the manufacture of coal-tar colours and in that of explosives.

Let us pause for a minute to consider the last-named. Even supposing it possible that all wars could be abolished on this terrestrial globe—a contingency not very likely to arise within the next few years, in spite of the laudable efforts of the Peace Societies—and that gunpowder were no longer required for shooting wild animals (an equally unlikely case, which would lead to a quite intolerable increase of game, big and otherwise)—we cannot conceive the possibility of our present system of civilisation enduring without a colossal consumption of explosives. How could we carry on mining operations without them? How could we get stones from the quarries? How could we construct roads, and tunnels, and railways without the help of explosives, all of which have a basis of salts or esters of nitric acid? And these have, up to the present, been prepared almost exclusively from Chilian saltpetre. The idea has certainly been mooted to imitate the natural process by which the nitrate is formed in India. This has been tried during a number of years in France and in Sweden, but has been given up as unprofitable in our northern climes. Also, the interesting experiment of sowing the bacillus of nitrification and of cultivating it in the soil has proved a failure, although I would fain believe that the last word has not been spoken on that subject. This, if successful, would replace some of the nitrate now used as fertiliser, just as a better utilisation of sewage would act in the same direction; but all this at the best goes only a very small way, and does not furnish the pure saltpetre required for the manufacture of nitric acid. What, then, shall we do when the nitre beds of Chili are exhausted? an event which, according to most estimates, is bound to take place within thirty or forty years from now. Unfortunately, there is no tangible hope of similar beds being found in any other localities, certainly not to any great extent. The beds of Atacama and Tarapacá on the Cordillera owe their origin to an altogether exceptional combination of climatic conditions and geological changes, the repetition of which in other quarters is exceedingly unlikely. Until very few years ago there was no prospect of any fresh supplies of nitrates in any other direction; but we may say that the solution of *this* problem, if not altogether settled in its final shape, has

now been found. After many unsuccessful attempts at realising for practical purposes the discovery of Cavendish, and after a thorough investigation of its scientific principles by Lord Rayleigh, Muthmann and Hofer, Nernst, Haber, and others, this has been achieved, and once more, by means of that well-nigh omnipotent agent, electricity, which thus renders yet another service to mankind. At Notodden, in the Norwegian Hitterdal, a factory has been established to carry out the process of Birkeland and Eyde, who, by an ingenious application of the extreme heat produced by the electric current, make the nitrogen and oxygen of air combine to nitric oxide, which at a lower temperature is spontaneously oxidised into nitrous vapours, with the ultimate production of nitrites or nitrates. This time there is really no doubt that a practicable and economical process has been discovered for which it is intended to employ, by the end of this year, water power to the extent of 30,000 H.P. The Notodden process bids fair to be followed by other even more efficient processes. The most important of these is that of the Badische Anilin- and Soda-Fabrik, for which an experimental factory is in course of construction, and for which 50,000 H.P. are to be employed. But for some time to come the Chilian saltpetre will still rule the trade ; a very large amount of water power will, indeed, have to be brought into use merely to cover the annual increment of consumption of this commodity for agricultural purposes.

One task it is certain that explosives will never fulfil, and that was suggested to me by one of the cleverest mechanical engineers I have known. He was intensely interested in the problem of aërial navigation, and for this purpose he wished to construct an engine worked by fuel of the most concentrated kind. Neither coal, nor benzine, nor oil would do. In his plight he came to me and asked what explosives I should advise him to try for working his engine, in the erroneous idea that explosives were a kind of concentrated fuel. Of course, I could not but reply as follows : All honour to his courage, but no explosive known, so far ever or likely to be invented, could possess that property he required, viz. a large store of energy. A pound of coal represents five times as much energy as a pound of the strongest explosive known—blasting gelatine. My friend had overlooked the fact that a pound of dynamite, though it gives out nearly 150 million horse-power, does so only for the space of $\frac{1}{50,000}$ of a second. He had omitted to take into account the element of *time*, and had confused *power* in the ordinary sense with *energy*, which is the capacity for doing work.

A similar confusion is sometimes made between energy and the creation of high temperatures. This can be very well illustrated by the use recently made of finely powdered aluminium, both as a component of explosives and as an agent for producing very high temperatures, in the shape of Dr. Goldschmidt's "thermite." In both cases the fact is utilised that aluminium is easily, and in the shape of fine

powder, almost instantaneously, converted into its oxide, alumina, by substances capable of giving off oxygen. In the case of thermite, a mixture of finely powdered aluminium and ferric oxide is, when lighted, decomposed instantaneously into molten iron and aluminium oxide. The heat produced thereby far exceeds that produced by coal in any conceivable way ; it is equal to that of the electric arc. One of the most important applications of this agent occurs in the welding of the ends of railway rails, when already laid down, into one continuous rail of any length required. And yet the total energy of thermite is only 450 thermal units per kilogram, or in other words, about one-twentieth of that of the best coal. But, whereas it takes a good deal of time to burn a pound of coal, during which process there is a great loss of heat by radiation, and the heat is spread over a current of gases which we call the flame, a pound of thermite burns off in about one second, and, as there are no gaseous products formed, all the heat generated remains within the molten iron and the alumina, which accounts for the extreme degree of heat to which these are brought.

Electricity has often been invoked to produce the most important of all inorganic products, iron. If this problem could ever be solved in an economical way, it would bring about a perfect revolution in the position of the leading nations. On the one hand, the enormous quantity of coal now consumed in the production of iron and steel (which is probably at least a quarter of the entire output of coal) would be set free for other uses, and the exhaustion of the coal-fields would be put off to a corresponding extent. On the other hand, the production of iron would pass over into the hands of those nations which command the largest amount of water-power, and which, therefore, can produce electricity most cheaply. Of the three countries which now produce between them the bulk, that is seven-eighths, of the world's iron, Great Britain and Germany would go to the wall, and the United States, which already produce more iron than these two countries put together, would become omnipotent in that field. Sweden, Italy, and some other countries would, at any rate, greatly increase their present production. But this radical change is, as yet, far off. No proof has, so far, been given that pig-iron, or the ordinary descriptions of wrought-iron and steel, can be generally produced by electricity at anything like the price at which it is now done by coal in the great industrial centres. Where a certain success has been scored in the electrical metallurgy of iron, it is for the refining of ordinary iron into a superior grade of steel which fetches an extraordinarily high price, and in the production of certain alloys of iron with chromium, nickel, and the like, whereby so-called special steels are obtained. But if at the present moment we do not see our blast furnaces and Bessemer works threatened by the competition of electrical iron, who can tell how soon this may not be the case ?

The limits of my time have been too nearly reached for me to

discourse upon many other problems which present themselves in inorganic applied chemistry, and only a few minutes are left to speak of those belonging to the domain of organic chemistry.

I will point to only two problems of this kind. One of these is the substitution of artificial for natural colouring matters. This, indeed, has now been carried out almost to the bitter end. Long ago, one of the oldest and most widely-used colouring matters, that contained in madder, succumbed to the attacks of the chemists, among whom the names of Edward Schunck and William Henry Perkin testify to the glorious share taken by Englishmen in that victory. The colouring substance of madder—alizarine—is now made from English coal-tar, and has altogether taken the place of the impure form in which it occurs in the madder plant. The growers of this plant in the south of France and elsewhere have had to abandon its culture altogether, to their great sorrow.

A similar fate has already partly overtaken, and may, in the end, destroy entirely, the culture of indigo, most of which, as you know, comes from British India, and formerly represented a value of some four million pounds sterling per annum. At first, after the great Münich chemist, Adolf Baeyer, had prepared the colouring matter of indigo by synthesis in his laboratory, the planters merely shrugged their shoulders, and that with good reason, since Baeyer's processes could not compete with their produce in respect of cost price. Another circumstance which at that time militated against artificial indigo was this, that it started from toluene, the total available quantity of which substance would not have sufficed for producing anything like all the indigo required, even if no toluene were used for other purposes, which is out of the question. But this state of matters has changed. Twelve years ago the late Carl Heumann, assistant professor in my laboratory at Zürich, discovered the synthesis of indigotine from naphthalene. This, like toluene, we get from coal-tar, but in about ten times the quantity, so that there is no fear of any scarcity of naphthalene even in the future. The late Dr. Rudolph Knietsch at the Badische Anilin und Sodafabrik at Ludwigshafen gradually transformed Heumann's laboratory process into a factory process, which is working with entire success on a large scale. Synthetic indigotine is now manufactured at such a low price that its competition has proved a severe blow to the indigo-planting interests. Thus the triumph of scientific investigation and practical skill in chemical manufacturing, gratifying though it be as a splendid achievement of applied chemistry, is a sad trial to many thousands of Indian ryots and their British masters; and this is merely the foretaste of what will inevitably happen in many other cases. What is food for one is poison for another. But yesterday this was the bitter experience of the French madder-grower ; to-day it is the turn of the Indian indigo-planter ; and to-morrow it may be some one else's lot.

Most other vegetable colouring-matters, several of which have also been synthetically produced, have become useless by the discovery of hundreds, and even thousands, of artificial colouring matters far exceeding them in beauty, and often also in fastness. On this well-known point I cannot dwell now.

In conclusion, I would touch upon what is, perhaps, the very greatest problem of applied chemistry, and that is the direct production of feeding-stuffs for man and beast. The synthesis of alimentary substances from inorganic matter has, up to this moment, not been even remotely achieved, nor can we at present so much as guess the direction in which this might be done; whilst, as for the production of food from sawdust and other waste organic substances, we are in no better case. But even here the word "impossible" should not be pronounced. In a more modest form, at all events, chemistry has found magnificent scope in that quarter—I mean in the extraction of alimentary substances from new sources and in the increase of production from old ones. The colossal industry of beet-root sugar is an instance of the former, whilst agricultural chemistry, as a whole, works in the latter direction.

But this is really too vast a subject to be discussed at the fag-end of this lecture, and I must, therefore, content myself with the foregoing few remarks, and beg to take leave of my esteemed audience.

WEEKLY EVENING MEETING,

Friday, March 22, 1907.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

PROFESSOR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Professor of Natural Philosophy *R.I.*

Rays of Positive Electricity.

IN 1886 Goldstein discovered that when the cathode in a discharge-tube is perforated, rays pass through the openings and produce luminosity in the gas behind the cathode ; the colour of the light depends on the gas with which the tube is filled, and coincides with the colour of the velvety glow which occurs immediately in front of the cathode. The appearance of these rays is indicated in Fig. 1, the anode being to the left of the cathode KK. Since the rays appeared through narrow channels in the cathode, Goldstein called them " Kanalstrahlen " ; now that we know more about their nature, " positive rays " would, I think, be a more appropriate name. Goldstein showed that a magnetic force which would deflect cathode rays to a very considerable extent was quite without effect on the " Kanalstrahlen." By using intense magnetic fields, W. Wien showed that these rays could be deflected, and that the deflection was in the opposite direction to that of the cathode rays, indicating that these rays carry a positive charge of electricity. This was confirmed by measuring the electrical charge received by a vessel into which the rays passed through a small hole, and also by observing the direction in which they are deflected by an electric force. By measuring the deflections under magnetic and electric forces, Wien found by the usual methods the value of $\frac{e}{m}$ and the velocity of the rays. He found for the maximum value of $\frac{e}{m}$ the value of 10^4, which is the same as that for an atom of hydrogen in the electrolysis of solutions. A valuable

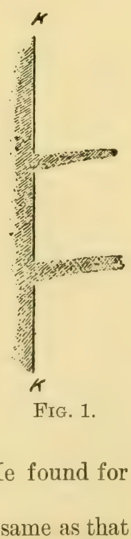

FIG. 1.

summary of the properties of these rays is contained in a paper by Ewers.*

As these rays seem the most promising subjects for investigating the nature of positive electricity, I have made a series of determinations of the values of $\frac{e}{m}$ for positive rays under different conditions. The results of these I will now proceed to describe.

Apparatus.

Screen used to Detect the Rays.—The rays were detected and their position determined by the phosphorescence they produced on a screen at the end of the discharge-tube. A considerable number of substances were examined to find the one which would fluoresce most brightly under the action of the rays. As the result of these trials willemite was selected. This was ground to a very fine powder and dusted uniformly over a flat plate of glass. Considerable trouble was found in obtaining a suitable substance to make the powder adhere to the glass. All gums, etc., when bombarded by the rays are liable to give off gas; this renders them useless for work in vacuum-tubes. The method finally adopted was to smear a thin layer of "water-glass" (sodium-silicate) over the glass plate, and then dust the powdered willemite over this layer and allow the water-glass to dry slowly before fastening the plate to the end of the tube.

The form of tube adopted is shown in Fig. 2. A hole is bored through the cathode, and this hole leads to a very fine tube F. The bore of this tube is made as fine as possible, so as to get a small well-defined fluorescent patch on the screen. These tubes were either carefully-made glass tubes, or else the hollow thin needles used for hypodermic injections, which I find answer excellently for this purpose. After getting through the needle, the positive rays on their way down the tube pass between two parallel aluminium plates A A. These plates are vertical, so that when they are maintained at different potentials the rays are subject to a horizontal electric force, which produces a horizontal deflection of the patch of light on the screen. The part of the tube containing the parallel aluminium plates is narrowed as much as possible, and passes between the poles P P of a powerful electromagnet of the Du Bois type. The poles of this magnet are as close together as the glass tube will permit, and are arranged so that the lines of magnetic force are horizontal and at right angles to the path of the rays. The magnetic force produces a vertical deflection of the patch of phosphorescence on the screen. To bend the positive rays it is necessary to use strong magnetic fields, and if any of the lines of force were to stray into the discharge-tube

* Jahrbuch der Radioaktivität, iii. p. 291 (1906).

in front of the cathode they would distort the discharge in that part of the tube. This distortion might affect the position of the phosphorescent patch on the screen, so that unless we shield the discharge-tube we cannot be sure that the displacement of the phosphorescence is entirely due to the electric and magnetic fields acting on the positive rays after they have emerged from behind the cathode.

To screen off the magnetic field the tube was placed in a soft iron vessel W with a hole knocked in the bottom, through which the part of the tube behind the cathode was pushed. Behind the vessel a thick plate of soft iron with a hole bored through it was placed, and behind this again as many thin plates of soft iron, such as are used

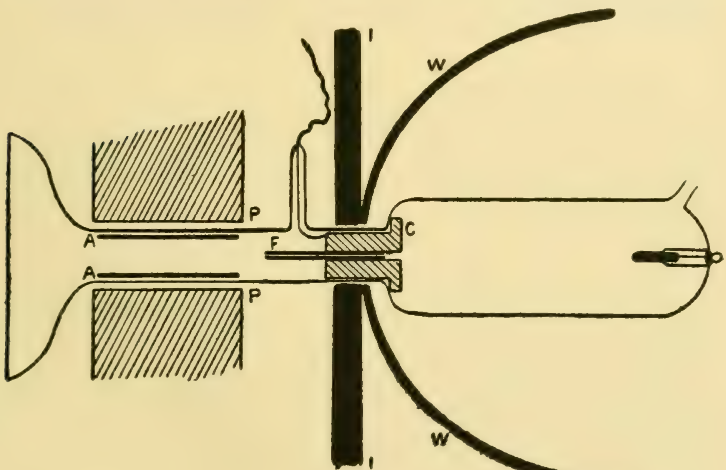

Fig. 2.

for transformers, as there was room for, were packed. When this was done it was found that the magnet produced no perceptible effect on the discharge in front of the cathode.

The object of the experiments was to determine the value of $\frac{e}{m}$ by observing the deflection produced by magnetic and electric fields. When the rays were undeflected they produced a bright spot on the screen; when the rays passed through electric and magnetic fields, the spot was not simply deflected to another place, but was drawn out into bands or patches, sometimes covering a considerable area. To determine the velocity of the rays, and the value of $\frac{e}{m}$, it was necessary to have a record of the shape of these patches. This might

have been done by substituting a photographic plate for the willemite screen. This, however, was not the method adopted, as, in addition to other inconveniences, it involves opening the tube and re-pumping for each observation, a procedure which would have involved a great expenditure of time. The method actually adopted was as follows: The tube was placed in a dark room from which all light was carefully excluded, the tube itself being painted over, so that no light escaped from it. Under these circumstances the phosphorescence on the screen appeared bright and its boundaries well defined. The observer traced in Indian ink on the outside of the thin flat screen the outline of the phosphorescence. When this had been satisfactorily accomplished the discharge was stopped, the light admitted into the room, and the pattern on the screen transferred to tracing-paper; the deviations were then measured on these tracings.

Calculation of the Magnetic and Electric Deviation of the Rays.

If we assume the electric field to be uniform between the plates and zero outside them, then we can easily show that x, the horizontal deflection of a ray whose charge is e, mass m, and velocity v, is given by the equation

$$x = \tfrac{1}{2} \mathrm{X} \frac{e}{m v^2} l \, (l + 2d),$$

where X is the force between the plates, l the length of path of the rays between the plates, and d the distance of the screen from the nearer end of the parallel plates.

To find the deflection due to the magnetic field, we have, if ρ is the radius of curvature of the path at a point where the magnetic force is H.

$$\frac{m v^2}{\rho} = \mathrm{H} e v,$$

or

$$\frac{1}{\rho} = \frac{e}{m v} \mathrm{H}.$$

If y is the vertical displacement of the particle, we have

$$\frac{1}{\rho} = \frac{d^2 y}{d z^2} \text{ approximately,}$$

where z is measured along the path of the ray. Hence

$$\frac{d^2 y}{d z^2} = \frac{e}{m v} \mathrm{H};$$

$$y = \frac{e}{m v} \left[\int_0^{l+d} \int_0^z \mathrm{H} \, d z \right] \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In these strong fields there are considerable variations of H along the path, so that to calculate the integrals we should have to map out the value of H along the path of the ray. This would be a very laborious process, and it was rendered unnecessary by the following simple method, which, while not involving anything like the labour of the direct method, gives much more accurate results. The method is shown in Fig. 3. The part of the tube through which the rays pass was cut off, and a metal rod placed so that its tip Z coincided with the aperture of the narrow tube through which the positive rays had emerged. A very fine wire soldered to the end of this tube passed over a light pulley, and carried a weight at the free end. The pulley was supported by a screw, by means of which it could be raised or lowered; a known current passed through the wire, entering it at Z

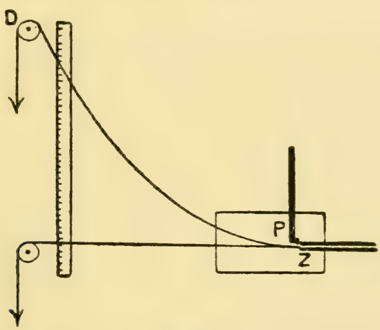

Fig. 3.

and leaving it through the pulley. The pulley was first placed so that the path of the stretched wire when undeflected by a magnetic field coincided with the path of the undeflected rays. A vertical scale, whose edge was at the same distance from the opening through which the rays emerge as the screen on which the phosphorescence had been observed, was placed just behind the wire, and was read by a reading microscope with a micrometer eyepiece. When the magnetic field was put on, the wire was deflected; and if T is the tension of the wire, ρ the radius or curvature into which it is bent, i the current through the wire,

$$\frac{\mathrm{T}}{\rho} = \mathrm{H}\,i;$$

or, if y_1 is the vertical displacement of the wire,

$$\frac{d^2 y_1}{d z^2} = \frac{i}{\mathrm{T}}\cdot \mathrm{H},$$

Now if $\frac{d y_1}{d z} = 0$ when $z = 0$ we have, if y_1 is the displacement of the wire at the scale,

$$y_1 = \frac{i}{\mathrm{T}}\int_0^l . \int_0^z \mathrm{H}\, d z \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

Hence, comparing (1) and (2) we have

$$\frac{y}{y_1} = \frac{\frac{e}{m v}}{\frac{i}{\mathrm{T}}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

a relation from which the magnetic force is eliminated. To ensure that the tangent to the wire is horizontal when $z = 0$, the following method is used. P is a chisel-edge carried by a screw and placed about 1 mm. in front of the fixed end of the wire; this is adjusted so that when the magnetic field is not on, the wire just touches the edge; this can be ascertained by making the contact with the wire complete an electric circuit in which a bell is placed. When the magnetic field is put on the wire is pulled off from the edge, and the tangent at $z = 0$ is no longer horizontal; it can, however, be brought horizontal by raising or lowering the pulley D until the wire is again in contact with P, which can be ascertained again by the ringing of the bell. Then y_1 is the vertical distance between the point where the wire now crosses the edge of the scale and the point where it crossed it before the magnetic field was put on. Since y, y_1, i, and T can easily be measured, equation (3) gives us the value of $\frac{e}{m v}$, while the deflection under the electric force gives the value of $\frac{e}{m v_2}$.

If y is the vertical displacement of the patch of phosphorescent light on the screen produced by the magnetic field, x the horizontal displacement due to the electrostatic field, we see that

$$y = \frac{y_1}{\left(\frac{i}{\mathrm{T}}\right)} \frac{e}{m v} = \mathrm{B} \frac{e}{m v},$$

$$x = \mathrm{A} \frac{e}{m v^2},$$

where A and B are constants depending on the position of the screen and the magnitudes of the electric and magnetic forces. These quantities can be calculated by means of the equations just given.

Since

$$\frac{y}{x} = \frac{B}{A} v,$$

$$\frac{y^2}{x} = \frac{B^2}{A} \frac{m}{e}.$$

We see that if the pencil is made up of rays having a constant velocity, but having all values of $\frac{e}{m}$ up to a maximum value, the spot of light will be spread out by the magnetic and electric fields into a straight line extending a finite distance from the origin. While if it is made up of two sets of rays, one having the velocity v_1 the other the velocity v_2, the spot will be drawn out into two straight lines as in Fig. 4.

If $\frac{e}{m}$ is constant and the velocities have all values up to a maximum, the spot of light will be spread out into a portion of a parabola, as indicated in Fig. 5.

Fig. 4.

Fig 5.

We shall later on give examples of each of these cases.

The discharge was produced by means of a large induction coil, giving a spark of about 50 cm. in air, with a vibrating make and break apparatus. Many tubes were used in the course of the investigation; the dimensions of these varied slightly. The distance of the screen from the hole from which the rays emerged was about 9 cm., the length of the parallel plates about 3 cm., and the distance between them ·3 cm.

Properties of the Positive Rays when the Pressure is not exceedingly low.

The appearance of the phosphorescent patch after deflection in the electric and magnetic fields depends greatly upon the pressure of the gas. I will begin by considering the case when the pressure is comparatively high, say of the order of $\frac{1}{50}$ mm. At these pressures, though the walls of the tube in front of the cathode were covered with bright phosphorescence and the dark space extended right

up to the walls of the tube, and was several centimetres thick, traces of the positive column could be detected in the neighbourhood of the anode. I will first take the case where the tube was filled with air. Special precautions were taken to free the air from hydrogen; it was carefully dried, and a subsidiary discharge-tube, having a cathode made of the liquid alloy of sodium and potassium, was fused on to the main tube. When the discharge passes from such a cathode it absorbs hydrogen. The discharge was sent through this tube at the lowest pressure at which enough light was produced in the gas to give a visible spectrum, until the hydrogen lines disappeared and the only lines visible were those of nitrogen and mercury vapour. This pressure was a little higher than that used for the investigation of the positive rays, but a pump or two was sufficient to bring the pressure down to this value. The appearance of the phosphorescence on the screen when the rays were deflected by magnetic and electric forces separately and conjointly is shown in Fig. 6.

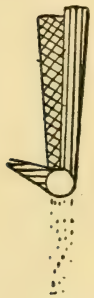

FIG. 6.

The deflection under magnetic force alone is indicated by vertical shading, under electric force alone by horizontal shading, and under the two combined by cross shading.

The spot of phosphorescence is drawn out into a band on either side of its original position. The upper portion, which is very much the brighter, is deflected in the direction which indicates that the phosphorescence is produced by rays having a positive charge; the lower portion (indicated by dots in the figure), which though faint is quite perceptible on the willemite screen, is deflected as if *the rays carried a negative charge*. The length of the lower portion is somewhat shorter than that of the upper one, but is quite comparable with it. The intensity of the luminosity in the upper portion is at these pressures quite continuous; no abrupt variations such as would show themselves as bright patches could be detected, although, as will be seen later on, these make their appearance at lower pressures. Considering for the present the upper portion, the straightness of the edges shows that the velocity of the rays is approximately constant, while the values of $\frac{e}{m}$ range from zero at the undeflected portion to the value approximately equal to 10^4 at the top of the deflected band. This value of $\frac{e}{m}$ is equal to that for a charged hydrogen atom, and, moreover, there was no specially great luminosity in the positions corresponding to $\frac{e}{m} = \frac{10^4}{14}$ and $\frac{10^4}{16}$, the values for rays carried by nitrogen or oxygen atoms, though these places were carefully scrutinised. As hydrogen when present as an impurity in the tube has a tendency

to accumulate near the cathode, the following experiment was tried to see whether the Kanalstrahlen were produced from traces of hydrogen in the tube. The discharge was sent through the tube in the opposite direction, i.e. so that the perforated electrode was the anode, the electric and magnetic fields being kept on. When the discharge passed in this way there was, of course, no luminosity on the screen; on reversing the coil again, so that the perforated electrode was the cathode, the luminosity flashed out instantly, presenting exactly the same appearance as it had done when the tube had been running for some time with the perforated electrode as cathode.

The fact that a spot of light produced by the undeflected positive rays is under the action of electric and magnetic forces drawn out into a continuous band was observed by W. Wien, who was the first to measure the deflection of the positive rays under electric and magnetic forces. The values of $\frac{e}{m}$ obtained from the deflections of various parts of this band range continuously from zero, the value corresponding to the undeflected portion, to 10^4, the value corresponding to those most deflected. Wien explained this by the hypothesis that the charged particles which make up the positive rays act as nuclei, round which molecules of the gas through which the rays pass condense, so that very complex systems made up of a very large number of molecules get mixed up with the particles forming the positive rays, and that it is these heavy and cumbrous systems which give rise to that part of the luminosity which is only slightly deflected. I think that the constancy of the velocity of the rays, indicated by the straight edges of the deflected band, is a strong argument against this explanation, and that the existence of the negative rays is conclusive against it. These negatively electrified rays, which form the faintly luminous portion of the phosphorescence indicated in Fig. 6, are not cathode rays. The magnitude of their deflection shows that the ratio of $\frac{e}{m}$ for these rays, instead of being as great as $1 \cdot 7 \times 10^7$, the value for cathode rays, is less than 10^4. The particles forming these rays are thus comparable in size with those which form the positive rays. The existence of these negatively electrified rays suggests at once an explanation, which I think is the true one, of the continuous band into which the spot of phosphorescence is drawn out by the electric and magnetic fields. The values of $\frac{e}{m}$ which are determined by this method are really the mean values of $\frac{e}{m}$, while the particle is in the electric and magnetic fields. If the particles are for a part of their course through these fields without charge, they will not during this part of their course be deflected, and in consequence the deflections observed on the screen, and conse-

quently the values of $\frac{e}{m}$, will be smaller than if the particle had retained its charge during the whole of its career. Thus, suppose that some of the particles constituting the positive rays, after starting with a positive charge, get this charge neutralised by attracting to them a negatively electrified corpuscle, the mass of the corpuscle is so small in comparison with that of the particle constituting the positive ray that the addition of the particle will not appreciably diminish the velocity of the positive particle. Some of these neutralised particles may get positively ionised again by collision, while others may get a negative charge by the adhesion to them of another corpuscle, and this process might be repeated during the course of the particle. Thus there would be among the rays some which were for part of their course unelectrified, at other parts positively electrified, and at other parts negatively electrified. Thus the mean value of $\frac{e}{m}$ might have all values ranging from α, its initial value, to $-\alpha'$, where α' might be only a little less than α. This is just what we observe, and when we remember that the gas through which the rays are passing is ionised, and contains a large number of corpuscles, it is, I think, what we should expect.

At very low pressures, when there are very few ions in the gas, this continuous band stretching from the origin is replaced by discontinuous patches.

Positive Rays in Hydrogen.

In hydrogen, when the pressure is not too low, the brightness of the phosphorescent patch is greater than in air at the same pressure; the shape of the deflected phosphorescence is markedly different from that in air. In air, the deflected phosphorescence is usually a straight

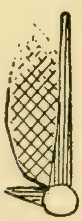

Fig. 7.

Fig. 8.

band, whereas in hydrogen the boundary of the most deflected side is distinctly curved and is concave to the undeflected position. The appearance of the deflected phosphorescence is indicated in Fig. 7.

The result indicated in Fig. 8, which was also obtained with hydrogen, shows that we have here a mixture of two bands, as indi-

cated in Fig. 4, the two bands being produced by carriers having different maximum values of $\frac{e}{m}$. The greatest value of $\frac{e}{m}$ obtained with hydrogen was the same as in air, $1 \cdot 2 \times 10^4$, the velocity was $1 \cdot 8 \times 10^8$ cm. per sec. The presence of the second band indicates that mixed with these we have another set of carriers, for which the maximum value $\frac{e}{m}$ is half that in the other band, i.e. 5×10^3. The curvature of the boundary generally observed is due to the admixture of these two rays.

Positive Rays in Helium.

In helium the phosphorescence is bright, and the deflected patch has in general the curved outline observed in hydrogen. I was fortunate enough, however, to find a stage in which the deflected patch was split up into two distinct bands, as shown in Fig. 9. The maximum value of $\frac{e}{m}$ in the band *a* was $1 \cdot 2 \times 10^4$, the same as in air and hydrogen, and the velocity was $1 \cdot 8 \times 10^8$; while the maximum value of $\frac{e}{m}$ in band *b* was almost exactly one quarter of that in *a* (i.e. $2 \cdot 9 \times 10^3$). As the atomic weight of helium is four times that of hydrogen, this result indicates that the carriers which produce the band *b* are atoms of helium. This result is interesting because it is the only case (apart from hydrogen) in which I have found values of $\frac{e}{m}$ corresponding to the atomic weight of the gas; and even in the case of helium, when the pressure in the discharge-tube is very low and the electric field very intense, the characteristic rays with $\frac{e}{m} = 2 \cdot 9 \times 10^3$ sometimes disappear, and, as in all the gases I have tried, we get two sets of rays, for one set of which $\frac{e}{m} = 10^4$ and for the other 5×10^3.

Fig. 9.

Although the helium had been carefully purified from hydrogen, the band *a* (for which $\frac{e}{m} = 10^4$) was generally the brighter of the two. The case of helium is an interesting one; for the class of positive rays, known as the α rays, which are given off by radioactive substances, would *a priori* seem to consist most probably of helium, since helium is one of the products of disintegration of these substances. The value of $\frac{e}{m}$ for these substances is 5×10^3, where we have seen that in helium it is possible to obtain rays for which $\frac{e}{m} = 2 \cdot 9 \times$

10^3. It is true that, at very low pressures and with strong electric fields, we get rays for which $\frac{e}{m} = 5 \times 10^3$; but this is not a peculiarity of helium; all the gases which I have tried show exactly the same effect.

Argon.

When the discharge passed through argon, the effects observed were very similar to those occurring in air. The sides were perhaps a little more curved, and there was a tendency for bright spots to develop. The measurements of the electric and magnetic deflection of these spots gave $\frac{e}{m} = 10^4$, the value obtained for other cases. There was no appreciable increase of luminosity in the positions corresponding to $\frac{e}{m} = \frac{10^4}{40}$, as there would have been if an appreciable number of the carriers had been argon atoms.

Positive Rays in Gases at very low pressures.

As the pressure of the gas in the discharge-tube is gradually reduced, the appearance of the deflected phosphorescence changes: instead of forming a continuous band, the phosphorescence breaks up into two isolated patches; that part of the phosphorescence in which the deflection was very small disappears, as also does the phosphorescence produced by the negatively electrified portion of the rays.

In the earlier experiments considerable difficulty was experienced in working at these very low pressures; for when the pressure was reduced sufficiently to get the effects just described, the discharge passed through the tube with such difficulty, that in a very few seconds after this stage was reached sparks passed from the inside to the outside of the tube, perforating the glass and destroying the vacuum. In spite of all precautions, such as earthing the cathode and all conductors in its neighbourhood, perforation took place too quickly to permit measurements of the deflection of the phosphorescence.

This difficulty was overcome by taking advantage of the fact that, when the cathode is made of a very electropositive metal, the discharge passes with much greater ease than when the cathode is made of aluminium or platinum. The electropositive metals used for the cathode were: (1) the liquid alloy of sodium and potassium which was smeared over the cathode, and (2) calcium, a thin plate of which was affixed to the front of the cathode. With these cathodes, the pressure in the tube could be reduced to very low values without making the discharge so difficult as to lead to perforation of the tube by sparking, and accurate measurements of the position of the patches of phosphorescence could be obtained at leisure.

The results obtained at these low pressures are very interesting. Whatever kind of gas may be used to fill the tube, or whatever the nature of the electrode, the deflected phosphorescence splits up into two patches. For one of these patches the maximum value of $\frac{e}{m}$ is about 10^4, the value for the hydrogen atom; while the value for the other patch is about 5×10^3, the value for α particles or the hydrogen

Hydrogen

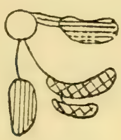

FIG. 10.

Helium

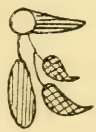

FIG. 11.

Air

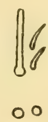

FIG. 12.

molecule. Examples of the appearance of this phosphorescence are given in Figs. 10, 11 and 12. In Fig. 12 the magnetic force was reversed.

The differences in the appearance are due to differences in the pressure rather than to differences in the gas; for at slightly higher pressures than that corresponding to Fig. 12, the appearance shown in Figs. 10 and 11 can be obtained in air. In all these cases the more deflected patch corresponds to a value of about 10^4 for $\frac{e}{m}$ while $\frac{e}{m}$ for the less deflected patch is about 5×10^3.

It will be noticed that in Fig. 11 there is no trace in the helium tube of rays for which $\frac{e}{m} = 2{\cdot}5 \times 10^3$, which were found in helium tubes at higher pressures; at intermediate pressures there are *three* distinct patches of helium, for the first of which $\frac{e}{m} = 10^4$, for the second $\frac{e}{m} = 5 \times 10^3$, and for the third $\frac{e}{m} = 2{\cdot}5 \times 10^3$ approximately. Helium is a case where there are characteristic rays—i.e. rays for which $\frac{e}{m} = \frac{10^4}{M}$, where M is the atomic weight of the gas, when the discharge potential is comparatively small, and not when, as at very low pressures, the discharge potential is very large. I think it very probable that, if we could produce the positive rays with much smaller potential differences than those used in these experiments, we might get the characteristic rays for other gases. I am at present investigating with this object the positive rays produced when the perforated cathode is, as in Wehnelt's method, coated with lime, when a potential

difference of 100 volts or less is able to produce positive rays. The interest of the experiments at very low pressures lies in the fact that in this case the rays are the same whatever gas may be used to fill the tube ; the characteristic rays of the gas disappear, and we get the same kind of carriers for all substances.

I would especially call attention to the simplicity of the effects produced at these low pressures ; only two patches of phosphorescence are visible. This is, I think, an important matter in connection with the interpretation of these results ; for at these low pressures we have to deal, not only with the gas with which the tube was originally filled, but also with the gas which is given off by the electrodes and the walls of the tube during the discharge; and it might be urged that at these low pressures the tube contained nothing but hydrogen given out by the electrodes. I do not think this explanation is feasible, for the following reasons :—

(1) The gas developed during the discharge is not wholly hydrogen ; if the discharge is kept passing long enough to develop so much gas that the discharge through the gas is sufficiently luminous to be observed by a spectroscope, the spectrum always showed, in addition to the hydrogen lines, the nitrogen bands ; indeed, the latter were generally the most conspicuous part of the spectrum. If the phosphorescent screen on which the positive rays impinge is observed during the time this is being given off, the changes which take place in the appearance of the screen are as follows : If, to begin with, the pressure is so slow that the phosphorescent patches are reduced to two bright spots, then, as the pressure begins to go up owing to the evolution of the gas, the deflection of the spots increases. This is owing to the reduction in the velocity of the rays consequent upon the reduction of the potential difference between the terminals of the tube, as at this stage an increase in the pressure facilitates the passage of the discharge. In addition to the increase in the displacement there is an increase in the area of the spots giving a greater range of values of $\frac{e}{m}$; this is owing to the increase in the number of collisions made by the particles in the rays on their way to the screen. As more and more gas is evolved the patches get larger, and finally overlap ; the existence of the second patch being indicated by a diminution in the brightness of the phosphorescence at places outside its boundary. As the pressure increases the luminosity gets more and more continuous, and we finally get to the continuous band, as shown in Fig. 6. At this stage it is probable that there may be enough luminosity to give a spectrum showing the nitrogen lines, indicating that a considerable part of the gas in the tube is air. It is especially to be noted that during this process, when gas was coming into the tube, there has been no development of patches in the phosphorescence indicating the presence of new rays ; on the contrary, one type of

carrier—that corresponding to $\frac{e}{m} = 5 \times 10^3$—has disappeared. The presence of the nitrogen bands in the spectrum shows that nitrogen is carrying part of the discharge, and yet there are no rays characteristic of nitrogen to be observed on the screen ; a proof, it seems to me, that different gases may be made by strong electric fields to give off the same kind of carriers of positive electricity.

Another result, which shows that the positive rays are the same although the gases are different, is the following. The tube was pumped until the pressure was much too low for the discharge to pass, then small quantities of the following gases were put into the tube : air, carbonic oxide, hydrogen, helium, neon (for which I am indebted to the kindness of Sir James Dewar) ; the quantity admitted was adjusted so that it was sufficient to cause the discharge to pass, and yet did not raise the pressure beyond the point where the phosphorescence is discontinuous. In every case there were patches corresponding to $\frac{e}{m} = 10^4$, $\frac{e}{m} = 5 \times 10^3$, and except with helium these were the only patches ; in helium, in addition to the two already mentioned, there was a third patch for which $\frac{e}{m} = 2 \cdot 5 \times 10^3$.

I also tried another method of ensuring that at these low pressures there were other gases besides hydrogen in the tube. I filled the tube with helium, and after exhausting to a fairly low pressure by means of the mercury pump, I performed the last stages of the exhaustion by means of charcoal cooled with liquid air. This charcoal absorbs very little helium in comparison with other gases ; so that it is certain that there was helium in the tube. The appearance of the phosphorescent screen of tubes exhausted in this way did not differ from those exhausted solely by the pump.

The most obvious explanation of these effects seems to me to be, that under very intense electric fields different substances give out particles charged with positive electricity, and that these particles are independent of the nature of the gas from which they originate. These particles are, as far as we know at present, of two kinds ; for one kind $\frac{e}{m}$ has the value of 10^4, that of an atom of hydrogen ; for the other kind $\frac{e}{m}$ has half this value, i.e. it has the same value as for the α particles from raidioactive substances.

This agreement in the maximum value of $\frac{e}{m}$ at different pressures is a proof that this is a true maximum, and that there are not other more deflected rays not strong enough to produce visible phosphorescence ; for if this were the case—i.e. if the value of $\frac{e}{m}$ for a particle

that had never lost its charge temporarily by collision were greater than 10^4—we should expect to get larger values for $\frac{e}{m}$ at low pressures than at high.

I have much pleasure in thanking my assistant, Mr. E. Everett, for the assistance he has given me in these experiments.

[J. J. T.]

GENERAL MONTHLY MEETING,

Monday, April 8, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Edward Taylor Hanson, Esq., B.A.
Mrs. H. Loeffler,
Sir Alexander Pedler, C.I.E. F.R.S.
John Percy Smith, Esq.
Frederic Harold Sully, Esq..
Alfred Herbert Tubby, Esq., M.S. F.R.C.S.
Christopher Alfred Woods, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir Andrew Noble, K.C.B. F.R.S., for his Donation of £200 to the Fund for the Promotion of Experimental Research at Low Temperatures; and to Lady Kelvin for her Gift of a Statuette of the Right Hon. Lord Kelvin, O.M. G.C.V.O. P.C. F.R.S. *M.R.I.*

The Chairman reported the decease of M. Marcellin Berthelot on the 18th of March, and of Dr. Allan Macfadyen on the 1st of March, and the following Resolutions passed by the Managers at their Meeting held this day were read and adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the irreparable loss to Science and to the Institution in the decease of their Honorary Member, M. Marcellin Pierre Eugène Berthelot, Hon.F.R.S. Hon.F.C.S. Grand Croix de la Légion d'Honneur, Sénateur, Membre de l'Institut, Secrétaire perpétuel de l'Académie des Sciences, Paris.

M. Berthelot was elected an Honorary Member of the Royal Institution on the occasion of the Faraday Centenary in 1891.

He was presented with an address from the Members of the Royal Institution on the occasion of the Jubilee of his Scientific Researches, and Professor Cornu and Professor Mascart, as Honorary Members, represented the Royal Institution at the celebration in Paris on November 24, 1901.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of their most sincere sympathy and heartfelt condolence with the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss to Science and to the Institution in the decease of the late Fullerian Professor of Physiology, Allan Macfadyen, M.D. B.Sc., formerly Director of the Lister Institute of Preventive Medicine.

Dr. Macfadyen, in a series of experiments encouraged by Professor Sir James Dewar and aided by the Cryogenic Laboratory of the Royal Institution, investigated the influence of low temperatures on bacterial life with most

remarkable results, and on June 8, 1900, delivered a Friday Evening Discourse on the subject of his researches.

Dr. Macfadyen was elected in 1901 Fullerian Professor of Physiology in the Royal Institution.

The Managers desire to offer to the family on behalf of the Members of the Royal Institution of Great Britain the expression of the most sincere sympathy with them in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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Journal of the British Dental Association for March, 1907. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 12, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Professor A. H. Church, M.A. D.Sc. F.R.S. *M.R.I.*

Conservation of Historic Buildings and Frescoes.

[illustrated by experiments, etc.]

The title of this discourse as announced is in one way too wide, in another too narrow. For "Conservation of historic buildings" should be substituted "Conservation of urban stone-work," and for "frescoes," "wall-paintings."

We have to consider the nature of the attack and of the defence —the weapons and the armour—the damage and its repair.

First amongst the destructive agents at work, I am bound to place sulphuric acid. I own myself a *thiophobist.* Dr. S. Rideal has given an estimate of the amount of sulphuric acid poured year by year into the atmosphere of London by the coal burnt in the Metropolis. His minimum figure is half a million tons, his maximum twice that amount. But before dealing with this branch of the subject in some detail, let me enumerate the chief of the other hostile agents. These are soot and tarry matters from coal-smoke; water, and an abnormal amount of carbon dioxide. Minor enemies, such as hydrochloric acid, nitric acid, and ammonia salts, with sulphuretted hydrogen and sulphurous acid, may be put on one side for the nonce. These assist in doing damage—damage of a special kind and to particular materials, such as metals and organic pigments—but our attention ought to be focused on the chief mischief-makers. Before dealing with these, I may observe that it is fortunate that nitric and hydrochloric acids exist in but small proportion in the atmosphere of towns, save in the immediate vicinity of certain factories. I say "fortunate," for at present no efficient chemical means of neutralising their bad effects upon stone-work seems available. Of course, one may use mechanical methods of protection, but a stone which has been attacked by nitric or by hydrochloric acid does not allow of the entire conversion of its soluble salts into insoluble, nor, in consequence, of its reconsolidation.

Even now I cannot approach closely the story of our arch-enemy, sulphuric acid, without previously saying a few words about soot and tar, about water and carbon dioxide. The important *rôle* of these last in the natural weathering of stone does not need discussion,

but water, as the carrier of sulphuric acid, and as the solvent of the sulphates which it helps to form, must not be passed by in silence. Moreover, the condensation of moisture upon the surface of a fresco within a building is most harmful, and, when it cannot be wholly avoided, indicates the desirability of applying some waterproofing material to the surface of the painting. As to soot and tarry matters just named, one has to remember that even the total abolition of smoke from coal would in no way lessen the amount of sulphuric acid produced in the burning of this fuel. The soot and tar of smoke do indeed adhere to stone and discolour and disfigure it, but, except as carriers of acid, they do not corrode it. Here are some figures * from the analysis of a strange crust attached to the under-surface of the cornice above the colonnade and below the dome of St. Paul's Cathedral. This crust has been slowly formed by the conversion of calcium carbonate from the Portland stone above the cornice into gypsum. This change has been effected by the acid rain of the City; then the gypsum has entered into solution, which has hung as drip to the lower face of the cornice, until the water has evaporated, and the residual gypsum, dirty with 1 per cent. of soot and somewhat less of tar, has formed a stalactitic mass, occasionally reaching 3 inches in depth. The phosphate of lime present has never been dissolved, but represents one of the constituents of the original stone mechanically carried down and entangled in this dark grey stalactitic gypsum. But a visit to St. Paul's is not needed to see what damage is wrought by sulphuric acid even upon Portland stone. Its decay, being regular and even, often passes unnoticed, but may be watched in all parts of London, in balconies, copings, quoins, etc.

Allow me to direct your attention to the two chief sources of the sulphuric acid found in the atmosphere of towns, and especially in that of London. There is no doubt that the larger proportion of the sulphur present in coal escapes during burning in the forms of sulphuric and sulphurous acids. Some remains in the ash, and this quantity may be increased by mingling slaked lime with the coal. [Lime-water, which has been suggested for this purpose, is far too weak; indeed, 3000 gallons, or 13 tons 8 cwt. would be required to fix the sulphur of 1 ton of coal!] But there is a minor though by no means negligible source of sulphuric acid in the products of the combustion of coal-gas. Since the restrictions imposed on the gas companies of London have been relaxed—that is, since

* Incrustation from drip below cornice, St. Paul's:—

Carbon	1·01 per cent.
Ammonium sulphate	0·93 ,,
Tar	0·60 ,,
Gypsum	73·80 ,,
Calcium phosphate..	2·22 ,,
Calcium carbonate	none.

October 1, 1905—the sulphuric acid from this source has trebled in amount, the average during the twelve months preceding the relaxation having been $11\frac{1}{2}$ grains per 100 cubic feet, but in the subsequent twelve months 33 grains.* And during the present year so far there have been 43 grains in the same volume of gas.† If sulphuric acid from coal-gas (like that from paraffin oil) contribute in a comparatively small degree to the pollution of the *general* atmosphere of London, yet it exerts a definitely injurious action when produced in close proximity to an easily affected surface, such as that of a fresco, *within* a building. No wonder that the delicate film of calcium carbonate, the distinctive binding material in a fresco, soon perishes under the onset of oil of vitriol. And here let me anticipate, what I might have relegated to a later part of my discourse, namely, a few words concerning the best-known frescoes in the Palace of Westminster. I do this because the question of sulphur in gas happens to have been brought into close connection with these paintings in consequence of a Parliamentary paper published last year (Cd. 3085). For it has been urged, by one who writes in the interest of the gas companies, that "the hydrochloric and nitric acid vapours resulting from the electric arc-lamps in nightly use in London are even more pernicious than the burning of gas as at present manufactured." But it must be remembered that analyses of London air and London rain prove that the proportion of these acids present is quite insignificant compared with that of sulphuric acid. The same writer states that five frescoes in the King's Robing Room in Westminster Palace have required nothing more than the removal of dirt. This remark is made in order to discount a statement of mine in the Parliamentary Paper just named. This statement runs thus: "The increased and increasing consumption of coal in London, and the greater licence allowed to the gas companies in the matter of freeing their gas from sulphur compounds, must result in a serious augmentation of sulphuric acid in the air of the Metropolis." But very much more than the removal of dirt was needed. The frescoes are no longer frescoes. While retaining the aspect of frescoes, they have been transformed gradually into paintings which have had their original binding material replaced mainly by ceresin and paraffin wax, unalterable compounds competent to resist even sulphuric acid. But not only has the painted surface required fresh protection, but the ground itself has had to be strengthened by similar treatment, the plaster having been in many places "sulphated" and thus rendered rotten. So the writer in the *Gas World* of August 25, 1906, has *not*, as he says, hunted out "this

* I have to thank Dr. F. Clowes, Chief Chemist and Superintending Gas-Examiner to the London County Council, for his kindness in supplying me with the official figures.

† The corrosion of copper-boilers heated by gas is now so much more rapid that their lives are shortened in the ratio 7 to 2.

mare's nest," and has *not* furnished a "typical illustration of the reason why what is called 'science' does not carry conviction when dragged into the concerns of everyday life."

And now I turn to another criticism of the already-quoted passage from the latest Parliamentary Paper on Westminster Frescoes. It will be found in the *Journal of Gas-Lighting* for the 21st of August last. The writer, after stating that he is not then disposed to question the opinion that it is sulphuric acid which is the chief destructive agency at work on the Westminster paintings, argues that the greater licence allowed to the gas companies will result in an improvement of the London atmosphere. The line of argument is this—"the use of gas will be extended by the cheapening which the slight increase in the permitted amount of sulphur in the gas involves," and in consequence less coal will be burnt, and there will, therefore, be less sulphuric acid produced and thrown into the atmosphere. For it is admitted on all sides that the burning of coal is the prime source of the sulphuric acid in the general atmosphere. Of course, if gas were made at or near the collieries, and sent in pipes to the big cities, and displaced coal for heating as well as lighting purposes, we should go far to abolish town smoke, and should greatly lessen the sulphuric acid. Unfortunately for the actual cogency of this argument, we find that the three chief gas companies in the county of London have NOT reduced the price of their gas in the smallest degree since the relaxation of the rule about sulphur, while their gas has, during the twelve months ending September 30, 1906, discharged into the air three times as much sulphuric acid (and during the last three months, four times) as it did under the former restrictions. This is, indeed, a retrogade movement.

One word more on this subject. The wall-paintings in the King's Robing Room at Westminster are, or rather were, true frescoes. Yet the *Westminster Gazette* stated, in one of its issues of August last, that a representative of the paper was told at the National Gallery that these works "were not real frescoes, but oil (paintings) on soft plaster." No person of experience and authority in Trafalgar Square could have made so absurd a statement. The paintings referred to, five in number, were executed in true fresco by William Dyce, R.A., four of them between the years 1851 and 1854, while the fifth and largest was completed in 1864.

Before going further, I would direct attention to the amount of calcium carbonate which the minimum quantity of sulphuric acid yearly produced from coal-burning in London might transform into gypsum: 500,000 tons of this acid might destroy 510,200 tons of $CaCO_3$, evolve 204,080 tons of CO_2, and produce 877,544 tons of gypsum. This change is, moreover, accompanied by an expansion, 100 vols. of calcite producing 120 vols. of gypsum. Happily, part of this sulphuric acid passes away harmlessly, yet the havoc done is everywhere conspicuous, on stone, marble, and mortar.

Thus far I have spoken about our chief agent of destruction, sulphuric acid, describing its sources and connecting it with stone-work on the one hand and with certain mural paintings on the other. Now I purpose dealing with certain preservative and restorative methods, taking, in the first place, stone-work into consideration, then mural paintings. My aim will be to describe two methods of treatment only—methods which are not always available, though of wide application.

In reference to decayed stone-work, I would name the opening section, "Baryta *versus* Lime." Some persons interested in the preservation of old buildings, and rightly refusing the ignominy of a coat of common oil-paint or of prosaic cement, recommend treating the decayed and decaying stone with lime-wash (not whitewash). The suggestion is an obvious one, but unfortunately this easy process is unsound in theory and ineffective in practice. Moreover, the application of this meagre and indiscriminate lime-wash destroys all the exquisite qualities of tone, colour, texture, and translucency which old stone-work may still possess in its age-worn state. And it cannot fail to distort and even obliterate the last surviving traces of surface-enrichment, of mouldings and carvings (e.g. the decaying diaper-work in the spandrils of the great arches in Westminster Abbey). Lime forms a crust on the decayed stone; lime does nothing towards the consolidation of the disintegrated substance; lime is itself subject to the same chemical changes which have injured the stone; lime sooner or later falls off, bringing with it the decayed layer to which it was applied. Mr. Thackeray Turner, one of the most strenuous apostles of the gospel of lime-wash, cited (*The Times*, Nov. 1904) the case of lime-washed cottages in Wales and Cornwall as evidence of the preservative value of this treatment. But as we are dealing with urban, not rural conditions, such evidence is beside the mark. Baryta, it is true, is a late-comer compared with lime, but it has been used, though in a half-hearted way, for forty-five years. Had it been known earlier to the advocates of lime, it ought to have ousted that earth ere now. Having the choice of two earths, why not use the better? Please visit the eastern walk of Westminster Cloister, and look at what has just been done to the ribs and vault of the bay nearest to the Abbey: no London dirt can turn that prose into poetry.

Let me describe the theory and practice of the baryta treatment of decayed stone. Baryta-water only is used, the liquid being applied, when all dust has been blown away, in the form of extremely fine spray, on very tender surfaces. Where the stone will bear it a rose syringe or even a brush may be used. Indeed, after a few sprayings and the lapse of a week or two, it is generally feasible to adopt the rougher methods. The rationale of the action of baryta is simple. The liquid, which saturated at 16° C. contains about 3 per cent. of BaO, is absorbed by the decayed stone, penetrating to some

depth, maybe 2 or 3 inches. The baryta forms an insoluble sulphate with the sulphuric constituent of the gypsum and of the other sulphates present, and at the same time calcium hydrate is set free. This probably in part unites with soluble silica, but most of it gradually becomes carbonated, while the calcium carbonate thus regenerated actually reconstitutes the original binding cement of the stone. This description applies especially to sandstones compacted by calcium carbonate; but limestones illustrate the same changes. Unlike water-glass and most of the other preparations which have been used for hardening decayed stone, baryta forms no crust on the surface—indeed it produces no sensible effect on the outside until it has penetrated to the deeper parts. So when the Dean of Exeter, in his letter published in *The Times* of October 26 last year, stated that "baryta does not strengthen the stone structurally, it forms a scab of the decayed stone on the face of the sound stone, which, as is the case with all scabs, has a tendency to peel off," he could hardly have selected more inappropriate terms in which to describe the action of baryta. The Dean further stated his belief "that the decay at Westminster was comparatively superficial." Surely very few persons would regard a decayed layer 2 or even 3 inches thick as superficial. And he raises the question whether the baryta-treatment, which his friend the late Mr. Micklethwaite told him seems to be successful "so far as we can judge yet," would answer outside a building as well as inside. Here I may cite the case of Chichester bell-tower. On analysing portions of decayed stone from the several faces of this campanile, I found sufficient sulphate present to warrant the application of baryta-water to a trial area. This was done some two or three years ago with so satisfactory a result that last July Mr. Somers Clarke, the architect, decided upon treating the whole of the octagon and the entire north face of the tower in the same way. He wrote: "the success of the experiment fully justifies this course." There is no need to multiply references to other successful trials of the use of baryta in strengthening external decayed stone-work and stopping further damage, but in the course of the next few years it may be expected that many favourable reports will be forthcoming. Experiments, large and small, are being and have lately been made, and promise well. I have been told of a single failure, but I learnt that the architect in charge expected that the consolidation of the decayed stone would be immediate, and had not waited for the few weeks needed for carbonation to take place. It is proper to add that where decay has resulted from simple weathering—the action mainly of water and carbon dioxide—or from sea-salt, the treatment with baryta is not applicable. Nor should it be forgotten that baryta water is toxic and easily spoilt by exposure to the air. Then, again, there may be too little gypsum present in the decayed stone for an adequate liberation of caustic lime to occur. Without

laying down a rule it may be said that where so much as 2 per cent. of this sulphate is present, baryta treatment is indicated.

In the case of the Chapter House, Westminster, to which I would now direct your attention, the proportion of calcium sulphate was far more than enough to fulfil this condition of success. Although the exterior of this building, and most of the upper portions of the interior, date only from the time of the restoration carried out by Sir Gilbert Scott, much of the original work of the middle of the thirteenth century remains. The Chapter House is in the custody of H.M. Office of Works. The successive First Commissioners, as well as the permanent officers of that Board, have sympathetically regarded its preservation. So when, in 1900, Mr. Micklethwaite pointed out to Mr. Akers Douglas that serious tokens of disintegration were appearing in the early mouldings of the arcading, in the diaper work above, and in the exquisite sculpture of the soffits of the entrance arch, my advice was asked. A few experiments showed that the disintegration and efflorescence were due to the formation of gypsum, which, in some parts of the decayed material, exceeded 17 per cent.* The stone, a sandstone, with a calcareous cement, and known as firestone and Reigate stone, no longer contained any calcium carbonate—indeed, had an acid reaction, and yielded to water 4 per cent. of saline matter other than gypsum. You will recollect that the conversion of calcium carbonate into gypsum is not only accompanied by the evolution of much carbon dioxide, but that 100 volumes of the former compound expand into 120 volumes of the latter. The rationale of the baryta treatment, to which the interior stone-work of the Chapter House was submitted, has been already described; but it may be added that the operations were commenced in May 1900, continued in 1901, and completed in 1903. Before treatment, a touch of the finger sufficed to bring away the surface of the carving, afterwards the stone was as sound as that newly quarried, and harder. Incidentally it was found that decayed Purbeck marble was equally amenable to treatment; a column at the right hand wall of the entry having been successfully *barytised.* In no instance has there been a sign of subsequent deterioration. Full particulars are to be found (of the operations in the Chapter House) in the Parliamentary Paper, Cd. 1899/1904. It may be of interest to mention that 220 gallons of baryta-water sufficed to treat 560 superficial yards in the Chapter House and its inner entry, eight applications being

* Reigate Firestone :—

	From the Quarry.	*From the Chapter-House.*
Calcium Carbonate	9·2 per cent.	none.
Gypsum	trace	17·85 per cent.
Chlorine	trace	·37 ,,
Ammonia	trace	·05 ,,
Reaction	neutral	acid.

generally made. The cost of the material was quite trivial, the expense of the treatment being practically confined to the cost of labour and scaffolding. Let me add that the cheapest and best source of the baryta is the fused crystalline hydrate, which contains about 58 per cent. of oxide, is less liable to carbonation than the ordinary crystals, and corresponds to the formula BaH_2O_2, 5 H_2O.

With regard to the treatment of the decayed limestone of Canterbury Cathedral, and especially of Bell Harry Tower, a few words only are required. Between November 1901 and June 1902, I made a series of analyses of decayed stones from this tower, and recommended the architect in charge to adopt the treatment with baryta, as an experiment on a small surface. This was to have been done in the summer of 1902, but I heard nothing further of the matter until more than two years afterwards, when I learned that under other advice, the discredited process of the late Jesse Rust, described in his incompleted Patent of June 6, 1861, alternate washes of baryta and fluosilicic acid, but modified by using the fluosilicic acid in smaller proportion than he recommended, was to be tried alongside of the simple baryta solution. I have always deprecated the application of a free acid, such as fluosilicic, to stonework injured by sulphuric acid. For it liberates carbon dioxide from any carbonates present, and at least sometimes produces an unsightly white efflorescence. My own patent process of 1862, according to which alternate applications of baryta-water and of a dialysed solution of silica were to be made, causes no efflorescence and no liberation of CO_2, and has been used with success on large surfaces of new stone. But the silica clogs the pores of the stone, and, if employed at all, should be reserved for the final dressing. My own convictions as to the best course to be pursued at Canterbury, as expressed in my letter to *The Times* of November 19, 1904, remain unaltered in the main, although there are cases in which a hydrofuge substance, such as paraffin or ceresin, may be safely used. But I have never found baryta to fail, where its employment was indicated, on calcareous stone. Nor does disruption ever occur with the repeated but reasonable application of baryta-water saturated at 16° C., although I can easily believe that warm and stronger solutions might be dangerous to use.

Last year five bays of the Cheapside frontage of Mercers' Hall were treated with baryta. The stone was suffering from serious decay, so that some loose portions had to be removed. The treatment with baryta was followed, in the case of the three easternmost sections of the frontage, by another treatment intended to render the re-consolidated stone acid-proof. I had used the second method, with apparent success, in protecting mural paintings in Westminster Palace. It consists in applying to the surface a paste or ointment of ceresin, a solid paraffin wax from ozokerite. The sample used had a solidifying point of 156° F.; it was made into a paste

by taking 4 parts by weight and melting them in the presence of 1 part of terpene and 16 parts of toluol. The mixture is spread on the stone, and then, after the lapse of 24 hours at least, the residual layer of ceresin is driven by heat into the stone. There results a slight deepening of the colour of the surface and a slight marble-like translucency. These changes of tone and colour may be seen on Mercers' Hall by comparing the untreated, the baryta-treated, and the double-treated parts. Ceresin produces a better result than the more crystalline paraffin of the same solidifying point, though the latter is perhaps less liable to be soiled by smoke. It is essential to the success of the double treatment that the calcium hydrate set free by the baryta should be allowed time to become carbonated before the ceresin-paste is applied. The advantage of associating the two treatments lies in this, that while the baryta repairs the damage done by sulphuric acid and reconstitutes the stone structurally, the ceresin waterproofs the surface and prevents further corrosion. Needless to say, it is wiser to waterproof a sound rather than a crumbling stone What Sir Gilbert Scott tried to do in Westminster Cloisters, with his very perishable so-called preservative, shellac in spirit, was the latter, and led to unhappy results. Thus even ceresin or paraffin cannot be always recommended, notably where soluble salts and moisture are present. To coat the outer weak layer of stone-work decayed to some depth tends to form an easily detachable crust.

However, ceresin alone has been used with apparent success so far in the case of the local Camaru stone of which the Bank of Australasia at Melbourne is built. After due laboratory experiments I recommended this treatment in February 1903. Mr. Anketell Henderson, the Melbourne architect, tried aluminium oxalate and other solutions, but found, he says, "that ceresin alone resisted the wet of our torrential rains and the erosion of our sand-storms." He emphasises the use of a ceresin of high melting-point to obviate tackiness, and employs an electric heater in his spraying apparatus. Ordinary oil-paint, frequently renewed, fails to preserve the stone in question. Even a solution of ceresin in petrol proved too weak.

We will now turn to the conservation of mural paintings. Besides ordinary oil-painting four methods are or were in common use—tempera or distemper, true fresco, stereochrome or water-glass, and the so-called spirit fresco of the late Mr. Gambier Parry. The last-named process or method need not detain us, for, so far, the paintings executed in this way, if the ground or plaster have been properly prepared and protected, require nothing further than cleaning with bread. And if they should suffer injury or need further protection, the diluted medium can be used for these purposes. Owing to causes explained in memoranda addressed to H.M. Office of Works, the two well-known lunettes by Lord Leighton (Arts of Peace, 1881–6, and Arts of War, 1870–80) in the Victoria and Albert Museum, have required cleaning and repairing in the manner indicated. But

the long series in the Ambulatory of the Royal Exchange, having been painted on canvas and affixed afterwards to a sound slate backing, have needed no restoration.

The English mediæval wall-pictures found chiefly in churches, but not unknown in secular buildings, are commonly but wrongly called frescoes. They were painted on dry plastered walls with pigments mixed with size or egg-vehicles. By the mistaken use of varnish, by over-drying when on panel, by mechanical injuries, by vibration, by the chemical action of a corrosive atmosphere, and by the penetration of damp or salts from behind or from below, such paintings have generally suffered serious changes, and are sometimes beyond repair. However, there are several ways of consolidating the painted surface and of securing it in position. Before describing these, and how to choose, in any given case, the most suitable, I would urge the importance of precautionary measures, namely, the disuse of gas, the shutting off from the back and foundation of the wall of all sources of damp, and the avoidance of all further accession of soluble salts, such as nitre, ammonia compounds, Epsom salts, and common salt.

The very worst preparation that can be applied to a damaged distemper-painting on a damp or saline ground, is a spirit varnish such as mastic or shellac, or an oil varnish such as copal. Where damp and saline matters are present, a dilute solution (under 5 per cent.) of pure gelatin in very weak spirit may be used : spraying with this liquid kept hot is often successful where the pigments are powdery. Another useful liquid is a 7 per cent. solution of casein in very dilute ammonia, containing 1 per cent. of glycerine. This preparation is particularly applicable for the treatment of distemper paintings on wood. But when the works to be treated are on a perfectly dry backing free from injurious saline matters, then, and then only, is it safe to apply what may be called a hydrofuge or waterproofing material to the surface. This we have in the ceresin-paste already described. Occasionally an extremely small addition of copal varnish to this vehicle may be allowed to secure transparency. Indeed, in my earlier operations of this character I have used Gambier Parry's spirit fresco medium, leaving out the elemi, and replacing the beeswax by paraffin or ceresin. But wherever possible nothing but ceresin-paste or ceresin in solution should be employed. After the cleansing of the surface by an air-blast, by spirits of wine and by bread-crumb, necessary repaints should be executed with powder-pigments mixed with egg-yolk : then the ceresin should be applied. Pigments to be avoided are yellow ochre, raw sienna, terre verte, and ivory black : zinc white should replace flake white.

With regard to the care of water-glass or stereochrome paintings, my experience, confined chiefly to those in Westminster Palace, has been described in full detail in three Parliamentary Papers, Cd. 7651/1895 ; Cd. 8054/1896 ; and Cd. 8893/1898. In these

memoranda, the treatment of Maclise's great paintings in the Royal Gallery, and Herbert's picture of Moses and the Tables of the Law in the Peers' Robing Room, is explained. But the quite recent operations on other mural paintings executed in the same method may claim our attention for a few minutes. The sixteen pictures in the Peers' and Commons' Corridors, unlike the other wall-paintings in the building, have been protected for many years past by tightly-fitting glasses; still they have undergone, in part since they were glazed, some changes. The lime-plaster ground had become, in places, quite soft by sulphation, while the pigments were here and there powdery. Moreover, the grey bloom, which so often appears on stereochrome paintings, had overspread many large areas of these interesting and attractive historical works. This grey bloom had produced, in the seven pictures of the series which were dealt with last summer, a peculiarly unpleasant disturbance of the balance of light and shade. In many places, notably in the robes of the figures, the deep-shaded portions of the folds had become lighter in tone than the general surfaces intended to be fully illuminated. I have said that in the seven pictures dealt with in 1906 some of the colours were powdery, the pigments chiefly affected being yellow ochre, raw sienna, artificial ultramarine, and ivory black. This class of decay was seen at its worst in the picture in the Commons' Corridor by E. M. Ward, "The Landing of Charles II. at Dover, 1660." A list of the seven paintings cleaned and repaired in 1906 may prove useful for reference. And I would beg those of my audience interested in the subject to inspect the series, and to compare the untreated with the treated examples, premising that the condition of those dealt with was far more unsatisfactory than that of the rest:—

In the Peers' Corridor, four works by C. W. Cope painted 1859 to 1866: "The Parting of Lord and Lady Russell"; "The Burial of Charles I."; "Charles I. erecting his Standard at Nottingham"; "Speaker Lenthall asserting the privilege of the Commons."

In the Commons' Corridor, three works by E. M. Ward: "The Last Sleep of Argyll"; "Charles II. and Jane Lane"; "The Landing of Charles II. at Dover, 1660."

The same methods of cleaning and reparation were adopted as in other cases. The grey bloom of silica and sulphate of lime was removed by oblique flicking with soft pads stuffed with carded cotton; cleaning with bread, distilled water, and spirits of wine removed dirt and saline matter. The weak places in the ground were consolidated by means of ceresin, while the whole surface finally received a slight protective film of spirit-fresco medium.

In the early part of this discourse I described, out of their proper place, the repairs executed in the five true frescoes by W. Dyce in the King's Robing Room. I now direct attention to another fresco. It was executed in 1872–3 in the church of St. Stephen, Sydenham Hill,

by Sir Edward J. Poynter, P.R.A. It occupies a recess on the south side of the chancel. The artist had repaired it in tempera not long after its completion, but it subsequently showed signs of more serious deterioration. I found that the usual causes of injury had been at work. Much gas is burnt in the church, some burners being very near the picture. Soot, tar, cobwebs, and dust were first removed from the painted surface; then the weak places in the ground were strengthened by ceresin, and repairs done in tempera colours. Finally, ceresin was applied to protect the restored picture.

There are many other cognate topics on which I should have wished to dwell, and other experiments which I should have been glad to show; but time fails me, and these experiments cannot be completed off-hand. But I may be allowed to state that my warrant for standing here to offer observations on certain methods for preserving stone-work and paintings is derived from long devotion to these subjects. Half a century ago I read a paper on the induration of stone before the Oxford Architectural Society, while in January, 1862, I treated the same topic in a discourse delivered before the Architectural Association. About the same time I was trying experiments on the north front of Westminster Palace. A little later on, about 1865, I dealt with distemper paintings in Cirencester parish church and elsewhere. Waterproofing stone with paraffin wax, driven in by heat, was the subject of several trials in the later sixties. Afterwards the great fresco by G. F. Watts, in Lincoln's Inn Hall, was cleaned and repaired. This was in 1890. To the list may be added the works of Ford Madox Brown, in Manchester Town Hall, and other pictures and other buildings in various parts of the country, besides the long series of mural paintings in Westminster Palace.

I shall be glad if I have said anything to-night which will prove helpful in the preservation of mural paintings and the stone-work in towns, especially in the case of historic buildings. The materials recommended are accessible, cheap, and easily applied. Doubtless there are other preparations available for the same purpose. Some of these may meet those conditions to which the materials I have recommended are not applicable.

[A. H. C.]

WEEKLY EVENING MEETING,

Friday, April 19, 1907.

THE RIGHT HON. EARL CATHCART, D.L. J.P., Manager, in the Chair.

PROFESSOR C. S. SHERRINGTON, M.A. M.D. LL.D. D.Sc. F.R.S.

Nerve as a Master of Muscle.

WE have on the table before us two muscles. The animal was dead when they were taken from it a short while ago. But the animal was, as we are ourselves, an assemblage of organs, and many of these organs go on living for a certain time after the animal, as an animal, is dead. Hence these muscles, carefully removed, are still alive. We notice a marked difference between their behaviour now. To understand the behaviour of organisms we have to think of them as processes rather than as structures. An animal is something happening. The function of muscles is to contract. Of the two muscles now before us, one still goes on contracting, although quite isolated from the body of which it formed a part; but the other does not contract, although that is its function in the body. The muscle which still goes on contracting is the heart; the other is a muscle like the biceps of our own arm. We might think that, as it rests there motionless, it is not alive. It is, however, fully alive. We can satisfy ourselves of that. If I apply to it a faint electric current, it answers by exhibiting its functional activity—it contracts. Yet it does not contract of itself, nor will it, however long we may preserve it; it will die without of itself even contracting once. What is the significance of this difference between the two?

The secret of this difference is largely an affair of the nervous system. The tie between muscular activity and nervous activity is always close; but it is very different in different muscles. The nervous system has been called, with a picturesque truth, the master-system of the body. It controls the action of organs; it controls, quite especially, the activity of the muscles. This heart which we see beating here receives nerves. One of those nerves when stimulated will cause it to contract less, the other to contract more. The contraction of the heart is its "beat." The vagus nerve slows the beating, the other nerve quickens the beating.

The heart is a tubular muscle; it drives blood through itself. When it contracts it squeezes the blood from it into the arteries, and so the blood flows to feed all the myriads of minute lives—cells—

composing the whole complex living animal. The lives of these myriad minute entities all depend on their supply of blood, and therefore the life of the whole creature depends on the contraction of the heart. At each beat the heart by squeezing the blood out of its arterial end maintains the flow of blood, and this flow resulting from its own contraction refills it, because the blood returns to it by the veins.

This beating is all which the heart has to do. Whatever happens it must continue to do this, or the creature perishes. Life-long, night and day, winter and summer, it must do this. Whatever act the creature may be accomplishing, sitting, walking, feeding, sleeping, catching its prey, or escaping its enemies, this beating must go on, in the frog about 10 times a minute, in ourselves about 70 times a minute. The task is monotony itself. How admirably is the heart muscle adapted to fulfil it!

Self-adjustment to meet the environmental conditions differentiates animate from inanimate nature. As characteristic as this self-adjustment itself is its constant trend toward what has sometimes been termed "purpose." Animate objects are observed to adjust themselves to their own advantage, that is, so as to prolong their individual existence or that of their species. The more we know of them the more complete appears to us this trend in their reactions. The living organism advantageously adapts itself to its surroundings. And every part of a living organism exhibits this power. The heart-muscle reveals it clearly. It must not tire, and under normal circumstances the healthy heart, unlike other muscles, shows no fatigue. Its beat must always be strong enough to press its contents over into the artery against considerable resistance which opposes it. A heart-beat which did not expel the blood would be useless, worse than useless, wasteful, because it would be energy spent in vain. Its task can be roughly likened to that of a man with a bucket who has to keep lifting water from a tank at his feet to pour it over a wall of certain height before him. If he lift the bucket much above the wall he expends more energy than he need do; if he lift it less than the wall's height his work fails altogether. If he still, when the bucket is emptied, keep it above the wall's height, his work stops although his effort does not.

The heart, whether its stimulus be weak or strong, beats always with sufficient power: it thus avoids the useless labour of a beat too weak to fulfil the office of a beat. If the heart were to give too prolonged contraction it would defeat its own purpose: after its beat which empties it of blood, it must relax to refill for the next beat: to keep contracted would be for its purpose as harmful as to cease from beating; it would stop the blood instead of pumping it onward. In harmony with this, we find a prolonged stimulus to the heart does not keep the heart contracted; after the heart has replied to the stimulus by a beat it exhibits a refractory phase,

during which it pays no attention to the further stimulation, and relaxes ; and only after it has fully relaxed does it again pay attention to the stimulus and contract, that it to say, beat again. In short, it replies rhythmically to a continued stimulus which would keep the other muscle continuously contracted.

That the heart should go on beating after removal from the body, does not seem greatly surprising because it is still then alive. The wonder lies rather in its continuing to live so long when thus removed : that granted, it seems natural that it should do what it has done previously all its life.

But this other muscle, which likewise continues to live when removed from the body ; it, though it *can* contract, does not. That seems—at least at first sight—the more remarkable. Why does this muscle stop ? So long as it was part of the living creature it showed contraction over and over again. We must turn to the nervous system for our answer.

In the first place let us note that an animal, unlike that other great example of life, a plant, cannot nourish itself from naked earth and air alone. The plant strikes down roots and throws up leaves, and draws through these material and energy with which it can replenish its own substance and activities. Where it as a seed fell, there its foster-mother Earth gives it the food it wants. Not so the animal. It must have subtler and rarer stuffs, or die. The material it needs is not spread so broadcast. It, to replenish itself, must have more special material : it must have for food material that is living, or has lived. To obtain this it has to range about. It has to hunt for it. And it itself is hunted by other animals following the same quest. Therefore its very existence involves locomotion. It must find food and seize it, and must itself escape being found and seized. It is both hunter and hunted. Moreover in a vast number of cases it has to seek its kind, to propagate its species. The movement necessary in this great game of life is million-sided—subtle beyond words—and most animal lives are spent in nothing else. Existence for the individual and the race depends upon success in it. Man plays it also —let us hope that sometimes he plays something else as well. In all cases the chief instruments of the game are the skeletal muscles, those muscles of which the biceps of our arm may stand as type. An old philosophic adage has it that all which mankind can effect is to move things. The dictum illustrates how supremely chief an executant of man's activity his muscles are. And all the things which man can move are moved in the first instance by that prime thing which he can move, his body. And for this his main agents are his skeletal muscles. These execute his movements, but in doing so are but the instruments of his nervous system. Therefore it is in reality the nervous system which is the player of the game. And it is because it is really the nervous system which is the player of the game that man is the most successful creature on earth's surface at the

present epoch, for his is the nervous system which, on the whole, is the most developed, much best adapted to dominate the environment.

To understand a little how the nervous system compasses this end we may turn to examine its performance in some of its simpler governing of the muscles. Its main office is to react to changes in the environment. The animal body is provided with a number of organs specially attuned to react to changes in the environment. These changes, in so far as they excite these organs, are termed stimuli. Thus, it has organs stimulated by the radiant energy of light and heat, others by chemical particles drifting from odorous objects, others mechanically by objects touching the skin, and so on. These organs, specially adapted to environmental stimuli, are called *receptors*. Attached to them are nerves. Through these the excitement set up in the receptor by a stimulus spreads to the general nervous system. Arrived there, two kinds of effect ensue from it—one, a change in nerve-cells innervating muscles and glands, the other, a change in consciousness on the basis of sensation. These two effects are separable. The former, or " reflex " reaction, is not necessarily accompanied by any manifestation of the latter, though it may be so and very often is so. We will confine ourselves to the former, or purely reflex effect, and to its operation on muscle.

The endowment with receptor organs is not equally rich in all parts of the body. It is the external surface of the animal which, as we might expect, has them in richest profusion. And the receptors of the external surface are likewise those most developed, specialised and sensitive. This also we might expect; for it is the external surface that for countless ages has felt the influences of the illimitable outside world playing on it. Through refinement of the receptors of its outer surface the animal has been rendered sensitive in many cases to stimuli delivered even by the remotest stars.

It is a feature of receptors generally that they react most to their agent when the intensity of that agent changes, and the more so the more abrupt the change. It is, therefore, changes in the outside world that operate especially as stimuli; though of course only changes which have relation to the animal in question. If we regard the mutual relation between the animal and the world at any moment as an equilibrium, then we can say that any change in the world which changes that relation disturbs the equilibrium.

Take the instance of a child asleep. A thousand agencies of the external world are playing upon it. Upon its skin, for instance, there is the pressure of the child's own weight against the receptors, and there is the pressure of the clothes which cover it; yet it lies restful. Suppose we touch its foot. That is a change in the external world in relation to the child. The familiar fact is that the foot is drawn up out of harm's way as it were. The change has acted upon the child as a stimulus to some receptors of the skin. It may be quite unconscious of the touch for its sleep may be deep. Yet the reflex action

has occurred, and has done the appropriate thing. A candle may be brought into the room and its light reach the face of the child. That is a change in the outside world in relation to the child. The familiar fact is that the child's head turns from the light. It *sees* no light, but reflex action averts its face. Or, turning to other forms of life, take a fish quiet in its aquarium. A worm is dropped into the water, and the disturbance of the water reaches the surface of the fish. The fish turns and seizes the morsel. Such a reaction on the part of such a creature is probably wholly reflex.

The point for us here is, that the changes in the outside world which act as stimuli bring about appropriate readjustments of the body to the external world, and that in doing so the instruments of readjustment are the skeletal muscles, worked by the nervous system. The child's heart goes on beating, whether the child's foot lies quiet or is moved, whether its face lies this way or lies that; the fish's heart whether the animal's skin was stimulated by fresh commotion in the water, or was not. But with the skeletal muscles it was different. Flexor muscles of the leg, that were relaxed, are by the touch to the foot thrown into action; muscles which lay relaxed were, when the light came, caused to contract turning the head away. Muscles of the fish that were inactive were thrown into activity by the new commotion in the water. It is these skeletal muscles, therefore, that the daily thousand changes of the external world so repeatedly and constantly affect in this way or that, and in reflex action it is always the receptors and the nervous system which impel them to react. And the result is to readjust advantageously to the animal its relation to the altering external world. Hence these muscles are called the muscles of *external relation*. So prominent are these muscles in the everyday work of life that they are *the* muscles of ordinary parlance. The man in the street is hardly aware that he has in his body any other muscles. And these muscles are, through the nervous system, driven by the external world. The world outside drives them by acting on the receptors. It is not surprising, therefore, that this little muscle, removed from the body, and therefore separated from the nervous system and all its receptors, remains, although still living and able to contract, as functionally inactive—for contraction is its function and it does not contract—as if it were already dead.

Now this muscle, when in the body, was the servant of a thousand masters. It had to contribute to a thousand acts. In a certain sense, it, like the heart, had to do for them all but one thing, inasmuch as it had to pull the limb in one certain direction. And yet its task is a very varied one. It has to pull the limb sometimes far, sometimes very slightly, or through all intermediate grades. It has to pull it strongly against great resistance, or weakly, and with all intermediate grades of intensity. We may suppose that in the course of evolution it had become adapted to this scope of purpose.

And indeed we find it so. Unlike the heart muscle this muscle when

a strong stimulus is applied contracts strongly, when a weak stimulus, weakly, under a long stimulus, it contracts long, under a brief, briefly. The nervous system, in making use of this muscle, wants of it just such varied action as this—now weak, now strong, now brief, now long, as may be suited to the act required. The little organ is admirably adapted to be the animal's instrument in the world in which it is placed. This muscle has its place in the economy of nature, and into it it fits as a key into the lock for which it has been made. Man's naive view, until somewhat recently, was that the earth and the universe were made to fit him. Was the universe made to suit this little muscle, or was this little muscle made to suit the universe? The problem concerning this muscle and that concerning man are, in so far, the same. Surely our answer is, that the muscle and the rest of the universe fit each other because they have grown up together—because they are part of one great whole; they fit just as a lock and key fit because they compose one thing; and it is pointless to ask whether the lock was made to fit the key or the key the lock.

The office of the nervous system is to co-ordinate the activities of the various organs of the body, so that by harmonious arrangement the power and delicacy of the animal's mechanism may be obtained to the full. When reflex action withdraws the foot of a sleeping child, it is not merely one such muscle as this which moves the limb but many. The limb has many muscles, and even in such a simple act many and many of them are employed.

That the act occurs during sleep shows that consciousness is not its necessary adjunct. A similar act can be similarly evoked in an animal when the brain—the seat of consciousness—has been removed. The brain can be removed under deep narcosis of chloroform without any pain or feeling whatsoever. After that removal the animal is no longer a sentient or conscious thing at all. Then we can study in it the power of reflex action sundered from conscient and sentient life altogether. Then it is that opportunity is given for further reverent analysis of those wonderful and subtle workings of the nervous system which in ourselves are so difficult to unravel for the very reason that their working goes on without appeal to, and often beyond access of, the conscious self.

When analysing the muscular action of even so simple a reflex act as that of drawing up the foot, a fact which early meets the observer is that the nervous system treats whole *groups* of muscles as single mechanisms. In lifting the limb it employs together muscles not only of one joint of the limb, but of all the joints—knee, hip, ankle, etc. It deals with all these muscles as if they were but one single machine. If the movement is forcible, it throws them all into strong contraction; if weak, into weak. In the grading of the reflex action its influence is graded in all these muscles alike. So also the contraction in all of them is timed to begin together, to culminate together, and to desist together. Further, although the movement of this lifting of the limb

is mainly flexion at its joints, the reflex accomplishes along with that some internal rotation of the hip, and some abduction of the thigh. Why it should do so we shall see presently. Suffice us for the present that, besides the flexor muscles, the nervous system brings into play, at the same time and harmoniously with those, two other great groups of muscles, the internal rotators and the abductors. So perfect is its skill in using the muscles as its instruments that it can deal harmoniously and simultaneously with all these individually complex groups of motor organs as though they were but one.

Were we to attempt to produce this movement in the limb experimentally without employing its nervous system, we should have to apply I know not how many stimuli simultaneously to more than half the muscles of the limb. Not only that, but we should have to grade the stimulation of each of these most accurately to a particular strength. We should also have to arrange that not only did each stimulus develop its full strength with the right speed, but that each should maintain it for the appropriate time and desist at the right speed and moment, and with proportioned intensity. Moreover, in the real reflex act the contraction of this or that muscle is now stressed, now subdued, with a delicacy and accuracy baffling all experimental imitation. The co-ordination in even the simple reflex we are considering may be likened to that exhibited by a vast assemblage of instruments in very perfect orchestration directed by a supremely capable conductor.

But it is more subtle and delicate than that, even in the simple reflex we are considering. The co-ordination goes much farther than we have yet assumed. The musculature of the limb is an instance of that kind of musculature which obtains where parts are adapted to move not in one direction only, or one way only, but in many. The limb has to do many different things. It has, according to circumstances, to bend or to straighten, to turn inwards at one time, at another to turn outwards, to move this finger or move that. Its musculature is therefore split up into many different muscles—some doing this, some doing that. Hence it comes that in the limb are muscles which when they contract do with the limb exactly opposite things. Thus we find a set of muscles which bend the knee, and another which straighten the knee; so, similarly, at hip and ankle, at elbow, and shoulder, and wrist. These muscles of opposed action are called antagonists. Now in the flexion reflex—the reflex we are considering—when the reflex bends the knee by causing the flexor muscles to contract, what happens with regard to the muscles which straighten the knee? Do the opponents, the muscles which straighten the knee contract, or does the reflex nervous influence leave these muscles untouched? It used to be taught that the muscles which straighten the knee, the extensor muscles, contract, and by their contraction exert a moderating influence on the muscles which execute the flexion. That was the anatomical speculation de-

duced from simple dissection of the musculature of the dead limb. Experiment with the living limb teaches that nature does not expend her muscular energy in using the power of one muscle to simply curb the power of another. When the knee is bent the reflex act does not hamper the working of the flexor muscles by causing a contraction of the extensors also. Nor does it simply leave the extensors out of account. No ; it causes them to relax and lengthen at the same time as it causes the flexor muscles to contract and shorten. This it does by reflex *inhibition.* And it proportions the grade of this relaxation exactly to the grade of contraction of the opponent muscles.

The inhibition acts, not on the muscle directly, but on the motor nerve-cells innervating the muscle. These nerve-cells are long filaments ; one end of each lies in the muscle the other in the spinal cord. The reflex inhibition is exercised upon them at the end which lies in the spinal cord. In the reflex we are considering, the reflex action besides *exciting* the motor nerve-cells of the three muscle groups—flexors, abductors, and internal rotators—before mentioned, *inhibits* the motor nerve-cells of three muscle groups antagonistic to those, namely the extensors, the abductors, and external rotators. We see, therefore, that in even the simple reflex lifting of the foot, almost every one of the many muscles composing the whole musculature of the limb receives from the nervous system a controlling influence, either of excitation to contract or of inhibition which relaxes contraction. And all this in result of a simple touch of the skin of the foot. The reaction typifies in a simple manner the action of the nervous system to knit the heterogeneous powers of the body together into one harmonious whole.

Thus we see that in these actions when one group of muscles contracts, the group antagonistic to it relaxes. This is a fundamental part of the co-ordination of the act, and its discovery throws a welcome light on the nature of certain maladies. Were the antagonistic group to contract at the same time as the protagonist, the desired movement would not result. The movement which then ensued would depend on which of the two muscle groups were the stronger, the protagonist or the antagonist. The alkaloid strychnine and the poison produced by the bacilli which cause the malady called "lock-jaw" possess the power of destroying reflex inhibition. What the intricate nature of the process of this inhibition is we do not yet know, but it seems to be the exact converse of the process of excitation, whose nature is also unknown. Strychnine and tetanus-toxin change the process of inhibition into its converse namely excitation. If a minute dose of strychnine be administered, the reflex which as we saw causes the limb to bend, now causes the limb to straighten instead. This is because the extensors, when the flexors contract, instead of being relaxed by inhibition are excited to contraction, and being more powerful than the flexors, move the limb in exactly the opposite direction to that in which it should move in this reflex action.

Similarly with the toxin of "lock-jaw." The muscles which close the jaws are much more powerful than those which open them. In the normal act of opening the mouth the relatively feeble opening muscles contract, and the powerful closing muscles are simultaneously relaxed by reflex inhibition. But in an animal or man poisoned with this toxin the normal inhibition of the closing muscles is changed to the exactly opposite process of excitation so that their contraction results. Against the power of these strong closing muscles the contraction of the weak opening muscles can effect little. Each time therefore that the sufferer tries to open his jaws to take food or speak, he clenches his jaws instead of opening them—experiencing a torture which, although unaccompanied by physical pain, is inexpressibly distressing. And the disorder leads to death from inanition.

But to return to the reflex lifting of the leg, whence we set out. It was mentioned that in this reflex the limb was not merely lifted, but was slightly rotated inwards at the hip, and that the thigh was slightly abducted, that is to say, drawn sideways, separating it more from the fellow-limb of the opposite. These accessory movements have a significance coinciding with much other evidence into which we have not time to enter now. They, together with other evidence, show that this lifting of the leg, so easily produced reflexly, is nothing more nor less than the first movement of the taking of a step. In fact, in our rough and imperfect analysis of this little movement, we have been examining part of the great and extraordinarily complex and perfect act which is called walking—or more technically, so as to include the cognate acts of trotting and running—locomotion. And a little reflection will suffice to assure you that included in the action of locomotion is also that of standing. We are apt to forget that the muscles have a static as well as a kinetic action—that they are the instruments of maintaining position, as well as of the execution of movements. Directly we begin to analyse locomotion we see that its basis, as it were, is the position of standing, upon which movements of stepping are, as it were, grafted. Not much is known as yet of how animals and ourselves stand, walk, and run. In these acts, probably, every skeletal muscle in the whole body is concerned. Rheumatism can make us aware of that. A little receptor organ in the ear is a great factor in the whole matter. But of this we may be sure, that foremost in its factors are reflex actions of the limbs. Great economic questions are involved in this unravelling of the act of locomotion—all beasts of draught and burden are chiefly useful to us because they can stand, and walk, and run. We can only employ their powers to full advantage and with due regard to them as they unfold these powers when we shall have learnt something of the way in which these movements are conducted and performed.

The crude and imperfect analysis which I have attempted to outline concerned but *one phase* of the step of a *single* limb. In the complete act the other limbs will at the same time be executing other

phases of the whole cyclic reflex. The neck and trunk are also involved ; so, likewise, the head itself. Our imperfect analysis threw sidelights on the nature of the mischief wrought by strychnine-poisoning and the malady "lock-jaw." Interesting and useful though these sidelights may be, more really interesting and valuable would be any light which such analysis, crude as it is, could throw on that great normal process of everyday health, animal (including human) locomotion. Analysis of the reflex movement in unconscious animals seems at the present time the only way by which such knowledge can be gained.

[C. S. S.]

WEEKLY EVENING MEETING,

Friday, April 26, 1907.

The Right Hon. LORD ALVERSTONE, G.C.M.G. P.C. M.A. D.C.L. LL.D. F.R.S., Vice-President, in the Chair.

JAMES SWINBURNE, Esq., F.R.S. M.Inst.C.E. *M.R.I.*

Incandescent Illuminants.

[ABSTRACT.]

A LITTLE more than twenty years ago, Auer von Welsbach, who was engaged on researches on the rare earths, invented the modern incandescent mantle. His first mantles were made of zirconia and yttrite earth, in the proportion to make a normal zirconate. Shortly afterwards, he found that the best material has a basis of thoria. Pure thoria, which requires care in its preparation, gives very little light, but if a small percentage of a coloured and permanent oxide, such as ceria, is added, it gives good illumination.

There has been much discussion about the theory of the incandescent mantle. It has been generally assumed that the temperature of a Bunsen burner is too low for a mantle to give the light it does by simple radiation, unless it is much hotter than the flame. Unfortunately, the temperature of the flame is generally taken with a thermo-couple, and this gives far too low a reading, as the thermo-couple never reaches the real temperature of the flame. But, admitting that the temperature of the flame is high, it is still urged that the light given by the thoria with a small percentage of ceria is so great that there is something else than mere thermal radiation. It is said that the ceria acts as a catalytic agent, and that it oscillates between two states of oxidation. Ceria does act in somewhat the same way as platinum; for instance, if a ceria mantle is put on a lighted burner, and the burner turned out, and the gas turned on again, the ceria mantle will glow and will finally light the gas. It is odd that this is not brought forward by the advocates of the catalysis theory; but the opponents might urge that zirconia will do the same thing, and the zirconia mantle gives very little light. This does not really dispose of the catalytic theory.

According to the simple radiation theory, the light depends only on the emissivity, or blackness of the mantle, and its temperature. Its temperature must be lower than the flame, as it must be robbing

the flame of the heat it radiates. In order to give the flame every chance of supplying the heat, the threads of the mantle have to be made very fine, so that the flame can rush through the meshes, and the hot gas should be in brisk movement through the interstices of the mantle. By using a special draught arrangement, known as the intensive system, about twice the light per cubic foot of gas can be obtained. In order to get the highest temperature the emissivity should be low, that is to say, the mantle should be very white; but then, though it would get to a high temperature, it would give very little light. On increasing the emissivity the light will first increase, but this means a lower temperature, so that as the emissivity is increased from white to black, the total radiation increases, but as that means a greater abstraction of heat from the flame, the mantle is cooler, and therefore radiates a larger proportion of the energy as heat and a smaller proportion as light, so the mantle gets redder and gives less light. This is just what happens in practice, whether ceria or any other coloured oxide is used.

It has been urged that as pure ceria is white, adding it cannot make the mantle blacker; but ceria is white only when cold. A mantle may look quite white cold, and be darker in colour when hot. Rubens has devised an experiment to show this. The mantle is strongly illuminated by an arc and condenser, and its image is thrown on the screen. It looks quite white, of course. On lighting the gas, the mantle, instead of becoming still brighter, at once becomes dull. Again, alumina, which is white, gives little light. Chromium oxide is so dark that it gives only a dull red glow. But on adding a little chromium oxide to the alumina, a dark red light is first given, because the chromium oxide is too dark; but as soon as it combines with the alumina to make a light pink mantle, a good light is obtained.

The incandescent mantle is now applied not only to the ordinary Bunsen burner, but to an inverted form, which lends itself to decoration, and to the petroleum lamp. It is now also applied to air carrying a little hydrocarbon gas, and this application is said to provide an extraordinarily cheap light, which is especially useful for country houses.

One of the drawbacks of gas, compared with electric lighting, is that merely turning on does not light gas. This difficulty has been largely overcome by the use of the by-pass, but further advances have been made. Welsbach has discovered that an alloy of cerium and iron gives off sparks on being scraped or filed, and a burner has been designed in which the act of turning on the gas scrapes a little wheel of this alloy, causing a spark, which lights the gas. This overcomes the drawback of having a little jet always burning. Another invention allows the gas to be lighted from a main tap. Each burner has an attachment, which lets the gas straight through to the burner when the pressure is on; but on turning the main

supply off, and allowing a little gas to pass at the controlling tap, the attachment to each burner turns off the burner and lights a little pilot jet, which keeps alight until the light is wanted again. On turning on the main tap, the pilot jets light the various burners and go out themselves. By this means burners can be fully lighted up by turning one tap at the door of the room.

The electric incandescent light is undergoing a great change. Carbon is being replaced by metal wires. It has been found possible to make wires of high enough resistance of tungsten, osmium, tantalum, and a few other metals and compounds. The osmium lamp was the first of these, but there was difficulty in making it of high enough resistance. The tantalum lamp is now in great demand. It is made for 100 to 130 volts, and is much more efficient than the carbon lamp. It will not last long on alternating currents, however. The wires of a lamp that have been run for some time on a direct current show a curious notched or crinkled appearance under the microscope. But a wire that has been run on an alternating circuit looks as if the metal had been melted into short cylinders with round ends, and these cylinders had stuck together end to end without their centres being in a line. Sometimes the little cylinders are nearly separated, merely touching at a corner. This action is very extraordinary, and has never been explained. In addition to this, when a lamp breaks down on an alternating circuit, the wire sometimes goes at one point, and sometimes it breaks in several places, and tangles itself up in an extraordinary way ; at other times it breaks up into numerous little pieces, which will be found lying on the inside of the globe. Some of the other lamps show a change under the action of the current, but it is not so marked as in the case of tantalum.

One of the most interesting of the new lamps is the Zircon. It is said to be made of zirconium and tungsten, and lamps of this material have been made for 200 volts, a matter of the greatest importance from a distribution point of view. It is possible that the conductor is really a zirconide of tungsten, and this opens up a new series of compounds. A Zircon lamp for 100 volts has really six separate loops of wire mounted in series inside a bulb. A recent improvement is to provide an extremely light spring for each loop, so as to keep it taut. The lamp can then be used in any position.

Tungsten seems to be the favourite metal, as it gives a very high efficiency. It is probable the lamp of the future will have an efficiency of nearly a candle per watt, and this is promised by the use of tungsten. At the same time it must be admitted that to make a wire with a resistance of 500 ohms small enough to give 20 candles with 20 watts is a triumph of inventive skill.

[J. S.]

[*The Lecture was illustrated by numerous Experiments.*]

ANNUAL MEETING,

Wednesday, May 1, 1907.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1906, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Thirty-six new Members were elected in 1906.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1906.

The Books and Pamphlets presented in 1906 amounted to about 216 volumes, making, with 782 volumes (including Periodicals bound) purchased by the Managers, a total of 998 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
SECRETARY—Sir William Crookes, D.Sc. F.R S.

MANAGERS.

The Right Hon. Lord Alverstone, G.C.M.G. P.C. M.A. LL.D. F.R.S.
Sir Benjamin Baker, K.C.B. K.C.M.G. LL.D. D.Sc. F.R.S.
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WEEKLY EVENING MEETING,

Friday, May 3, 1907.

The Right Hon. Earl Cathcart, D.L. J.P., Manager, in the Chair.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treas.R.I.*

Dexterity and the Bend Sinister.

There are periodical outbreaks of ambidexterity, or rather, I should say, of feverish attempts to persuade us to adopt ambidexterity or to force it upon us. An innate love of symmetry, which Ruskin refers to as one of the essential constituents of beauty and a symbol of abstract justice, a correlative aversion to lop-sidedness which suggests abnormality, the hope of duplicating the special aptitudes to which the right hand has attained, and the ever-recurring craving for some new thing, have, from time to time during the last 2000 years, led to revolts against the existing state of matters and to efforts to establish what has been regarded as a better system of government in manual affairs.

Plato, I believe, included in his idealism the perfect equipoise of the two sides of the body; and, skipping the innumerable eruptions of ambidextral enthusiasm since his day, down to thirty years ago, I may remind you that at that time the late Mr. Charles Reade again stirred up widespread interest in the subject by a series of articles entitled "The Coming Man." In those articles the brilliant novelist denounced dextral pre-eminence as wicked and against nature, the outcome of the foolish practices of credulous mothers, silly nurses and hide-bound schoolmasters on our sacred bodies in defenceless childhood. He ridiculed biological science, especially as represented by my profession, and promised rich rewards to those who would diligently cultivate either-handedness. But somehow in his passionate philanthropy Mr. Charles Reade overstepped the mark, rode rough-shod over facts, and indulged in blatant exaggeration and transparent fallacies; and so his advocacy failed, his campaign proved abortive, and "The Coming Man" did not come. For about a quarter of a century we were left undisturbed in the enjoyment of our lop-sidedness, but some five years ago ambidexterity again popped up, and a new crusade on its behalf is now being carried on under influential auspices. We have now an Ambidextral Culture Society; big books

upon ambidexterity have been published, pamphlets and leaflets dealing with it are being circulated, schools are trying to attract pupils by advertising that they give ambidextral training, of course with unparalleled educational successes ; and in the most renowned of all our schools the thin edge of the wedge has been introduced, for it has been ordained, we are told, that at Eton the boys who for their transgressions are called upon to write lines, are henceforth to do so with the left hand.

In this present movement in favour of ambidexterity I fancy I detect the old taint of faddism. Some of those who promote it are addicted to vegetarianism, hatlessness, or anti-vaccination, and other aberrant forms of belief ; but it must be allowed that beyond that it has the support of a large number of highly educated, intelligent and reasonable people, and of some men of light and leading.

An eminent admiral gives his blessing to ambidexterity, or either-handedness, and evidently thinks it will conduce to our naval supremacy ; an eminent soldier assures us that it is of the utmost value from a military point of view ; an eminent artist opines that it will contribute to personal beauty ; an eminent surgeon testifies that it will increase brain power, and therefore, working and intellectual ability by 25 per cent. ; and an eminent physician predicts that it will to a large extent ward off paralysis.

If this be so, there is no time to lose in amending our ways, and it must certainly be worth while again seriously to reconsider the question of ambidexterity, hackneyed though it has become.

Is the preferential use of the right hand detrimental to our species ? Is it a mere mischievous convention or acquired trick, to be corrected by the "illuminati" of this new century ? Ought we to aim at establishing an equality of power in the two hands ? I think not. I shall endeavour to convince you that ambidexterity is, on the large scale, impossible and undesirable ; that it is by the superior skill of his right hand that man has gotten himself the victory, and that to try to undo his dextral pre-eminence is simply to fly in the face of evolution.

Now right-handedness is, as I dare say you know, a very old story. We can trace it back to remote antiquity in historical records, in pictorial representations, in language, in religious rites, in the weapons of war and the implements of peace. Of modern times I need say nothing, but I may just illustrate the kind of evidence, enormous in amount, that justifies us in inferring the universality of right-handedness among the ancients. Tacitus tells us that the Lingones, a Belgian tribe, sent presents to the Legions, and in accordance with ancient usage—ancient at that time—*vetere instituto*, gave as the emblem of friendship two right hands clasped together. Throughout the whole range of Roman and Greek art we see that the right hand invariably took the leading part, just as it does with us to-day.

In perhaps the best known of Greek sculptures, the Discobolus of Myron, from the Palazzo Lancelotti at Rome—an "instantaneous photograph in marble" it has been called—representing a single moment in the course of an action, we see the throwing of the quoit from the right hand and the position the body was bound to assume in gathering impetus for its discharge. That is a sculpture of the best period of Greek art—the epoch of Phidias and Praxiteles—and innumerable specimens of the same period and kind exist, all showing right-handedness, not one, as far as I am aware, left-handedness. And surely the pronounced right-handedness of the Greeks at that era did them no harm, for it may be questioned, as Dr. Francis Galton has said, whether human nature has ever, physically or intellectually, reached a higher pitch of perfection than it then did, or is, indeed, capable of doing so. But not merely at its

FIG. 1. BRONZE FOUNDRY. (From a bronze in Berlin.)

zenith, but in its formative stages, is right-handedness declared in Greek art. Bronze-making was practised by the Greeks as early as 600 B.C., and here we have (Fig. 1), from a vase at Berlin, a representation of a bronze foundry, where we note the raking of the furnace with the right hand foremost on the rake, just as a right-handed metal worker holds it to-day, the use of the hammer and also of the spear in the right hand. Of about the same date we have (Fig. 2) a singularly uncouth and barbarous slab—Perseus slaying Medusa—from one of the Doric temples at Silenus, in Sicily, leaving no doubt as to the preferential use of the right hand; and from the very beginning of Greek art, at a period probably contemporaneous with Troy, about 2000 B.C., we derive this ivory carving (Fig. 3) from Enkomi in Cyprus, in which the bow is being handled just as our

right-handed toxophilites do it to-day, and in which the wielding of the axe by the right hand is also seen.

FIG. 4.

Throughout another civilisation as wonderful if not as advanced as that of Greece—that of Assyria, with its mighty and luxurious cities and splendid canals, its acquaintance with transparent glass and the magnifying lens, its manufactures, its art, which for grace, correctness and delicacy of execution excels everything known in Asiatic art — throughout the Assyrian civilisation from first to last we have uninterrupted right-handedness.

Here we have (Fig. 4) Ashur Bani Pal, about 650 B.C.—a sculpture vindicating the use of the left hand for the bow, the right for the arrow, the left for the shield, and the right for the spear, and the wearing of the sword upon the left thigh to be ready for the right hand.

FIG. 5.

In the next illustration (Fig. 5) the ceremonial use of the right hand by the priests is shown forth. You will notice that the wand is

Fig. 2.

Metope. (From Temple at Selinus.)

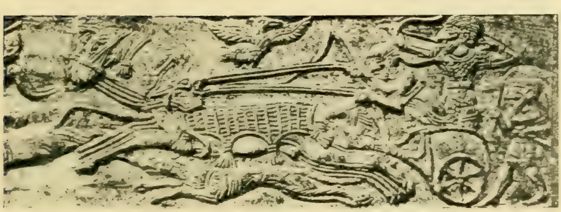

Fig. 3.

Faïence Vases, and Part of Ivory Casket.
(From Cyprus.)

Fig. 6.

Fig. 7.

carried in the right hand, that the sacred vestment is crowned by a right hand, and that in the folded hands the right hand is uppermost.

Much further back, 1500 B.C., we have right-handedness pronounced in the sculpture of the Sun god of Sipara in his shrine (Fig. 6), and further back still, 2000 B.C., we have it again in the giving of the Laws to Kammurah the Sun god (Fig. 7). Referring to Assyrian right-handedness, Dr. Wallis Budge writes to me: "I see no single instance of the use of the left hand." And bear in mind that Dr. Wallis Budge has thousands of tablets and sculptures under his scrutiny at the British Museum.

In the more conventional art of Egypt the occasional use of the left hand is seen, but in an immense majority of instances it

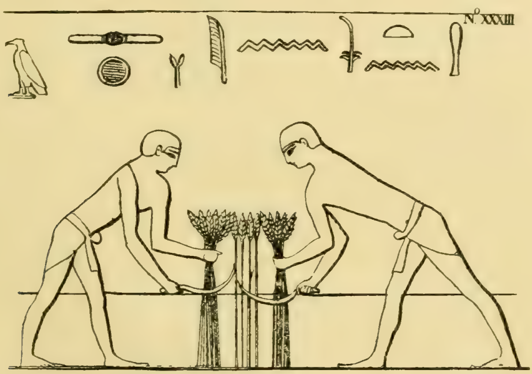

FIG. 8.

is the right hand that is in use. As everybody goes to Egypt nowadays, it is almost unnecessary to illustrate the statement; but I may pass before you two or three of what may be called trade pictures.

1. Reapers at work, 1200 B.C., the sickle in the right hand, the sheaves grasped by the left (Fig. 8).

2. A carpenter at work, from a Tomb at Thebes, 1500 B.C. (Fig. 9).

3. Painters at work, the brush in the right hand, 1500 B.C. (Fig. 10).

But if we go beyond historical records and remains and dip into the obscurities that lie beyond, we still have glimpses of right-handedness amongst our ancestors in the Bronze Age and even in palaeolithic times. We have here probably one of the most ancient examples of an implement expressly adapted for the right hand, in the handle of a bronze sickle found in a lake dwelling at Brienz in Switzerland and described by Dr. Keller (Fig. 11). It is a right-handed implement

made of yew, carefully fashioned so as to adapt itself to the grasp of a very small right hand, and is incapable of use by a left-handed shearer. In the next slide we go back to the Palaeolithic cave-dwellers, and have one of those sketches of animals in a large majority of which the animal is depicted looking to the left, which is suggestive of a right-handed artist (Fig. 12). It is the well-known engraving of the Mammoth found in the cave of La Madeleine. Young children, being right-handed, when set to draw profiles, almost invariably turn them to the left, and this direction of the profile in so many of the cave-drawings—and the same direction is seen in a large majority of the sculptured hieroglyphics of Central America and in the Mexican picture writings—is regarded by Sir Daniel Wilson as a proof that the cave dwellers, and prehistoric men generally were right-handed. "The skilled artist," he says, "can no doubt execute a right or left profile at his will. But an unpremeditated profile drawing, if done by an untaught right-handed draftsman, will almost inevitably be represented looking to the left."

With the object of ascertaining whether any vestige of this primitive tendency to draw the profile to the left is still discernible among our artists, I have had the curiosity to take the bearings of 1062 portraits in the National Portrait Gallery. It is, of course, the endeavour of the professional artist to overcome any such tendency and to place his subject according to artistic considerations—personal traits and characteristics, light distribution, etc.—without regard to any bias of his own hand; but in spite of his endeavours, the old bent displays itself. Of these 1062 portraits 165 are full face, fronting the spectator, 718 are three-quarters right or left, and they are pretty evenly divided, 352 being turned to the right and 366 to the left, an inclination of 2 per cent. to the left; while 159 are profiles, amongst which the inclination to the left is strongly marked. In those 159 profile portraits the face is turned to the right in 52 instances and to the left in 107—that is to say, the left hand profiles are to the right as 2 to 1.

But besides this preferential direction of the profile to the left which Sir Daniel Wilson regards as an unerring test of right-handedness, we have many other evidences of the existence of that habit amongst the Palaeolithic cave-dwellers. In the cave of La Madeleine there was unearthed a perforated antler, on which is scratched a human figure between two horses' heads, and the figure is carrying a baton or stick in its right hand (Fig. 13); while another engraving discovered by M. Massenet, represents an auroch hunt, rudely but clearly enough to satisfy us that the hunter's right arm is thrown back and on the point of hurling a javelin (Fig. 14). Then Palaeolithic flints as a rule appear to be better adapted to the grasp of the right hand, and as regards them Sir John Evans, our highest authority, says: "I think there is some evidence of the flint workers of old having been right-handed, the particular twist in palaeolithic implements,

FIG. 9.

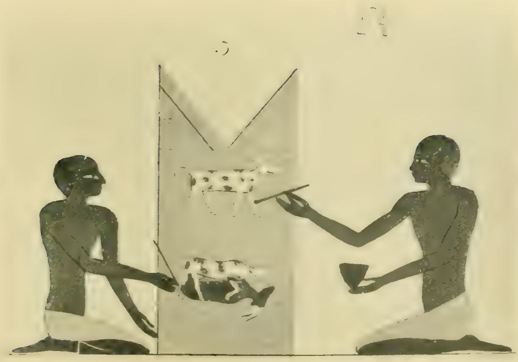

FIG. 10.

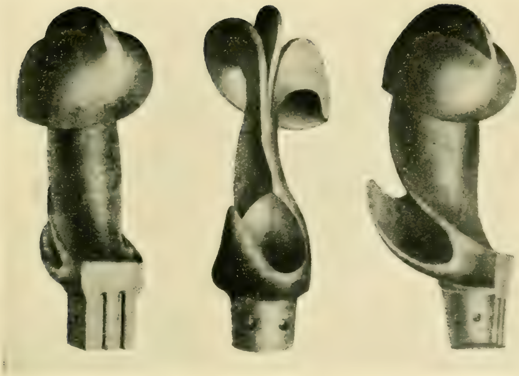

Fig. 11. Head of Bronze Sickle.
(Lake of Brienz.)

Fig. 12. Mammoth Engraved on Ivory.
(Found in Cave of La Madeleine.)

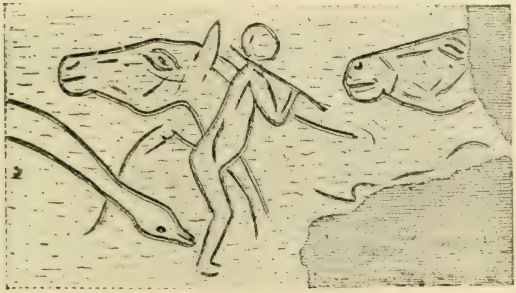

Fig. 13. Perforated Antler.
(Discovered in the Cave of La Madeleine.)

and in some American rifled arrow-heads, being due to the manner of chipping and being most in accordance with their being held in the left hand and chipped by the right." And the view that the cave-dwellers were right-handed is corroborated by the observation

FIG. 14. ENGRAVING OF AUROCH'S HUNT.

of Dr. Lehmann-Nitsche that the clavicle and long bones of the right upper limb in the remains of prehistoric man in Bavaria, are distinctly heavier and more massive than the corresponding bones of the opposite side. Within certain limits, the preponderant use of any part leads to its preponderant development, and so we may infer that these men of Southern Bavaria used their right fore-limbs more than their left.

It appears certain that right-handedness was a characteristic of man at a very early period of his evolution; but it was perhaps in very remote times a less marked characteristic than it has since become, for civilisation has of course in its progress made ever-increasing demand on manual dexterity, and has put a premium on differentiation of function in the two hands.

But can we go beyond man? Have we any indications of right and left handedness amongst the lower animals? On that point opinions are divided. Dr. Ogle, who wrote on Dextral Pre-eminence in 1871, was convinced that monkeys are, as a rule, right-handed. Of 23 monkeys whom he watched he found 20 right-handed and 3 left-handed, and Mr. Osawa of Tokio and several German and American observers who have independently investigated the matter, have supported Dr. Ogle in his view. But it must be confessed that the tokens of right-handedness accepted by these observers were somewhat faint and ambiguous, and one is not therefore surprised to find Professor Cunnyngham, after familiar intercourse for a number of years with the higher apes in the Dublin Zoological Gardens, differing from these authorities. "I have never been able," he says, "to satisfy myself that they show any decided preference for one arm over the other."

Soon after Dr. Ogle wrote, I made a few observations, and was inclined to agree with him, and during the last three months, through the kindness of Mr. Thomson, I have had observations made by the custodian of the monkey house at Regent's Park. He is an adept in monkey manners and free from scientific predilections, and his decided

opinion is that the anthropoid apes exhibit a preferential use of the right hand.

It is to be borne in mind that the upper limbs in the monkey are still largely used for locomotion, in which both sides must equally participate if progress in a straight line is to be accomplished, and are only to a limited degree manipulatory and prehensile in gathering and shelling nuts or pods, in opening shell-fish, in pulling up roots, in picking thorns or burrs from its fur, or in hunting for parasites. And we should not, therefore, expect anything like such a division of labour as in man when they are emancipated from that more servile and automatic office. But wherever the upper limbs have functions assigned to them other than locomotion, divergence will, I fancy, begin ; and I am disposed to believe that the first dawnings of right-handedness are to be recognised not only in the monkey, but much lower down in the animal kingdom.

The *felinidæ*, or cats, use their fore-paws not only in walking and running, but in striking their prey, in performing their ablutions and in many playful acts, and as regards them Mr. Frank Buckland affirmed that the cat kind seize their prey with the right paw and strike it with the left. Sir Joseph Fayrer, who has seen much of lions and tigers in their native haunts, and whom I consulted, is under the impression that in all feline animals the right paw leads. To test the matter I got 28 ladies who are devoted to their cats, to keep them under strict surveillance for six months, and the returns made to me betray, I think, incipient right-handedness. In 7 cats the right paw was invariably used first in washing the face ; in one cat only was the left paw always used first in this operation, and in 20 cats both paws were used indifferently. In 14 cats the right paw was used first and principally in playing with a ball, and in only 4 was the left so used The indication is exceeding trifling, but it is there.

But lower down than the felinidæ, even amongst the birds, differential use of the limbs is alleged. In them there is inversion of the relations of the limbs, for the fore-limbs or wings, while anatomically homologous with the fore-limbs in the mammalia, are physiologically homologous with the hind limbs, for locomotion is their duty, while holding, scraping, striking and other movements of a like nature are performed by the hind limbs. In birds, therefore, we should look for fore-shadowings of lop-sidedness in the legs and claws, and that is where they have been found. Dr. Ogle studied the parrots and found that of 86 of them 63 invariably perched on the right leg and accepted any *bon bouche* offered with the left claw, while 23 invariably perched on the left leg and grasped with the right claw. The present keeper of the parrot house at Regent's Park, who has been making observations for me, entirely agrees with Dr. Ogle and says a very large majority of parrots perch on the right leg and accept offerings with the left.

But to return to man, it is indisputable that right-handedness has

been an attribute of his as far back as we have any knowledge of him, and has been one of the main means of lifting him out of darkness and barbarism into light and civilisation. Whenever he ceased to be solitary and become gregarious, it became essential that in all co-operative work he should preferentially employ one fore-limb. "Curious," exclaims Carlyle, "to consider the institution of the right hand among universal mankind, probably the very oldest institution that exists, indispensable to all human co-operation whatsoever; he that has seen three mowers, one of whom is left-handed, trying to work together, has witnessed the simplest form of an impossibility which but for the distinction of right hand would have pervaded all human things."

And in this sentence of Carlyle lies really the quietus of ambidexterity, for if the habitual priority in the use of the right hand from a remote period has controlled industrial development, regulated all systems of associated manual activity, the form of tools, the construction of machinery, the organisation of sports and games, and even of dress down to hooks and eyes, and buttons, it is obvious that the general adoption of ambidexterity, if that were practicable, would upset our whole social life, introduce hopeless confusion, and multiply accidents of all kinds. And again: if all nations and tribes and races, civilised and savage, have in all time preferentially used the same hand, it is obvious that the origin of the custom cannot be looked for in any acquired habit that can be taken up or laid aside at pleasure. In what other custom, rite, convention, institution, has there been such world-wide consensus and uniformity? We can conceive that every tribe and community, finding the preferential use of one hand convenient and profitable, would adopt one hand and give it established precedence; but we cannot conceive that all tribes and communities should have adopted the same hand; and the fact that they have done so is an irrefragable proof that the source of right-handedness is much deeper than voluntary selection and must be sought in anatomical conformation. Had the selection of one hand as preferential been adopted by any tribe or community as a convention, it is certain that love of change, the spirit of opposition, caprice or pure cussedness would have set the convention at defiance and rendered the use of one hand as common as the use of the other.

And right-handedness is as prevalent to-day as it has ever been. We have no instance of any tribe, community or people that has grown out of it or broken away from it and found salvation in ambidexterity.

It has been alleged, on inadequate evidence, that in some races—as, for instance, the Fijians—left-handedness is exceptionally common; and as regards the inhabitants of the Murray Islands, Dr. McDougall has said: "I think the difference in the manipulative dexterity between the two hands, and also the preference for the use of the right hand, was less marked than in ourselves." But he is careful to add:

"This is no doubt due merely to the fact that they do very little work requiring manual dexterity." But the races in which diminished right-handedness has been reported are all backward, non-industrial races, and it is impossible to point to any civilised race manifesting any degree of either-handedness.

But although this is impossible it is attempted, and I cannot better exemplify the reckless way in which ambidexterity is sometimes bolstered up by its perfervid adherents than by exposing an attempt to establish an instance of the kind. In an authoritative work on ambidexterity published recently we are told that the Japanese are by law and practice ambidextrous. There is a well-deserved encomium on the achievements of that remarkable race in science, art, industry, military and naval prowess; and then we are told that their pre-eminence in these is "the inseparable concomitant"—these are the words—"of their ambidextral culture."

Now I knew from my own observations that that statement was incorrect, but I thought it well to be on sure ground, and so I submitted it to the Japanese Ambassador, and Baron Komura has been good enough to authorise me to say emphatically, "There is no foundation for the statement that the Japanese are ambidextrous." My friend, Mr. John Dixon, to whom I also appealed, who has lived long in the country and is an expert in Japanese art, characterises the statement as preposterous. "Japan's greatest wood-carver," he says, "Hidari Jingors (*circa* 1630) was left-handed, and *Hidari*, which means left-handed, was added to his name because of this personal peculiarity, but I have hundreds of photographs taken by myself showing the Japanese engaged in all kinds of work and play, and proving conclusively that they are a right-handed people."

We have no ambidextral race and no left-handed race, but in every right-handed race we have a certain number of left-handed individuals, and in the hope of ascertaining the proportion of these in this country and of throwing light upon the subject of right-handedness generally, I some years ago circulated a leaflet containing a series of questions relating to various voluntary muscular movements, which through the kindness of friends were placed in the hands of adult men and women, all of the educated and intelligent class, who were requested after careful observation to answer the questions and return the leaflet to me. The returns were in some degree disappointing, for of 3500 leaflets which I cast upon the waters, I found only 957 after many days, and the explanation of this was that the meaning and purpose of my little catechism was largely misunderstood. The questions, it must be admitted, were liable to misconstruction including as they did—such interrogations as "Can you wink with equal facility with both eyes?" and "Can you wag your ears?" and the consequence was that some of those who received the leaflet regarded it as a practical joke and consigned it to the waste-paper basket, while others, knowing something of the source from which it emanated,

suspected that it was a subtle way of detecting incipient insanity and declined to have anything to do with it. The basis of induction supplied to me was not, therefore, as ample as I had hoped it would be, but it was still sufficiently broad to justify some interesting and suggestive conclusions, one or two of which I shall submit to you this evening.

In the first place, the returns bear on the relative frequency of right and left-handedness, and of ambidexterity in the educated classes in this country, and show that of the 957 persons—regarding whom information was obtained, 881, or 92 per cent., were right-handed, 40, or $4\frac{1}{7}$ per cent., were left-handed, and 36, or 3·76 per cent., were ambidextrous, or stated themselves to be so.

Right and Left Handedness in 957 Persons: 694 Males, 263 Females.

—	Right Handed.	Left Handed.	Ambidextrous.
	Per cent.	Per cent.	Per cent.
Males	635 = 91·49	29 = 4·17	30 = 4·32
Females	246 = 93·57	11 = 4·1	6 = 2·8
Both Sexes ..	881 = 92·05	40 = 4·17	36 = 3·76

These figures, I have no doubt, place the proportion of the left-handed and ambidextrous much too high. I found that my distributors, in circulating my papers, naturally thought more of their interesting left-handed acquaintances than of the common-place right-handed ones, and that left-handed persons, regarding themselves as unique and being generally rather proud of their eccentricity, were more ready to answer my questions than their less distinguished right-handed neighbours. As a control observation I got Dr. Charles Mayhew to examine some of the prisoners in Pentonville Prison—not, certainly, a selected group with reference to this inquiry—and he found that of 975 prisoners just 24, or about $2\frac{1}{2}$ per cent., were left-handed, no ambidexterity being noted, although one might have thought that a convenient accomplishment for the pick-pockets; while of 60 officers examined there were 2 left-handed men. Dr. Ogle discovered left-handedness in $4\frac{1}{2}$ per cent. of the individuals whom he examined, and Dr. Brinton has estimated that amongst educated Americans the proportion of the left-handed is from 2 to 4 per cent. I am inclined to believe that the left-handed and ambidextrous together do not exceed 4 per cent. of the population of this country.

Under ambidexterity in my table have been included all those who declared themselves to be ambidextrous, although only in 20 cases out of the 36 was complete equality claimed—probably without justification—for both hands. In the 16 other cases, while ambidexterity was recorded, there were admitted disparities in the

performances of the two hands. In one case, for instance, the return was: "I am ambidextrous, but more so on the right side." In another: "I am ambidextrous but can only play golf with a right-handed club." In another: I am ambidextrous but in cutting or using scissors preferentially employ the left hand." In still another: "I am ambidextrous and can operate with either hand, but would have been left-handed if not corrected in infancy."

These cases are really instances of natural left-handedness modified by right-handed education, or of mixed right and left-handedness. A few movements in each of them were executed with equal facility and precision by both hands, but all other movements are relegated to one hand or the other, and the preferential use of one hand is not difficult to trace.

In several of the 20 cases in which ambidexterity was claimed without qualification, there were indications under other questions, such as those relating to movements in the legs and face, that a one-sided tendency existed, and all my inquiries lead me to doubt whether

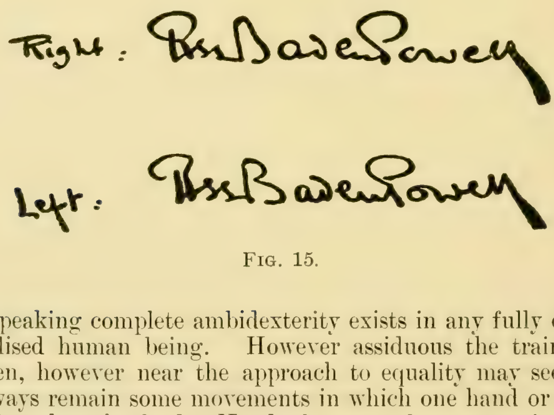

Fig. 15.

strictly speaking complete ambidexterity exists in any fully developed and civilised human being. However assiduous the training may have been, however near the approach to equality may seem to be, there always remain some movements in which one hand or the other habitually takes the lead. No doubt very close approximations to complete ambidexterity occur, and a classical instance is that of Major-General Baden Powell, who, it is said, is accustomed to use both hands interchangeably, to mount equally well on either side of his horse, to employ sword, pistol and lance equally well with both hands, and to shoot off the left shoulder as rapidly and accurately as from the right, and a specimen of whose right and left handwriting I show you (Fig. 15). But this writing, and indeed all the writing that is exhibited as ambidextral is nothing of the kind, for the muscular movements and nerve processes involved in writing with the right hand from left to right with the little finger first, are entirely different from those involved in writing with the left hand also from left to right with the thumb first. The only genuine ambidextral writing would

be the Coustrophedon, or ploughing writing, in which each hand moves backwards and forwards on the paper.

I suppose there is no body of men who have more diligently cultivated ambidexterity than the surgeons, for in some of their work, especially that of the ophthalmic surgeon, it would apparently be of the highest utility to be able to use either hand, and many of them have attained to remarkable proficiency in the ambidextral use of surgical instruments; but very few, if any of them, I believe, pretend to or practise ambidexterity outside the operating theatre or hospital ward. On this subject Lord Lister writes to me: "As a student aiming at surgery and much admiring Liston, I followed his precept, and the way in which I endeavoured to cultivate ambidexterity was by holding the knife in my left hand and the fork in my right when cutting up meat, etc. on a plate, and I remember being struck with the fact that the right hand was at least as awkward in the use of the fork as the left was in the use of the knife."

There is even a suspicion that ambidexterity may sometimes be a drawback to a surgeon. That gifted surgeon, the late Mr. John Duncan, of Edinburgh, wrote to me: "The only surgeon I know or have known to be ambidextrous is G——. He is so in all manual actions, so far as surgery is concerned, and it is, I think rather an embarrassment to him than otherwise, as he seems always uncertain which hand he had better use."

That is a pregnant remark, for the result of general and systematic ambidextral training must, I believe, be to reduce the person submitted to it to a state of wobble.

The mistake in ambidextral philosophy is in imagining that the left hand has been reduced to slavery, or at least to servile conditions of labour. But that is, I will not say a terminological inexactitude, but incorrect. It is not really inferiority and superiority we have to deal with as much as difference. To speak of the left hand—owing, no doubt, largely to religious metaphors embalmed in language—as the dishonoured hand, the unlucky hand, the crippled hand, the supplementary, *gauche*, uncouth, clumsy hand, is all wrong. Its role is in some respects humbler than that of the right hand, but it is not less essential to complete manual efficiency. "They also serve who only stand and wait," and the left hand contributes its fair share to human achievement. If the right hand holds the pen, the left steadies the paper; if the right hand twangs the string, the left grasps the bow and props the arrow; if the right wields the cue, the left provides the bridge; if the right drives the plane, the left guides it and sweeps away the shavings; if the right sways the bow of the violin, the left supports it and fingers the strings. As our poet has it:

> "Of all the things in this offensive world,
> So full of flaws, inversions and caprices,
> There's nought so truly awkward and ridiculous
> As a left-handed fiddler."

What a Walpurgis night we should have with an ambidextrous orchestra and a hundred ambidextrous couples waltzing on the floor!

What is commonly known as right-handedness includes movements of the shoulder, elbow, wrist, fingers and thumb, and the degree in which strength and delicacy of movement in each of these predominates on the right side over the strength and delicacy of movements of similar movements on the left side varies considerably. It would take me too long to enter on these distinctions, but I may tell you generally that right-sidedness is always most marked in the most voluntary muscles, and least so in those which combine most automatic with most voluntary action.

KICKING POWER IN LEGS IN 957 PERSONS: 694 MALES, 263 FEMALES.

—	Greater in Right Leg.	Greater in Left Leg.	Equal in Both Legs.
	Per cent.	Per cent.	Per cent.
Males	575 = 82·85	69 = 9·94	50 = 7·2
Females	207 = 78·7	19 = 7·2	37 = 10·26
Both Sexes ..	782 = 81·71	88 = 9·19	87 = 9·09

In illustration of this, let me refer to the legs. Right-sidedness in them, as displayed in kicking, is strongly marked in both sexes, but less strongly marked than in the upper limb—the percentage is 82 against 91; and I need scarcely remind you that the automatic work of the legs in walking places them on a much lower voluntary level than the arms. You perceive their reduced voluntary position at once if you turn to the toes and compare them with the fingers. How wretchedly limited are their voluntary performances beside those of the fingers, and yet in them a certain degree of right-sidedness is apparent. I daresay some of you remember the artist at Antwerp who, being minus hands and arms, painted with his foot; he held the brush with his right toes; and in those persons whom I have examined who can pick up coins or small articles from the floor by opposition of the big and second toe, all being right-handed persons, could do that better with the toes of the right foot, and most of them could not do it with the toes of the left foot at all.

But while kicking is a voluntary movement in which only one leg can be with safety employed, there are other leg movements essentially bilateral and becoming automatic, in which one-sidedness may still be detected. In walking both legs are equally engaged, but in starting to walk the initiative will naturally be taken by the leading leg.

Foot First Advanced in Walking from Position of Attention in 957 Persons: 694 Males, 263 Females.

—	Right Foot.	Left Foot.	Either Foot.
	Per cent.	Per cent.	Per cent.
Males	294 = 42·36	366 = 53·02	34 = 4·89
Females	193 = 73·38	54 = 20·53	16 = 6·08
Both Sexes ..	487 = 50·88	420 = 43·77	50 = 5·22

But on this point the returns seem equivocal, for while a large majority of the women start off with the right leg, a large majority of the men start with the left; but the meaning of that is that a considerable number of the men who have undergone military training or are accustomed to dance—to waltz at any rate—start with the left foot. The movement is half voluntary, half automatic, and therefore peculiarly amenable to training.

The snapping of the fingers, a movement not subject to training, and which we should expect to be bilaterally equal, for it depends on the opposition of the thumb to the fingers, which is the foundation of all manual operations, shows a decided right side tendency; 6 per cent. cannot snap at all, 29 per cent. can do so with both hands equally well, 5 per cent. do so with greater facility with the left hand, but 58 per cent. with the right.

I am not able to say exactly how things stand at the present moment, but I can testify that a short time ago there was not, amongst the Judges on the English bench or amongst the Bishops in the House of Lords, a single instance of left-handedness or ambidexterity. I have evidence that at the present moment there is not, with one exception, a Royal Academician of London, or Associate of the Royal Academy of London, or a Royal Scottish Academician, or Associate of the Royal Scottish Academy, who is either left-handed or ambidextrous; but, on the other hand, it is certain that these conditions are exceedingly common amongst idiots. At my instigation five-and-twenty years ago, Dr. W. Ireland examined the idiots and imbeciles under his care at the Larbert Institution, and found that, out of a total of 70 boys and 44 girls, 53 boys and 30 girls were right-handed, while 7 boys and 6 girls were left-handed and 10 boys and 8 girls were ambidextrous, using either hand indifferently. The governesses teaching sewing constantly complained that they had difficulty in getting the girls to use one hand in preference to the other. The sinistrous and ambidextrous at Larbert were 27 per cent. of the number examined, whereas the highest percentage ever observed in an investigation amongst sound-minded people was 8 per cent., and that was obviously exceptionally high. Dr. Ireland's inquiry

included idiots and imbeciles of all classes, some of them of a high type, merely weak-minded children; but a stricter analysis was made in a recent inquiry conducted by Dr. F. R. W. Taylor of the Darenth Idiot School. He selected the microcephalic, or small-headed idiots, and he divided these again into two groups—the pathological, in whom the small-headedness was due to disease, and the morphological, in whom it was due to arrest of development. There were 18 microcephalic idiots, 8 pathological, and 10 morphological. Of the 8 pathological, 2 were right-handed, 4 left-handed or ambidextrous, and 2 (I presume from paralysis) had no power in either hand. Of the 10 morphological idiots, 5 were right-handed and 5 were left-handed or ambidextrous. In idiocy from arrest of development, therefore, left-handedness and ambidexterity reach a proportion as high as 50 per cent. The brains of the morphological idiots are, we know, comparatively simple in conformation, symmetrical in their convolutions, and approach to the Simian type, and we have in them the absence to a large degree of that peculiarly human characteristic of right-handedness and an atavistic reversion to what was probably the condition of our very remote animal ancestors.

Do not let me be misunderstood. I do not suggest that left-handedness or ambidexterity is indicative of mental failure or incompatible with the highest intellectual power, or even genius. Natural left-handedness is only a transference of power from one side to the other, and acquired ambidexterity means merely the special training of certain groups of muscles for certain movements. We have had instances of the highest ability and the finest gifts possessed by the left-handed. Leonardo da Vinci, that consummate painter, sculptor, architect, musician, was left-handed—not ambidextrous, as has been affirmed—but if he had attached any special value to this trait or thought it imitable, he would surely have secured its adoption by some of his pupils at Milan, which he did not do. Holbein has been represented as left-handed, but that myth is exploded by the portrait which passed from the Arundel to the Stafford collection, in which he is painted holding the brush in his right hand. But Amico Aspertino was undoubtedly left-handed, and the late Sir Edwin Landseer is said to have painted equally well with both hands; but that accomplishment in his case was not the outcome of any educational discipline, but an inherited proclivity, for his brother Charles was markedly left-handed. The late Louis Haghe, an excellent artist and President of the Society of Painters in Water-colours, was also, I believe, left-handed, but the late Mr. Calder Marshall wrote to me, "I have never known or heard of a left-handed or ambidextrous sculptor, although sculpture is an art in which the equal use of both hands might apparently be an advantage."

The higher branches of art—painting, sculpture, engraving, wood-carving, etc.—are not co-operative work, and there is no reason why the left-handed man might not in his solitary labours attain to the

highest excellence; but the notable facts are that ambidexterity which, if it has any utility, ought to be especially useful to them, has not been seen amongst them and has never been cultivated by them, and that, with a mere fractional deduction, all the art treasures of the world have been the offspring of the right hand.

The theories that have been advanced to account for right-handedness have been numerous, ingenious, and some of them fantastic enough. It has been ascribed to the machinations of the devil, to a loss of balance in the body owing to the removal of a rib from Adam's side while he slept; and Aristotle attributed it to the fact (how he arrived at a knowledge of it he did not say) that the blood that flows to the right side is purer and hotter than that that flows to the left. It would be instructive to traverse all of these theories, for in refuting each of them we should obtain a glimpse of right-handedness from a different point of view. But time permits me to mention one only which has been revived of late, and in a modified version, still to some extent holds the field. That is the theory of Dr. Buchanan that right-handedness is due to certain mechanical advantages possessed by the right side of the body. The right lung is larger and more capacious than the left, and has three lobes instead of two, so that when a deep inspiration, the necessary prelude of any great muscular effort, is taken, a more stable basis of support is given to the right upper limb. But more than that, the weight of the viscera on the right side of the body, where lies the solid mass of the liver, is considerably greater than that of those on the left side, chiefly filled by the hollow viscera, the stomach and the intestines. The difference amounts, according to the late Professor Struthers, to about 15 ounces, and although that estimate is probably too large, especially when the stomach to the left is occupied by a heavy meal, there can be no doubt that the centre of gravity of the body lies to the right of the mesial line. According to Buchanan, this gives a bias to the body like that of bowls that are oblate on the one side and prolate on the other, and in the erect posture exerts an influence on attitude and movements and gives a mechanical advantage to the right arm. Right-handedness, he argued, is not a congenital attribute of man transmitted from parent to offspring, but a personal habit picked up during the individual life, as the result of the greater weight of the right side. All infants, he said, are born ambidextrous and gradually find out for themselves in using their hands that they have more mechanical power on the right side.

It is not difficult nowadays to perceive that Buchanan was wrong. Infants are born ambidextrous in a sense—that is to say, for the first few months of their lives they use both arms together. But unmistakeable right-handedness declares itself about the eighth month, before the infant can have profited by experience or has assumed the erect posture: and it would be as reasonable to maintain that the emergence of its teeth is due to the use of its jaws as it is to

argue that the emergence of its right-handedness is due to the use of the hands.

Then right-handedness is unquestionably congenital and innate, and not acquired in any way, as Buchanan would have it. As I have before said, it is impossible to conceive that a habit of such universality is not bred in the bone. Had it not been vested in human nature, we should have had left-handed tribes and races, and the study of the exceptions to it reveals its hereditary nature. Left-handedness runs in families. Professor Cunnyngham has quoted the case of a sailor who was left-handed, who had a left-handed mother and seven brothers and six sisters all left-handed, and in whose family there were 25 instances in all of this variation, with a certain number of right-handed specimens. The tribe of Benjamin with its 700 chosen men, left-handed, every one of whom could sling stones at a hair's breadth and not miss—and by the way, I see that with consummate audacity these 700 are claimed by a recent ambidextral scribe as having been of that cult, although it is expressly said they were left-handed—the tribe of Benjamin does not, as has been supposed, afford an instance of hereditary left-handedness, for the 700 were chosen men out of 26,000 Benjamites who drew the sword, which gives barely 2·7 per cent. of left-handed men, not above the proportion at the present time ; and, curiously enough, the name of the tribe, Benjamin, means right-handed. But there have been families and clans in which the proportion of left-handed persons has been excessive, and in which the inheritance of that condition from one generation to another has been well marked. In Scotland the left-handed man is said to be "kerry-handed," and the tradition is that the term is derived from a Dabraida king, Krynach Ker, and that members of the Ker family in all its ramifications are very liable to exhibit left-handedness. The Ettrick Shepherd wrote, no doubt verifying a current belief :—

> "But the Kers were aye the deadliest faes,
> That e'er to Englishmen were known,
> For they were all bred left-handed men,
> And fence against them there was none."

Sir Lauder Brunton tells me that left-handedness is still more than usually common in the Kers' country near Galashiels.

But if left-handedness is hereditary, *a fortiori* right-handedness must be so ; and Dr. Buchanan's derivation of it from the inclination given by body weight must be abandoned. But a more serious and, indeed, fatal objection to his hypothesis has arisen. Since he propounded it a considerable number of cases have been observed in which there has been transposition of the viscera without any transposition of manual dexterity. In these curious cases, occasionally encountered, the heart is on the right side, the liver on the left, and all the internal organs are shifted from one side to the other and the centre of gravity is therefore to the left of the mesial line. Now, if Dr. Buchanan was right, the mechanical advantage being on the left

side in these men and women, they ought to have grown up left-handed, and the established fact that they were just as right-handed as ordinarily constructed mortals is a conclusive proof that he was wrong.

But Professor Cunnyngham of Edinburgh, accepting all this, still thinks that there may be some truth in Buchanan's theory. Right-handedness, he says, is a character not acquired independently by each individual and perishing with him without transmission to offspring, but attained in the ordinary course of evolution, by natural selection. A variation which tended to place this attribute of man on the more favourable (that is to say the heavier side) is one which would be strengthened and fostered until in the end it would become a permanent possession of man—a possession which would not even be disturbed by the transference of the bodily conditions which led to its acquisition, to the other side of the body. That is tantamount to saying that the greater weight of the right side of the body originally started right-handedness but has nothing to do with its maintenance as its immediate condition has been stereotyped and does not oscillate from one side to the other when the position of the viscera is reversed.

As I shall presently show, there are grave difficulties in the way of accepting this view—the one fact that in the anthropoid apes the viscera are placed as in man, the centre of gravity being to the right of the mesial line, while in them right-handedness has not been developed to the same degree as in man, is enough to negative it; but even if we did accept it, it would not finally solve the problem of right-handedness, but only postpone the solution, or carry it a step further back, for if we admit that right-handedness took origin in bodily preponderance on the right side, we must next ask how did this bodily one-sidedness arise? How comes it that the heart is on the left side and the liver on the right in an enormous majority of human beings, and that in a few exceptional cases, analogous to but by no means coincident with cases of left-handness, they are transposed?

It is not in the configuration of the body, nor in any individual functional habit dependent thereon that the cause of right-handedness is to be sought, but in the structure and organisation of the brain that initiates, directs and controls all voluntary movements. The brain has two hemispheres, presiding over the two halves of the body, and its action is crossed, the right hemisphere presiding over the left half of the body and the left hemisphere over the right. It has long been clear to investigators that the functional differences in the two hands must be in some way connected with differences in the two hemispheres, and the first difference that suggested itself was one of size. It was thought that probably the left hemisphere was larger and heavier than its companion on the right, and so supplied more energy and therefore determined the superiority of the right arm and hand. Measurements for a time seemed to corroborate this supposition. Dr. Boyd weighed the hemispheres separately in 200 patients dying in the Marylebone Infirmary, and found that almost invariably the weight

of the left hemisphere exceeded that of the right by at least the eighth of an ounce; and Broca also in 40 cases found the left hemisphere the heavier. But then came other observers—Professor Wagner and Dr. Thurnam—who found the right hemisphere the heavier; and it is to be noted that the excess claimed for the left by Dr. Boyd is trifling—an eighth of an ounce, or 60 grains—and might well depend upon methods of examination.

I have weighed upwards of 2000 human brains, but I submit to you the figures relating to only 400, as in that last series the weighings were conducted with the most scrupulous care and after the hemispheres had been stripped of their membranes (Fig. 16). You will observe that in both sexes the right hemisphere is slightly the heavier,

Weight of Cerebral Hemispheres in 400 Cases.

No. of Cases.		Whole Brain.	Right Hemisphere.	Left Hemisphere.
		grammes.	grammes.	grammes.
Male	244	1334·7	580·7	577·0
Female	156	1198·5	521·1	519·0
Total	400	1281·6	557·4	554·4

Weight of Lobes of Cerebral Hemispheres in 60 Cases.

Lobe.	Right Hemisphere.	Left Hemisphere.
	grammes.	grammes.
Frontal Lobe	232·0	225·8
Parietal Lobe	124·3	130·8
Temporo Sphoidal Lobe	123·5	125·6
Occipital Lobe	67·5	63·9

Fig. 16.

but the difference is negligible, being only about 3 grammes or 45 grains; and I feel sure that there is no difference in the weight of the hemispheres corresponding with right and left-handedness.

In 60 brains I weighed the different lobes of which the hemispheres are composed. In the parietal lobe there is a very appreciable difference in favour of the left side, but considering the difficulty of dividing the lobes, I am not inclined to attach much importance to that.

Dr. Charlton Bastian has affirmed that the specific gravity of the grey matter, or the active constituent of the brain, is higher in the left than in the right hemisphere. The specific gravity varies greatly in different regions of the brain and in different pathological conditions,

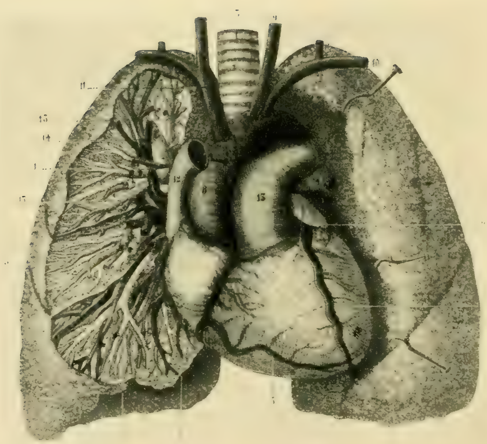

FIG. 17.

but in the healthy human brain I have found it almost exactly alike in the two hemispheres, 1036 in the frontal, 1038 in the parietal, 1040 in the occipital and 1038 in the tempero-sphenoidal lobes.

The next cause of right-handedness brought forward when it was realised that the greater weight of the left hemisphere of the brain and its higher specific gravity would not do, was vascular supply. The left hemisphere, it was alleged, has a better blood supply than the right. It is, therefore, better nourished, is functionally more active, and therefore energises the right arm and hand more copiously than the right hemisphere does the left. Of the two great arteries that supply the fore part of the brain—the carotids—that on the left side rises directly from the arch of the aorta, while that on the right branches off the innominate artery, through which the blood for it and the subclavian artery has been carried (Fig. 17). The jet from the left ventricle of the heart to the brain is, therefore, more direct and less deflected than on the right side; and it has been inferred, therefore, that the left hemisphere is better irrigated with arterial blood than the right; but the important point is the calibre of the arteries that enter the skull, and as regards that Professor Cunnyngham has found that moulds taken of the carotid canal through which the internal carotid enters gave 583½ square millimetres on the left and 583 on the right side. Dr. Sidney Martin and I found that rings cut from the internal carotid arteries within the skull gave in adult males an average diameter of 2·8 mm. on the right and 2·75 mm. on the left side, while the vertebral arteries supplying the hind brain gave an average diameter of 2·2 mm. on the right and 2·75 on the left side. In view of these facts and of the beautiful arrangement for the equalisation of the cerebral circulation, called the Circle of Willis, it is clear the arterial blood supply to the left hemisphere is not ampler than that to the right, and that right and left-handedness cannot be connected with cerebral circulation, especially as the vessels exist for the brain and not the brain for the vessels.

But apart from their weight and blood supply, there is a feature, and a prominent feature, in which the two hemispheres of the brain differ from each other, and that is their convolutional development, and through that we approach nearer to the fountain-head of dextral and sinistral differentiation. The hemispheres in the higher animals are folded or puckered, as it were, into ridges with hollows between them, and thus the superficies, affording accommodation for the cortical layer, grey matter or active substance is extended according to the degree to which the plication is carried. In humble creatures like the rabbit (Fig. 18), in which a little intellect goes a long way, the surface of the hemispheres is smooth without foldings, and then, speaking generally, as we ascend in the scale of intelligence through the animal kingdom, the number and complexity of the convolutions increases. It is in civilised man that the cerebral convolutions reach their richest development.

I shall pass rapidly before you photographs of the brains of a few of the primates, showing progressive convolutional elaboration.

In the Ouistiti, *Jacchus vulgaris*, or Marmoset monkey, an attractive but weak-minded member of the monkey tribe, the hemispheres are almost as smooth as in the rabbit, and show just two rudimentary sulci or folds (Fig. 19).

In the Pincho monkey, or *Midas oedipus*, a South American species, in intelligence about the Marmoset level, we have deepening of these folds and the appearance of two new ones (Fig. 20).

In the Moloch monkey, the *Callithrix Moloch*, a stage higher in intelligence, we have these folds repeated and extended and supplemented by others of a very distinctive character (Fig. 21).

In the Sapajou, another South American monkey (Fig. 22), we have still further convolutional complexity, and I would ask you particularly to note that in these lower forms—in this one, the Sapajou, and in this, the Mangabey—*Cercopithecus Æthiops* (Fig. 23), an African species, there is almost perfect symmetry in the two hemispheres as regards their foldings, a symmetry which becomes less and less marked as we ascend the scale.

There is still a good deal of symmetry in the hemispheres of the Chimpanzee brain (Fig. 24), though less than in the Mangabeys, and as regards convolutional complexity you will have no difficulty in perceiving that that is really much more intricate and advanced in the Chimpanzee than in the brain of the human microcephalic idiot, a representation of which is next thrown on the screen (Fig. 25), and in that idiot brain you will notice another striking feature in which it resembles that of the Chimpanzee, and that is the exposure of the cerebellum when the brain is looked at from above.

Nothing of that kind is seen in even a low type of the normal human brain like that of the Hottentot (Fig. 26), in which pray observe that the convolutions are larger and more regular and symmetrical than in the brain of an European which I next show you (Fig. 27). This is an average European human brain, not that of a highly intellectual person, in which the convolutions would be smaller and more numerous, but it illustrates clearly the multiplication of ridges and hollows in the brain of the civilised human being and the divergence in the form, twistings and arrangements of the convolutions in the two hemispheres. Rigid symmetry has disappeared and we have reached a condition of right and left-brainedness.

But throughout this symmetry and variability in the brain—and no two human brains were ever exactly alike in their convolutional arrangement—we can invariably recognise one plan of construction and identify all the main convolutions, however they may contort themselves and put forth secondary gyri. And more than that, we can now, thanks to the brilliant discoveries of Fritch aud Hitzig, Ferrier, Victor Horsley, Sherington and others, assign specific functions to certain of the convolutions. We know that there are

Fig. 18.

Fig. 19.

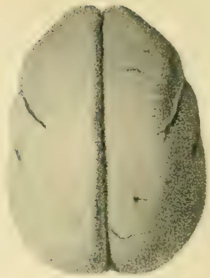

Fig. 20.

Fig. 21.

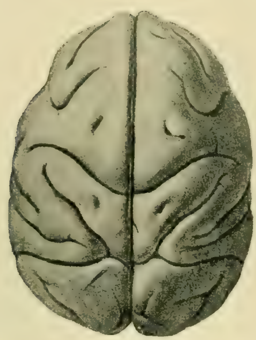

Fig. 22.

Fig 3.

Fig. 24.

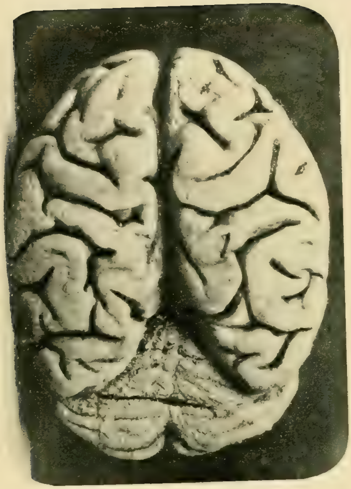

Fig. 25.

Fig. 26.

Fig. 27.

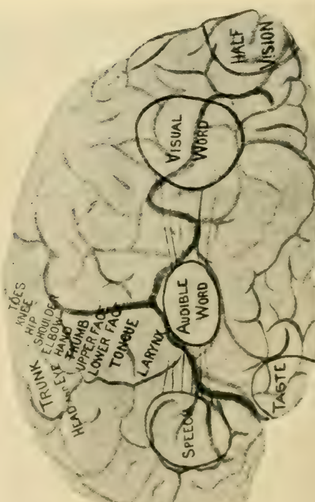

Fig. 28.

centres in the brain for vision, hearing, taste, and touch, and that there is a great motor area in the middle of the brain in which are represented all the voluntary movements of the body. And we know further—and that is the significant point for us—that in that motor area of the cerebral cortex there is a region distinctly differentiated into centres, each of which stands in connection with a particular group of muscles, or with the movements produced by these muscles in the upper limb (Fig. 28). We have here the centres for movements of the shoulder, elbow, hand, thumb and fingers, centres stimulation of which by electricity in the exposed brain of the monkey or by disease in man, produces movements in the muscles subtending them, and destruction of which, experimentally in animals or by disease in man is followed by paralysis of these muscles.

Now, study and experimental interrogation of these hand and arm centres in the brain does not afford any clue to the comprehension of right and left-handedness. Each side reacts apparently in the same manner to the stimulation of its centre in the opposite hemisphere of the brain, and we should not expect to distinguish fine differences. But there is another centre in the brain, the study of which has thrown a flood of light upon the subject in which we are interested, and that is the speech centre.

I cannot enter on the psychology and physiology of speech, a vast and controversial topic ; but there is one point on which we are all agreed, and that is, that there is an emissive centre for speech in the third frontal or Broca's convolution, as it is called. We need not localise speech in any such small part of the brain, but of this we are certain : that damage in this part is destructive of speech, but in a different way in the two hemisphere. Damage to Broca's convolution, as by a clot of blood or an embolon, in the left hemisphere deprives the right-handed man of speech but leaves the left-handed man with speech unimpaired ; while damage to Broca's convolution in the right hemisphere deprives the left-handed man of speech and leaves the right-handed man with speech intact.

The evidence that has been accumulated makes it certain that we have aphasia and paralysis of the right hand when Broca's convolution is damaged in the left hemisphere of a right-handed man, in whom damage of the same convolution in the right hemisphere involves paralysis of the left hand but not loss of speech, and that in the left-handed man the exact opposite of all this holds good.

Here then we have conclusive proof that the old opinions, that the action of both hemispheres is required in all mental operations, and that either half of the brain indifferently can act alone, must be discarded. The two halves are not double in function in the sense that both are required for speech, since a right-handed man can speak perfectly well when the right half of his brain is damaged ; nor are they independent in function in the sense that the two halves

are such exact duplicates that either of them will do for speech, since extensive damage in the left hemisphere destroys speech altogether.

Confining our attention now to right-handed human beings, we find that the anatomical substrata for the nervous processes, animating and regulating the highly special movements of the articulatory muscles in voluntary speech, are localised in the left hemisphere. How came they there? Bear in mind that the muscles engaged in speech, all except those of the tongue and lips, are bilaterally co-ordinated and act simultaneously on the two sides. You cannot breathe with one lung, leaving the other at rest; you cannot throw one vocal cord into vibration without at the same time vibrating the other; and it is impossible, therefore, that the preferential use of the left hemisphere in speech can have been induced by any educational efforts or lop-sided use of the vocal apparatus. Here you have one-sidedness in the brain, assuredly not due to use and wont or to any acquired habit or mechanical advantage. But one-sidedness, our ambidextral friends tell us, is disastrous in its consequences, and they must of course desire to correct it and restore symmetry to the distorted brain—thus, in their familiar formula, "doubling our brain-power"—think of that!—doubling the flow of speech in these loquacious times! Well, I should be glad to know how they propose to set about it, by what ingenious exercises they will confer upon the right hemisphere a power of voluntary speech equivalent to that possessed by the left.

But the hand and arm centres are intimately linked with the speech centres in the brain; they lie close together and are much associated in action. In disease they rise and fall together; and as it is established that there is a preferential use of the left hemisphere in voluntary speech, is it only logical to infer that the preferential use of the right hand and arm in voluntary movements is due also to the leading part taken by that hemisphere?

No one will have the temerity to suggest that it is artificially acquired right-handedness that has dragged the voluntary speech centre to the left side. If there has been any dragging, it is speech that has dragged right-handedness after it, for speech begins before and is generally in advance of manual dexterity, and it is only rational to ascribe right-handedness and the emission of voluntary speech by the left hemisphere to a common cause.

That common cause is the constitution of the brain, a constitution which has differentiated the functional activities of its two halves, and as the greatest living pioneer in neurology, Dr. Hughlings Jackson, has maintained, made the left hemisphere more voluntary and the right hemisphere more automatic. The right-handed man who is aphasic, is paralysed on the right side, and has lost speech, but not altogether. What sort of speech is it he has lost? The voluntary. He cannot propositionise, and that is speech, for a mere succession of words embodying no meaning is not speech but

jargon, and it is only when verbal utterances have a proper inter-relation and convey a meaning, that speech is attained. But the asphasic man although speechless is not wordless, for he understands what is said to him and can therefore receive a proposition although he cannot express it; he can recognise an object although he cannot name it; he can utter interjections when alarmed or moved, and when he is vexed he can swear!

But the receiving of a proposition is automatic; he cannot help it; recognition does not involve the voluntary revival of words, but of visual impressions, and exclamations and oaths are for the most part involuntary and automatic explosive discharges.

What the common aphasic has lost is the voluntary use of words, not their automatic revival, and an analysis of his condition makes it clear that the left half of the brain is for the more voluntary reproduction of movements, and the right half for their more automatic reproduction. Both halves are alike in so far as each contains processes for words; but they are unlike in that the left only is for the use of words in voluntary speech, and the right for other processes in which words serve.

As Hughlings Jackson has again and again declared—and this applies to hand movements as well as to those of speech—the left hemisphere is the more voluntary and the right the more automatic. In all voluntary operations the left hemisphere leads.

The left may be described as the more intellectual, the right as the more emotional hemisphere. As in most powerful emotions there is bilateral expression, it is difficult to get proof of this in movements; but I may point out that in winking, which is generally an emotional expression dependent on a centre close to the thumb-centre in the brain, there is a decided preferential performance on the left side. Nearly 4 per cent. have lost the art of winking, 38 per cent. retain it equally on both sides, but about 35 per cent. can wink only with the left eye, while 23 per cent. can wink only with the right.

WINKING POWER IN 957 PERSONS: 694 MALES, 263 FEMALES.

—	Greater in Right Eye.	Greater in Left Eye.	Equal in Both Eyes.	Cannot Wink.
	Per cent.	Per cent.	Per cent.	Per cent.
Males ..	144 = 20·74	253 = 36·25	286 = 41·21	11 = 1·58
Females ..	80 = 30·41	80 = 30·41	79 = 30·03	24 = 9·13
Both Sexes	224 = 23·45	333 = 34·79	365 = 38·14	35 = 3·65

The essential point to bear in mind is that the left is the leading hemisphere. Goodsir, the greatest anatomist of the last century, taught that the brain is composed of two symmetrical elements: symmetrical apart unsymmetrical in combination screws which inter-

twine, spirals which interlace. Interlacing spirals are seen in the heart. The muscular structure of the heart, an asymmetrical organ, is still in need of elucidation, but there can be no question that there is at its apex a peculiar spiral concentration known as the vortex, or whorl, produced by the twisting and interlocking of the external fibres with those of the interior (Fig. 29). But if you interlace two spirals one must lead; they cannot be co-terminous; and so if the cerebral hemispheres are interlaced, and we can see their fibres crossing and recrossing, one of them must lead. If you interlace the fingers, one thumb must be uppermost, and in a great majority of persons it is the right thumb that is so. Everyone has a fixed habit in this simple matter from which it is uncomfortable to depart.

It cannot now be doubted that right and left handedness are dependent on cerebral organisation and on nothing else. But if we go a step further and inquire how this cerebral organisation came about, we are face to face with the inexplicable. We do not know why the two hemispheres should have become unsymmetrical in their convolutional arrangement, or in their pathways of intercommunication, why one should have become more voluntary, the other more automatic; why in an enormous majority of persons the left hemisphere should lead the right; why in a few persons the right should lead the left.

We can only fall back on cosmic principles and recognise a great but obscure law regulating dextral and sinistral development throughout the organic world. We find ourselves amongst those residual phenomena that are not yet explicable in terms of chemistry or physics, pointing to a directive force which enters upon the scene with life itself, and which, while in no way violating the laws of the kinetics of atoms, determines the course of their operation within the living being. We find ourselves in the presence of a guiding principle or power that is above and beyond the symmetric forces of inorganic nature.

The asymmetry, or the one-sidedness of the brain, is a mystery, but it is only one of a long series of mysteries of the same kind, and is no more mysterious than the almost invariable position of the heart on the left side and its occasional transposition to the right of the median line. Indeed, when we come to look into it, it would seem that all organic forms have been cast in an asymmetric mould, their tissues being developed, from inherited asymmetrical beginnings in the ovum or seed, or obtained by fission.

I should like to trace back asymmetry through the organic world, but time permits me to cite only one or two examples, and I cannot select a more familiar one than that of the flat fishes—soles, plaice, flounders, and so on (Fig. 30). A lady born and bred in London, told me lately that until she attained mature age and married, she did not know that there was such a thing as a flat fish in nature. She saw them, of course, in the shops, but believing that all fish were of the proper accredited symmetrical shape, like salmon, herrings or whiting,

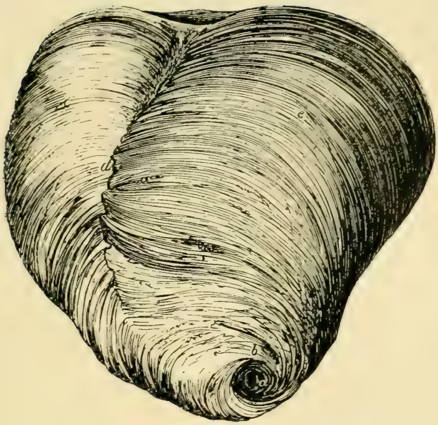

FIG. 29.

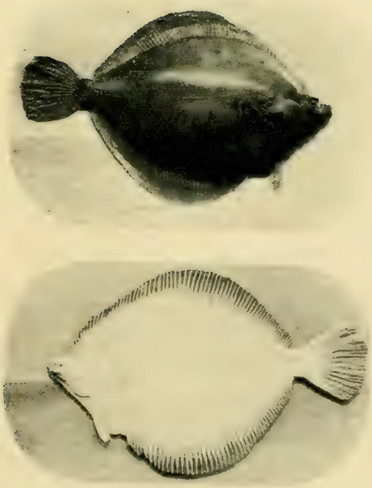

FIG. 30. PLAICE.

FIG. 31.

FIG. 32.

she thought that the flat fish were made so by being beaten out or passed through rollers by the fishmonger, simply for table purposes like crimped cod. The members of the Royal Institution know better, but I am not sure they all know that certain species and genera of the Pleuronectidæ are dextral or sinistral: that is to say, that in one species it is invariably the right side of the fish that is uppermost, coloured and carrying the orbits, and the left side that is downwards and colourless; while in another species the reverse of this holds good. I have said "invariably," but that is hardly correct, for the remarkable fact is that in each species there are occasional rare exceptions to the specific rule, a few sinistral fish occurring in a dextral species, just as there are a few left-handed individuals amongst right-handed human beings.

I am not sure, too, that all Members of the Royal Institution know that the flat fish make a perfectly straight start in life. The very young are transparent and symmetrical throughout, with an eye on each side, and swim in a vertical position like other fishes; and it is as they grow that the skull becomes twisted, and that the eye of one side moves round by degrees to the other side and becomes the upper eye.

This transformation in the flat fishes cannot be attributed to prejudiced mothers or silly nurses, or hide-bound schoolmasters or acquired habits; and the fact that it occurs at a particular stage of growth disposes, I think, of the argument that dextral pre-eminence in the human being must be induced by education, because the baby for the first eight or nine months of its life uses both of its fore-limbs equally. All dextral and sinistral forms and tendencies emerge at different stages in the growth of the animal, in accordance with specific predestination; and some of them, as the sinistral establishment of voluntary speech in the brain, come late.

Next let us take the Crustacea, and with the lobster all of us have an intimate and agreeable acquaintance, and all of us have, I doubt not, observed that notwithstanding its striking symmetry, its two claws, or forceps, which are to it hands and jaws, sometimes vary in size. But more important than that, they differ essentially in structure and in function. I show you a photograph of the claws of *Homarus gammarus*, the common lobster, and you will observe a marked difference in the chelæ (Fig. 31). On the right side you have the sharp tubercles, or teeth we may call them, adapted for the cutting and tearing of food, and on the left side the blunt tubercles, adapted to the crushing and grinding of it. You will see this better in a photograph of the chelæ of the American lobster, *Homarus Americanus* (Fig. 32).

Now let us for one moment turn to the shells, in the spiral forms of which dextral or sinistral tendencies are conspicuous. By far the greater number of univalve spiral shells are, like human beings, dextral, but in nearly every genus a few sinistral monsters occur. If held with the spire pointing upwards and the aperture downwards, the aperture

is to the right of the axis of the spire, so that if we imagine the shell to be a spiral staircase, we should in ascending it have the axis of the spire to the left. We have here the shell of the common snail, *Helix aspersa* (Fig. 33), which is so widely distributed over Europe, Asia, Africa and America, which is commonly dextral but in which occasional instances are found of a sinistral turn ; kerry-handed shells.

We have next the *Fulgar carnica* from Florida, which is commonly sinistral with occasional dextral specimens (Fig. 34) ; and next the *Ampidromus inversus*, very rare, from Annam, which is a near approach to an ambidextrous species, of which dextral and sinistral specimens seem to be about equally numerous (Fig. 35). Some genera are normally sinistral in the embryonic state, but afterwards become dextral.

Now no explanation can be offered of this dextral pre-eminence in shells and these twists and turns in them, but this is certain that they are not due to education or individual environment, for they are seen already declared in the embryo.

I need not remind you that plants as well as animals have dextral and sinistral constitutions. Darwin reported many interesting examples of this in twining and climbing plants. He observed that the hop travels slowly round its support to all points of the compass, moving like the hands of a watch with the sun from right to left right, and that the *Ceropegia gardnerii*—an Asclepiadaceous plant—revolves in a course the reverse of the hop and opposed to the sun (Fig. 36). "A greater number of twiners," Darwin says, "revolve in a course opposed to that of the sun than in the reversed course, and consequently the majority ascend their supports from left to right (Fig. 37). Occasionally, though rarely, plants of the same order turn in opposite directions, but I have seen no instance of two species of the same genus turning in opposite directions, and such cases must be rare." But whatever the fixed habit of the species may be, occasional individual departures from it are noted. Professor Assa Grey noticed that in *Thurga Occidentalis*, the twisting of the bark is very marked. The twist is generally to the right of the observer, but in about a hundred trunks four or five are seen to be twisted in the opposite direction, and thus the number of reversals in this tree is exactly in the same proportion as are left-handed reversals in human beings.

But we have not finished with dextral and sinistral manifestations when we have reached the plants. We have to follow them down into organic compounds. These substances are said to be optically active when they produce rotation of the plane of a ray of polarised light which passes through them, and the establishment of the connection between optical activity and molecular asymmetry is not the least momentous of the discoveries that we owe to Pasteur. His later biological work attracts more attention at the present time, but his researches of 1853 may yet prove to be even more fundamental and far-reaching in their

Fig. 33. Helix Aspersa.

Fig. 34. Fulgar Carnica.

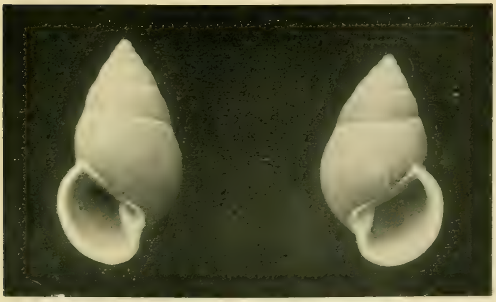

Fig. 35.

FIG. 36.

FIG. 37. CONVOLVULUS (*Common Garden*), CONVOLVULUS (*Major*), and GARDEN HOP.

nature. He himself said to a friend at that time: "I have made a great discovery and am so elated that a nervous tremulousness has seized me." And that great discovery was that the rotation of the plane of polarisation of a ray of polarised light may be dextro-rotary or lævo-rotary according to the nature of the substance through which it passes. The effect is as if the ray had been forced through a twisted medium—a medium with a right-handed or left-handed twist, and had itself received a twist in the process. Some substances produce rotation only when in the crystallised state, but others are optically active in solution. In the former case the molecules of the substance have obviously no twisted structure, but they unite to form crystals having such a structure. As Pasteur expressed it, we may build up a spiral staircase, an asymmetric figure from symmetric bricks; when the staircase is again resolved into its component bricks the asymmetry ceases. It is the building and the builder that have done it. Discussing the question of the molecular constitution of dextro- and lævo-tartaric acid, he says: "We know on the one hand that the molecular structures of the two tartaric acids are asymmetric, and on the other hand that they are rigorously the same with the sole difference of showing asymmetry in opposite senses. Are the atoms of the right acid grouped in the spirals of a right-handed helm, or placed at the solid angles of an irregular tetrahedron, or disposed according to some particular asymmetric grouping or other? We cannot answer these questions, but it cannot be doubted that there exists an arrangement of the atoms in asymmetric order having a non-superposable image. It is no less certain that the atoms of the left acid realise precisely the asymmetric grouping which is the inverse of this."

It is not for me to discuss the question on which chemists have been divided, whether Pasteur was right in regarding the formation of asymmetric organic compounds as the special and exclusive prerogative of the living organism. My point is to accentuate the all-pervading character of asymmetrical arrangements throughout organic nature. In the artificial production of unsymmetrical bodies it is found that the two kinds, dextral and sinistral, are produced in equal numbers, just as a glove-maker turns out an equal number of right and left handed gloves, and by suitable means the two kinds can be separated from each other. In nature, on the other hand, we find that where we have asymmetry the one kind of structure always predominates over the other. The chemist in his laboratory produces dextral and sinistral tartaric acid in equal quantities, but the grape produces only dextral tartaric acid. Similarly the sugar-cane and beetroot produce only dextral sugar, though as far as we know, it would be equally easy for them to produce the sinistral, or a mixture of both. It is not strange that there should be asymmetry in nature, but it is strange that asymmetry should practically exist in one form to the exclusion of the other, and that this should occur alike in molecules and in plants and animals. Look at the human body.

If human beings were synthesised in the laboratory of the chemist, we should have an equal number of persons with the heart on the right and on the left side; but as they are put together in the laboratory of nature, the left side has it by a gigantic majority.

After all asymmetry is all-embracing; it is a property of the globe we inhabit. Our world rotates on its axis, in one direction from west to east, and shows no ambi-rotary predilection; but that implies the possibility of another, not necessarily a better world rotating in the opposite direction.

I have skimmed the subject of right-handedness. There are a hundred aspects of it on which it has been impossible to touch. I have endeavoured to show that the propaganda of ambidexterity is not according to physiological knowledge, and that either-handedness is not the charter of the coming man.

We have been right-handed for a very long while, the foundation of that human characteristic having been laid down long before the first syllable of recorded time.

We have found our right-handedness very useful, civilisation has largely depended on it, and the world is full of the treasures it has piled up.

There is not a tittle of evidence that our right-handedness is growing upon us and that we are becoming more and more lop-sided. We are apparently just as right-handed as were the Greeks in their palmy days, neither more nor less; and, indeed, right-handedness seems to be a terminal form in evolution.

We cannot, I believe, get rid of our right-handedness, try how we may. To "raze out the written troubles of the brain" is no easy matter; to delete its deeply engraven records is a task impossible. Ambidextral culture, useful enough in respect of some few special movements in some few specially employed persons, must on the large scale tend to confusion. Pushed towards that consummation which its ardent apostles tell us is so devoutly to be wished for, when the two hands will be able to write on two different subjects at the same time, it must involve the enormous enlargement of our already overgrown lunatic asylums. Right-handedness is woven in the brain; to change the pattern you must unravel its tissues. My own conviction is that, as regards right-handedness, our best policy is to let well alone and to stick to dexterity and the bend sinister.

[J. C.-B.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

James Herbert Morrell, Esq., B.A.
Frederick Luard Pattisson, Esq.
Otto Oberländer, Esq., Ph.D.

were elected Members of the Royal Institution.

It was announced from the Chair that His Grace the President had nominated the following Vice-Presidents for the ensuing year :—

The Right Hon. Lord Alverstone, G.C.M.G. P.C. M.A. LL.D. F.R.S.
Sir Benjamin Baker, K.C.B. K.C.M.G. LL.D. D.Sc. F.R.S.
Donald William Charles Hood, C.V.O. M.D. F.R.C.P.
Sir Andrew Noble, K.C.B. D.Sc. F.R.S.
Alexander Siemens, Esq., M.Inst.C.E.
The Right Hon. Sir James Stirling, P.C. M.A. LL.D. F.R.S.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. (*Treasurer*).
Sir William Crookes, D.Sc. F.R.S. (*Honorary Secretary*).

The Special Thanks of the Members were returned to Mr. Richard Bagot, *M.R.I.*, for his present of a Water-colour Sketch done by Sir James Ross at the furthest point reached by the Antarctic Expedition of H.M.S.S. " Erebus " and " Terror."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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WEEKLY EVENING MEETING,

Friday, May 10, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Signor Com[E] Giacomo Boni.

Recent Excavations on the Forum Romanum, and the Forum Ulpium.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, May 17, 1907.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Professor George Chrystal, M.A. LL.D. Sec.R.S.E.

Seiches in the Lakes of Scotland.

Historical Introduction.—As my subject to-night is Seiches in the Lakes of Scotland, I could scarcely begin better than by quoting the following extract from the 'Scots Magazine' for 1755, which gives a good general account of the phenomenon called a seiche, and is the earliest accurate account that I know of any such thing in Scotland :—

"On the first of November last, Loch Lomond all of a sudden, and without the least gust of wind, rose against its banks with great rapidity : and, immediately retiring, in about five minutes subsided as low, in appearance, as ever it used to be in the greatest drought of summer. In about five minutes after it returned again, as high and with as great rapidity as before. The agitation continued in the same manner, from half an hour past nine till fifteen minutes after ten in the morning ; the waters taking five minutes to subside and as many to rise again. From ten to eleven, the agitation was not so great, and every rise was somewhat less than the immediately preceding one, but taking the same time, viz. five minutes to flow, and five to ebb as before. About eleven the agitation ceased. The height the waters rose was measured immediately after, and was found to be 2 feet 6 inches perpendicular.

"The same day, at the same hour, Loch Lung and Loch Keatrin were agitated in much the same manner ; and we are informed from Inverness, that the agitation in Loch Ness was so violent as to threaten destruction to some houses built on the side of it."

From this clear account there can be no doubt that the phenomenon described was a seiche, caused by the great earthquake which destroyed Lisbon on the morning of November 1, 1755. In two important respects this seiche is, however, exceptional. Its amplitude, i.e. the rise of the water above the ordinary level, is much out of the common, and, so far as we know, ordinary seiches, which are plentiful enough, are very rarely caused by earthquakes, although earthquakes of small amplitude are of everyday occurrence, as any seismologist will tell you.

The modern history of seiches begins with the researches of Professor Forel at Morges, in 1869. He, and his friends Plantamour (1877), Sarasin (1879), and others, made a thorough investigation of the seiches of the Swiss lakes. Ebert (1901), Halbfass (1902), and Endrös (1903) have done a similar service for many of the German lakes; the results of Endrös in particular being of great interest and variety. Marinelli (1891), Bettoni (1900), Palazzo (1904) and Magrini (1905) have worked in Italy; Von Cholnoky in Hungary; Denison, Henry and others in America; Burton, Nakamura and Yoshida in Japan. But the great central authority in the matter is Forel, and the rest of us are merely children, gleaning in the field which he has harvested before us.

The earliest modern observation of a seiche in Scotland was made in the summer of 1902 by two of the Scottish lake surveyors, Dr. Johnstone and Mr. Parsons, on Loch Treig; and my own connection with the matter began in February 1903, when, at the request of Sir John Murray, I gave a brief account of the hydrodynamical principles of the subject, with suggestions to his surveyors regarding the observations they might make on the Scottish lakes. But I speedily caught the seiche madness myself, and have been devoting most of my little leisure to the subject for the last three years.* In particular, in 1905, I organised for the Lake Survey a seiche campaign on Loch Earn, and it is mainly of some of the work done then that I wish to speak to-night. And first, a word or two regarding the instruments which we used, and their installation.

Limnographic Apparatus used.—One of the simplest and most effective of the instruments for measuring the denivellation of a lake is the Index Limnograph, originally devised by Endrös. Here is a specimen (Fig. 1) of the form used by the lake surveyors, battered by much active service in all kinds of weather. The essential parts are the float, and its sheltering well and access tube; a piece of fly-fishing line, passing from the float over a small pulley to a counterpoise; and an index, attached to the pulley, which indicates on a scale, that can be made as large as we please, the rotation of the pulley, which is of course directly proportional to the rise or fall of the float. The observer is provided with a piece of squared paper, the horizontal divisions of which represent half minutes, and the vertical divisions the readings on the limnograph scale. An observation is made every half minute, and a corresponding dot made on the recording paper. All that is required is a well-sharpened pencil and patience. The result is a curve such as I show you, which we call a Limnogram.

For many purposes it is desirable to have a continuous record, extending over a considerable time, for both night and day. For

* See various memoirs in the Proceedings and Transactions of the Royal Society of Edinburgh.

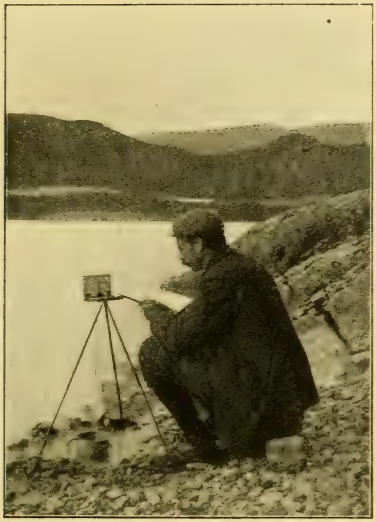


FIG. 1.

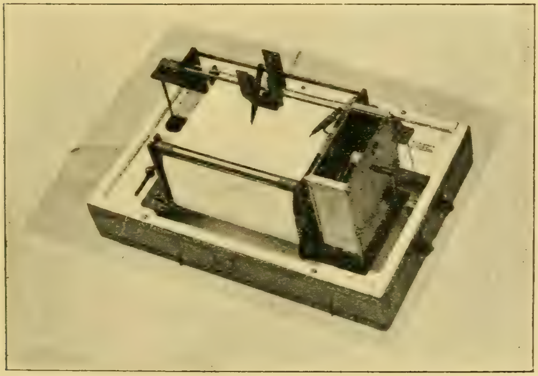


FIG. 2.

this purpose a special instrument was constructed after my design, which we called the Waggon Recorder (Fig. 2). It is really a combination of the essential principles of the older limnographs of Plantamour and Sarasin. The string of the index limnograph is replaced by a Chesterman's steel tape, which passes horizontally over two pulleys, between which it drags backwards and forwards a little waggon carefully mounted by means of three wheels, which run two on one and one on another of two parallel rails. The waggon carries an ordinary stylographic pen, so mounted as to write on a long strip of paper which is moved horizontally by rollers driven by clockwork. As the motion of the paper is perpendicular to the motion of the pen, caused by the rise and fall of the water, the result is the same as before, only the work and the patience are now transferred from the living observer to the waggon and the clock; and the record is absolutely continuous. The lantern-slides which I now show you will give an idea how the instrument is mounted by the

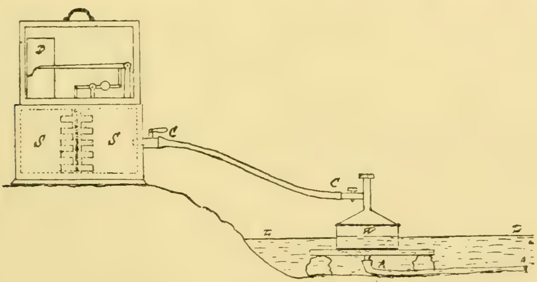


FIG. 3.

lake side, so as to resist the combined efforts of rain, wind and waves, to make an end of the observations of the limnographer. The precautions taken were by no means unnecessary, for in November, 1904, part of the Sarasin limnograph under Mr. Wedderburn's charge on Loch Treig was destroyed during a storm, and there were times during the months of August and September, 1905, when I trembled for the security of our installations.

The slide which I show you next shows a form of limnograph (Fig. 3) which I devised for investigating the nature of the embroidery on the limnograms. It consists essentially of a large and very sensitive barograph (Richard statoscope), which is connected with a closed well placed partly in, partly out of the lake. The rise and fall of the lake level causes a corresponding rise and fall of the water level inside this closed well, thereby increasing and diminishing the air-pressure in the cylinder, into which are fixed the barograph capsules, which are thus compressed and extended like the bellows of a concertina. This compression and extension is transferred by a

system of multiplying levers, working the pen which writes on the recording drum. The inertia of the working parts is very small, and the sensitiveness to alteration of pressure is 15 or 20 times that of an ordinary mercury barometer. The instrument will therefore show quite plainly extremely small denivellations of the lake ; and it can be made more or less sensitive by increasing or diminishing the diameter of the well. By merely turning the stopcock, and shutting off the communication with the well, we convert the instrument into a very sensitive barometer. The curve which it traces is thus changed, at a moment's notice, from a limnogram into a barogram, so that we can alternately record the denivellation of the lake and the variation of the atmospheric pressure. I shall show you some of the results obtained later on. The instrument itself I call a statolimnograph.

In addition to the statolimnograph, and four index limnographs which were worked at constantly varying points, we had three fixed limnographs, one near St. Fillans, one near the binode, and one near the uninode. The last, unfortunately, was useless for a good part of the time, partly because its clock went out of order, partly because the Sarasin gearing proved too crude to deal with the delicate seiches of Loch Earn ; and it was near the end of the time at my disposal before we were able to remodel it on the plan of the waggon recorder which worked so well at St. Fillans.

Besides the limnographic apparatus, we had quite a battery of meteorological instruments : three microbarographs of the Dines-Shaw pattern and a Dines pressure anemograph, which was installed near my house at Ardtrostan and worked beautifully. One of the microbarographs was placed at Ardtrostan, one at the west end of Earn, and one at Killin. At each of these places we also ran ordinary barographs, which were controlled by means of bi-daily observations with a standard barometer in charge of Messrs. White and Watson.

Typical Limnograms.—I will next show you some typical specimens of limnograms obtained in various lakes under various conditions, so that you may gather some idea of the phenomena which we have to correlate, and, so far as possible, to explain.

By the kindness of Professor Forel I am able to show you a facsimile * of what is, so far as I know, the most remarkable seiche record in existence. It is from Plantamour's limnograph at Sécheron (near Geneva), and represents a seiche, having a period of 73·5 minutes, and a maximum range of 9 inches, which lasted from 21 hours on March 26 to 14 hours on April 3, 1891 : that is to say, 7 days 17 hours. Apart from occasional wind disturbances, the oscillations are very regular, as you will see.

You may compare with this a seiche * taken with the waggon

* Exhibited in the Lecture Room and in the Library of the Institution.

recorder at Lochearnhead which lasted for 6 days $11\frac{3}{4}$ hours. It is what we shall presently define as a dicrote seiche. Although insignificant as to range, compared with Plantamour's example, it is, so far as I know, the next best in point of duration yet observed.

The next example is a seiche on the Sea of Galilee.* The range is magnified to about $1\frac{3}{4}$ of the natural size, and an hour occupies about $13\frac{3}{4}$ in. It is the most recent limnogram in my possession, having been taken by Mr. E. W. G. Masterman, on March 26 last, in the course of an investigation into the seiches of the lakes of Palestine.

The next set of examples (see Fig. 4) are from Loch Earn, all taken by the waggon recorder near St. Fillans. The two upper curves are very smooth, and furnish excellent examples of what we

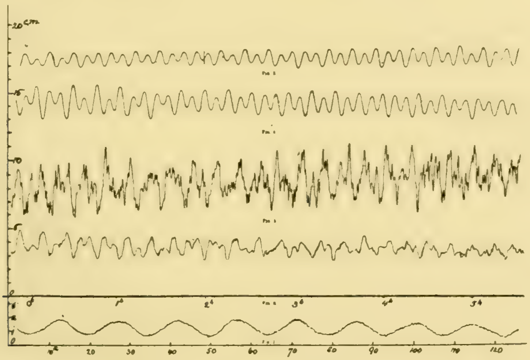


FIG. 4.

call the configuration period of a dicrote seiche. No. 3 is an example of the strongly-marked embroidery which appears on the limnogram during stormy weather. No. 4 is an example of a seiche in moderately calm weather broken by varying weather conditions. No. 5, except for the slight wind embroidery, gives an example of an almost pure sinusoidal curve. It was taken near one of the points on the lake, which we shall presently define as binodes, and furnishes a test of the mathematical theory.

Causes of Denivellation.—Let us now consider the various causes which may affect the level of a lake. We may enumerate the following :—

1. *Volume Denivellations.*—Caused by precipitation or evapora-

* Exhibited in the Lecture Room and in the Library of the Institution.

tion. These are usually of slow variation, easily traced to their causes, and evidently not directly concerned with seiche phenomena.

2. *Persistent Wind Denivellations.*—Due to the heaping up of the water at one end of a lake or in shallow places, where the bottom friction prevents the development of an under return current to counteract the surface wind current. These denivellations are slow and irregular in their variation, and again easily traced to their cause.

3. *Fluctuating Wind and other Denivellations.*—Due to the propagation of trains of waves on the surface of the lake by the passage of wind squalls, and associated with the rapid variations of wind pressure shown by the self-registering anemograph. Such wave trains may also be started by passing steamers or other accidental causes.

4. *Swell Denivellations.*—After a persistent wind has blown for some time over a stretch of water of a certain length, a kind of dynamical equilibrium is established between the wind and the water, and the surface becomes covered with more or less regular trains of progressive waves. Owing to reflection at banks and retardation at shores and shallows, and also to unsteadiness of the wind, there is an interference of superposed trains, which spoils the wave pattern, and prevents absolutely regular periodicity in the denivellation at any given point. The general effect as seen at any one place is, however, a fairly regular pattern of small progressive waves of apparently constant length, usually diversified by wave maxima at approximately equal intervals. This system persists for some time after the wind falls ; and in this stage is usually spoken of as "swell."

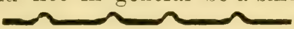
5. *Seiche Denivellations.*—These are stationary oscillations of the whole lake, having nodes (i.e. places of no vertical motion), ventral points (i.e. places of no horizontal motion), and periods depending only on the configuration of the lake basin.

The three last forms of denivellation—which for shortness we may call *solitary wave, swell, and seiche*—all make themselves felt on the limnogram ; and it may be worth while to show you a few experiments to make clear the distinction between them, which I find are often imperfectly understood.

Experiment Showing the Solitary Wave.—You will see that the sudden sweep given to the water at the end of this shallow trough has raised a hump on its surface, which travels along the trough* without very rapid alteration of form, is reflected at the end, and travels backwards, and so on. Observation shows that the particles of water are affected by this wave only while the hump is immediately over

* The trough used was rectangular, 8½ in. broad, 6 ft. 8 in. long, the depth of the water about 3 inches. The experiment will not succeed if the depth of the water be great compared with the dimensions of the wave, or if the depth vary rapidly. It would fail, for example, in the parabolic troughs used in the two succeeding experiments.

them. If the trough were infinitely long, they would come to rest after the solitary wave had passed away. Each particle comes to rest in a position at, or at least near, its original one. It is the wave form, and not the constituent water, that really travels, as you will see by watching the splash of red ink thrown on top of the wave as it passes. Theory and observation agree in giving the formula $V = \sqrt{(g\,h)}$ for the velocity of the highest point of the solitary wave.

It is important to observe that a wave of this kind, travelling backwards and forwards in a lake of uniform breadth and depth, would produce a periodic disturbance at one end of the lake having a period $\frac{2\,l}{\sqrt{(g\,h)}}$. It should be noticed, however, that the curve on the limnogram would not in general be a sinusoid, but something more of this shape  , where periods of positive denivellation alternate with periods of no disturbance.

Progressive Waves Generated by a Wind Current.—We now show the effect of a steady wind current blowing along the surface of water. So long as the current is below a certain strength (0·45 miles an hour), there is no disturbance of the mirror-surface; above that limit, and under a velocity of about 2 miles an hour, there is disturbance which is transient, i.e. does not long survive the disturbing causes (cats-paws). For higher current velocities a regular train of so-called progressive waves is formed, which increase in height and in length as you go down the wind. In nature, the water is comparatively calm at the windward end of the lake, but more—it may be very much—agitated at the other. Even at their greatest, these waves, as you see, are very short (say $\lambda = 0 \cdot 25$ feet); their period also is short (say $T = 0 \cdot 22$). If you watch the thin stream of red ink dropped from the pipette into the water in the trough you will see that the oscillatory disturbance, of which these progressive waves are the manifestation, dies away as you descend from the surface, and is not appreciable at any great depth. From the well known formulæ

$$V = \sqrt{\left(\frac{g\,\lambda}{2\,\pi}\right)} \qquad T = \sqrt{\left(\frac{2\,\pi\,\lambda}{g}\right)}$$

it is easy to calculate the velocity of propagation and the period of these waves.

For Loch Earn, common values of the figures would be about $\lambda = 20$ ft., $T = 2$ sec., $V = 10$ ft. per sec. $= 6 \cdot 8$ miles per hour. Apart from the drift of the surface water as a whole (which you may notice by watching the cloud of red ink), the motion of the individual water particles is in closed elliptic orbits.

Generation of a Seiche by Horizontal Stirring at the Nodes.—Following a method due to two young experimenters, Messrs. White and Watson, to whose results I shall presently refer, Mr. Heath will

now start a seiche in the long tank.* This is done by stirring horizontally in the middle of the tank, with a period of about 5 sec., which depends on the shape of the basin in which the liquid is confined, and on gravity; but on nothing else. The result, you see, is a motion of quite another kind, which we call a pure *uninodal seiche.* It is a periodic wave motion; but the wave form does not travel as in the two former cases. At first sight it would appear as if the surface particles merely moved vertically upwards and downwards, except at one point, which we call the node, where there is little or no perceptible vertical motion of the surface. In reality the water particles describe rectilinear orbits of various lengths, inclined at various angles to the

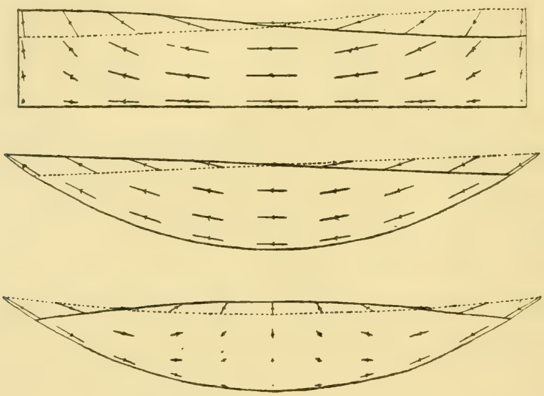


Fig. 5.

horizon. These are drawn to scale for a selection of different particles in the lantern-slide which is shown (see Fig. 5). We can show the nature of the motion at various places by dropping in red ink as before. The whole of the water is now in motion; and the striking thing is that all the water particles keep exact time, like a company of well-drilled soldiers. Each particle is at the middle of its orbit at the same time; each at the arrow-marked end at the same time; and so on. This collaboration we express mathematically by saying that the particles are always in the same phase, although the directions and lengths of the orbits vary from point to point. It is a matter of wonder that this should be the case for two particles 12 feet apart in

* The dimensions of this tank are: length 12 ft., breadth $2\frac{3}{4}$ in., depth 12 in. It was fitted with a wooden parabolic bottom, concavity upwards, the parabola reaching within 2 in. of the top and bottom. The stirring was effected by means of wooden paddles, slightly less in breadth than the trough, reaching nearly to the bottom, and working about horizontal axles resting on the upper margins of the trough.

this tank ; but it seems well-nigh incredible, though unquestionably true, that the same holds for two water particles at the two ends of Lake Geneva, that is to say, 45 miles apart. It was therefore not an obvious remark, but a brilliant generalisation, which Forel made long ago, when he asserted that the seiches of Lake Geneva were standing oscillations, similar in nature to the one which Mr. Heath has just started in the 12-foot tank.

By stirring with a period of about $2\frac{1}{4}$ sec. at a distance from the end of our miniature lake = $0 \cdot 21$ of its length, we raise a standing oscillation of another description, with two nodes each somewhat nearer the end of the lake than a quarter of its length, and a ventral point in the middle. At the ventral point the motion of the water is wholly vertical, whereas at the two nodes it is wholly horizontal. This kind of motion is called a pure *binodal seiche.* The orbits of the particles at various parts of the liquid will be understood from the figure in the lantern slide (Fig. 5).

With equal ease we can stir up a trinodal seiche.

It should be noticed that the uninodal water-surface for a parabolic lake is always a plane, which oscillates about the nodal line between the full drawn and the dotted positions in Fig. 5. For the same kind of lake the binodal water surface is a parabola, which varies in position and curvature between the dotted and full drawn positions.

In the case of a lake of uniform depth, the corresponding surface curves are sinusoids.

Generation of Seiches by Periodic Wind Impulses.—Mr. Heath will now show us another method of generating seiches, which is nearer to nature than the last.

He has covered in half of the small trough,* and arranged to drive an air-blast along the surface as before. So long as the blast is continuous we simply get surface progressive waves, as in a previous experiment. There might possibly be a small seiche left on stopping owing to the drift current carrying an excess of water to the leeward end of the lake, but it is imperceptible. Now he repeats the experiment with an intermittent blast, timed to a period of about 5 sec. by the metronome. The result you see is a uninodal seiche.

If we repeat the experiment, covering in the tank up to the binode merely, and timing the puffs to $\frac{1}{4}$ sec., we get, as you see, a binodal seiche ; for the water rises and falls at both ends together, instead of rising at one end and falling simultaneously at the other, as before.

Hydrodynamical Theory.—And now a word or two about the mathematical aspects, at least the more general of them, of this matter. Wave motions of the kind shown are termed pure seiches, and they may be uninodal, binodal, trinodal, . . ., ν-nodal.

---

* Length, 5 ft. ; breadth, 4 in. ; depth, 5 in. ; depth of water, about 3 in. Like the large trough, it had a parabolic bottom.

From the theory it appears that—

1. In any given lake, pure seiches of all degrees of nodality, i.e. uninodal, binodal, trinodal, etc., are possible; and any actual seiche is either one of these or a superposition of several of them. A compound seiche, which is a superposition of two pure seiches, we call a dicrote seiche; and so on, following the nomenclature of Forel.

2. When the lake is of uniform breadth and depth, the periods are proportional to—

$$\frac{1}{1},\quad \frac{1}{2},\quad \frac{1}{3},\quad \frac{1}{4},\quad \ldots \text{ (harmonic series)}$$

and the quarter-wave length, i.e. the distance from each node to the next ventral point, is the same all over.

3. When the depth or breadth, or both, varies, the periods are no longer commensurable. Thus, for a *complete parabolic lake* the ν-nodal period is given by $T_\nu = \pi l / \sqrt{\{\nu(\nu+1) g h\}}$; that is to say, the periods are proportional to

$$\frac{1}{\sqrt{(1\times 2)}},\quad \frac{1}{\sqrt{(2\times 3)}},\quad \frac{1}{\sqrt{(3\times 4)}},\quad \frac{1}{\sqrt{(4\times 5)}},\quad \ldots$$

Again, for a lake whose longitudinal section (or normal curve) is a certain quartic curve $T_\nu = \rho / \sqrt{(\nu^2 + \epsilon)}$ where ρ and ϵ depend on the dimensions of the lake, and ϵ may be positive or negative, according to circumstances.

4. Hence it follows that the ratio of the binodal to the uninodal period may be less than, equal to, or greater than $\frac{1}{2}$, according to circumstances—a fact which seems to have puzzled seiche observers considerably. Indeed, I have shown that quartic lakes can be imagined in which the periods T_1, T_2, T_3 ..., may be as nearly all equal as we please.

5. The positions of the nodes are given by the roots of certain equations $\chi_\nu(x) = 0$; and the ventral points by the roots of certain other equations $\phi_\nu(x) = 0$. The roots of these equations interlace with each other; but the quarter wave lengths are not, in general, equal, as in the case of the lake of uniform breadth and depth.

6. A shallow or other obstruction, or a deep near a node, greatly affects the corresponding period; a shallow increasing the period, a deep increasing it. Also a shallow attracts the node towards itself, and a deep repels it. Thus, for example, the binodes in a parabolic lake are nearer the ends than in a rectangular one.

If the obstruction at a node is very great, it may render the corresponding seiche unstable, or prevent its occurrence altogether. This explains the absence in certain particular lakes of certain seiches of the theoretically possible series.

Du Boys' Theory.—My predecessor in the mathematical theory of seiches, M. Du Boys, gave, sixteen years ago, in his interesting

Essai Théorique sur les Seiches, an approximate method for calculating the periods of a seiche. He treats the seiche as the interference of two solitary waves travelling backwards and forwards in the lake, the velocity of propagation being at each section that due to the greatest depth there. He thus arrives at the formula

$$T_\nu = \frac{2}{\nu}\int_0^l \frac{dx}{\sqrt{(gh)}}.$$

The symbol $\int_0^l \frac{dx}{\sqrt{(gh)}}$ simply means the time that a man would take to travel from one end of the lake to the other along the line of greatest depth, his speed at each point being that which a stone would have after it has fallen from rest through a distance equal to half the depth at that point.

This formula is exact for a lake of uniform breadth and depth, but errs in excess for a lake having a concave, and in defect for a lake having a convex bottom. But the approximation gets better as the nodality rises; and, for that and other reasons, his rule is very useful in limnographic calculations.

Results of Seiche Observations, and Comparison with Theory.—I propose next to give you a few results, selected here and there from various seiche observers. In the first place, I show you a table of the periods (in minutes) of some foreign lakes. As you see, they vary greatly—from the 14 hours' period of the fair weather seiches of Erie, which is 250 miles long, to the 14 seconds' period of the seiches observed by Endrös in a pond about 120 yards long.

Some Foreign Lakes.

Lake.	Period T_1.	Length in Miles.	Depth in Feet.	
			Max.	Mean.
Erie	960–840	250	180	..
George	131	18	..	16
Geneva	73	45	1014	500
Bodan	56	41	827	295
Neuchâtel	50	24	502	210
Zurich	46	18	470	144
Lucerne	45	24	732	..
Walensee	15	10	496	..
Traunsee	10	7	627	..
Brientz	10	9	856	..

The next table shows the periods found for such of the Scottish lakes as have hitherto been examined. Except for Treig, Ness, Earn and Tay, the determinations are very rough.

Scottish Lakes.

Lake.	Periods.		$\frac{T}{T_2}$	Length in Miles.	Depth in Feet.	
	T_1	T_2			Max.	Mean.
Ness	31·5	15·3	2·06	22	751	436
Tay	28·4	16·4	1·73	15	508	199
Laggan	26·6	..	..	7	174	68
Lubnaig	24·4	..	..	4	146	43
Arkaig	24	..	..	12	359	153
Maree	15	..	..	13	367	125
Earn	14·5	8·1	1·79	6	287	138
Morar	14	..	..	12	1017	284
Fadd	11·5	6	1·91	4	248	102
Chroisg.	11·2	..	..	3	168	74
Treig	9·2	5·2	1·77	5	436	207

[Lantern slides of typical limnograms from various Scottish lakes were then shown. Two of these are given in Figs. 6 and 7.]

I now show you a map of Loch Earn (Fig. 8), on which are laid down the positions of the uninode, binodes and trinodes, as calculated

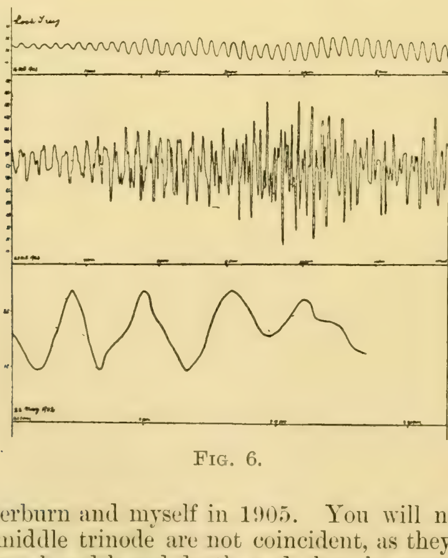


Fig. 6.

by Mr. Wedderburn and myself in 1905. You will notice that the uninode and middle trinode are not coincident, as they would be in a lake of uniform breadth and depth, and that the eastern binode and trinode are nearer the shallower end than $\frac{1}{4}$ and $\frac{1}{6}$ respectively of its whole length.

The calculations for Earn were carried out by determining a number of points on its normal curve * from the data of the Scottish Lake Survey, and then fitting two parabolæ to these points as nearly

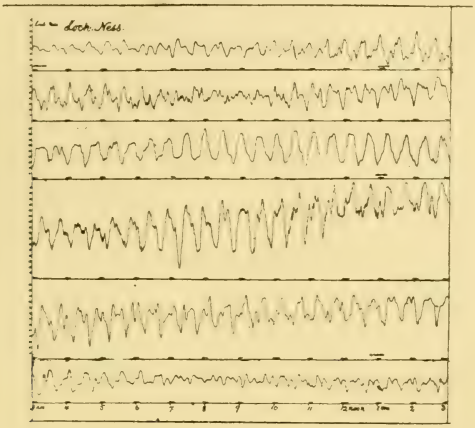


FIG. 7.

as possible by the method of least squares. The fit is by no means perfect, as you see by the diagram now shown; yet the agreement of the calculated with the observed periods is wonderfully good, as is shown by the table on next page.

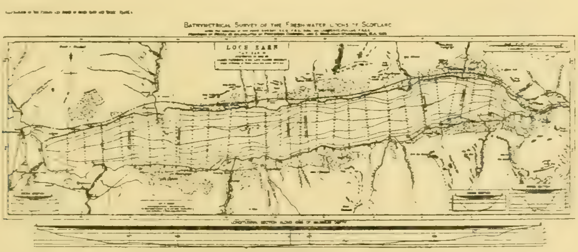


FIG. 8.

We have not yet completed the determination of the nodes from the limnographic observations, but the next slide (see Fig. 9) will show you that our theoretical determinations must be fairly correct. The upper limnogram shows a dicrote seiche at the eastern end of the lake;

* If the lake were of uniform breadth, the normal curve would be the curve of longitudinal section.

PERIODS OF LOCH EARN.

Nodality.	Calculated Period.		Observed Period.
	Chrystal.	Du Boys.	
1	14·50	17·81	14·52
2	8·14	8·91	8·09
3	5·74	5·94	6·01
4	4·28	4·45	3·99
5	3·62	3·56	3·48–3·60
6	2·93	2·96	2·88

the two others show its uninodal and binodal components nearly pure, because they were taken simultaneously with the first, near the computed places of the binode and uninode respectively.

The next picture (Fig. 10) gives a comparison between simultaneous seiches on Lochs Earn and Tay, which you will remember are

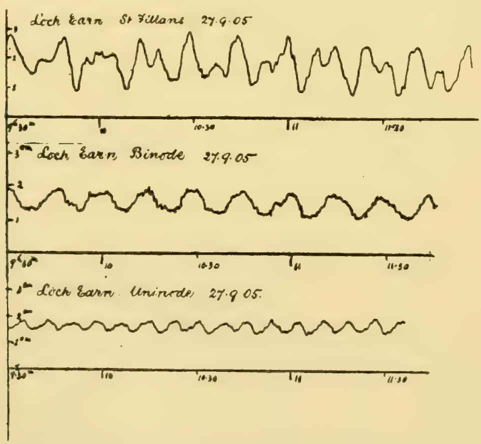


FIG. 9.

not very far apart. The distance between their western ends is 6½ miles. Tay is more than double the length of Earn, but only about 50 ft. deeper. It is therefore relatively a shallower lake. It is curious to notice how variable the seiche is on Tay as compared with Earn, the two pieces of limnogram from the latter being parts of the long seiche of 6½ days which I have mentioned before; and yet the first of the two pieces from Tay is the purest example of a uninodal that we got from Tay during more than two months' observation.

Next I show you a rarity (Fig. 11), a short seiche from Lubnaig, having a period of about 24 minutes. This is the best of only four distinct seiches observed from that lake during six weeks. The modified Sarasin limnograph ran the whole time, and occasional observations were made with the index limnograph as a control. Besides these four seiches nothing else was found but persistent wind denivellation,

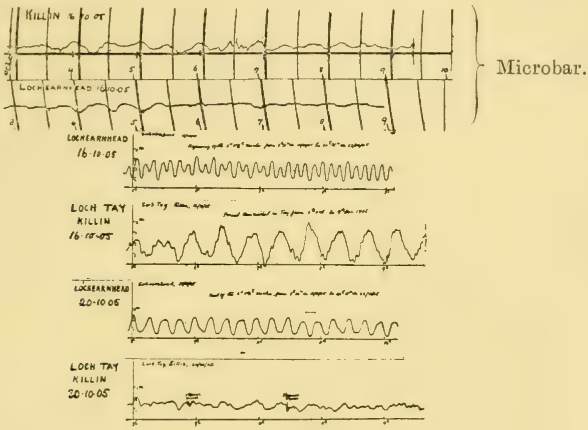


FIG. 10.

and occasionally wind embroidery. Yet during all this time there were continual seiches on Earn and Tay—much more regular on the former than on the latter.

The wonderful persistence of seiches on deep lakes, such as Geneva and Earn, and the almost total absence of seiches on shallow lakes, such as Lubnaig, is a remarkable confirmation of the ordinary theory

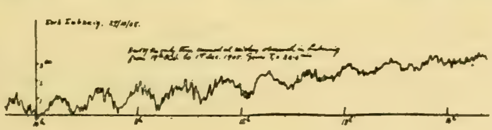


FIG. 11.

of viscous liquids, according to which the dissipation of energy is mainly due to friction at the boundary. A striking example of this has long been known in the case of the ocean swell. This is often propagated with little change of period or wave length for distances of over a thousand miles. No one who has watched from Cliff House, near San Francisco, the great billows that often break on the beach

there in perfectly calm weather can ever forget the economic transmission of energy by the long Pacific swell. Next to the transmission by the ether of energy from the sun, this has always seemed to me one of the most wonderful things in nature.

Origin of Seiches.—The question of the origin of seiches is one of great interest, on account of its connection with meteorology. It abounds, however, in unsettled questions, which could not profitably be discussed within the compass of a single hour. Moreover, the data we have obtained are not yet fully discussed and ready for publication. I will therefore be content with a few notes, the object of which is to draw your attention to the interesting questions involved, and to invite the assistance of any enthusiast present in the attempt to solve them.

Apart from obviously accidental causes, such as landslips, sudden floods, etc., and causes such as earthquake disturbances, which have not been proved to be other than accidental, the main causes of seiches which have been indicated by Forel and his disciples are—

1. The sudden release of a static denivellation of the whole lake surface, caused by a barometric gradient along the lake, which has suddenly been altered.

2. The action over portions of the lake surface of small fluctuations of the barometric pressure, which happen to synchronise with one of the seiche periods of the lake.

3. Action, similar to the last, of fluctuations in the velocity and pressure of the wind, as shown in the anemogram.

4. Sudden disturbance of a considerable portion of the lake surface by the passage of squalls. This disturbance might be of a static character, i.e. due to sudden increase or decrease of the barometric pressure; or it might be of a dynamic character, i.e. due to impact of wind-gusts or rain-showers; or, indeed, partly static and partly dynamic.

Clearly it is only by an induction from a large accumulation of observed facts that we can hope perfectly to disentangle the action of several of these causes, and assign to each its proper share in the origin of seiches. We may notice, however, that the first and last mentioned, which we may class as *Sudden Disturbances*, might be expected, in general, to produce a sudden alteration in the limnogram; while the second and third, which we may class under the general name of *Resonance Disturbances*, would, in general, give rise to a gradual alteration.

That both of these cases are actually observed I now proceed to show by a few examples. In each slide you will see the limnogram, the microbarograms at Ardtrostan, Lochearnhead, and Killin (which is, roughly, about as far north of Lochearnhead as Ardtrostan is east of the same spot), and also the anemogram taken at Ardtrostan.

The limnograms at St. Fillans were all taken by the waggon recorder.

The first slide (Fig. 12) shows a gradually increasing seiche observed on August 14, 1905.

The second (Fig. 13), a seiche suddenly generated on the 17th.

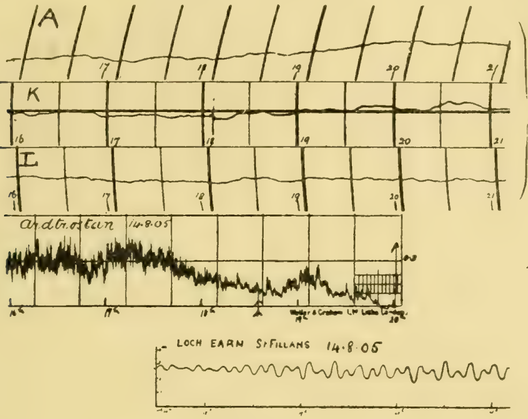


FIG. 12.

The third (Fig. 14), a sudden increase of seiche amplitudes during a storm on the 21st.

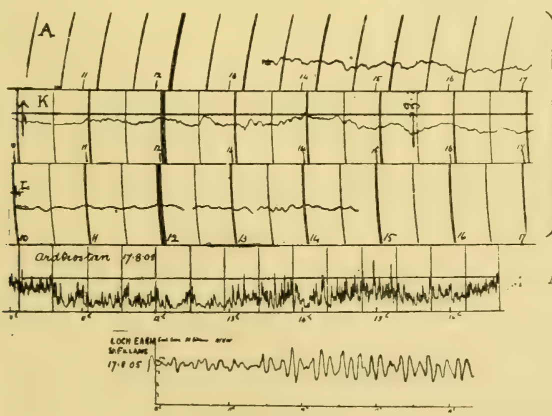


FIG. 13.

The next slide (Fig. 15) is interesting because it shows the sudden generation, on September 7, of a dicrote, compounded of the binodal and trinodal seiches of Earn. This is a very rare, and, so far as we

know, non-persistent seiche configuration on Earn. Indeed the trinodal is so little prominent there that, until we began to analyse the limnograms by residuation, we despaired of getting an accurate determination of its period.

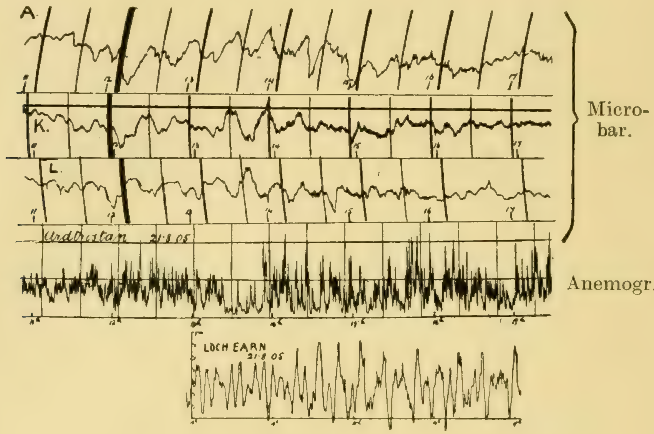


Fig. 14.

I have reserved for the last illustration (Fig. 16) under this head the most striking observation which we made. On August 23, 1905,

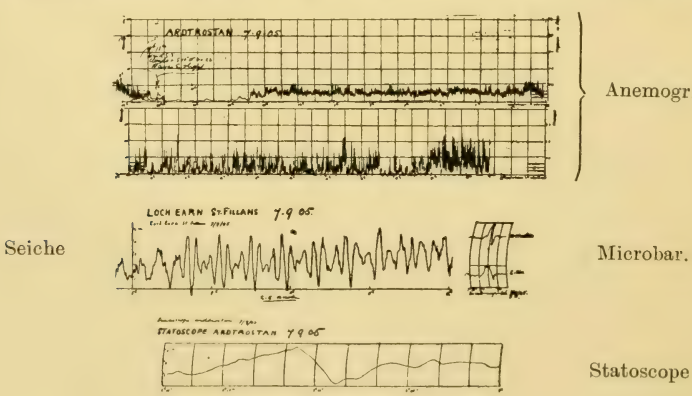


Fig. 15.

I was busy seiche hunting, when I observed a great black storm-cloud come down Glenogle upon Lochearnhead and the upper part of Loch Earn. I was thinking of going indoors for shelter, when

I observed that it suddenly stopped short about the middle of the lake. It was afterwards found that the rain had not come east of Ardvoirlich. The weather in the forenoon had been calm and sunny, and the advent of the squall was quite sudden. This is well seen both on the microbarograms and on the anemograms. One of the Lake Survey staff was looking at the uninode limnograph, and stated that he saw it record the sharp depression on the limnogram just as the squall came up. For some time immediately before, the limnographs at the uninode, binode, and at St. Fillans, had been drawing almost straight lines. The seiche weather had, in fact, been the calmest known in our two months of observation; so that we caught the lake in the very act of responding to the storm. The maximum depression at the uninode and the maximum elevation at

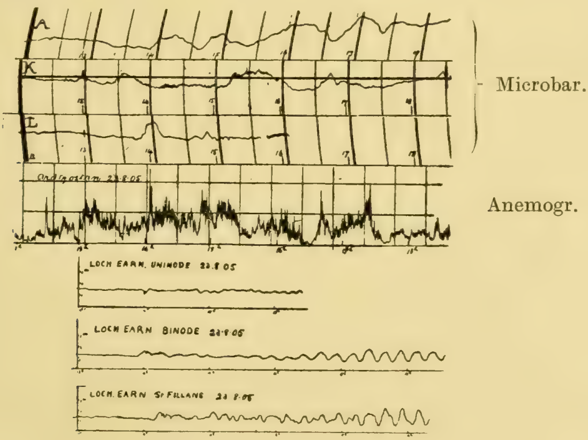


FIG. 16.

the binode were nearly simultaneous, the latter apparently following about $1\frac{1}{2}$ minute after the former. Unfortunately, owing to the irregularity of the clock at the uninode, certainty on this point is not attainable.

It is however abundantly clear, that an elevation of the lake surface travelled along the eastern part of the lake. The first rise occurred at 13 hr. 55·31 min. at the binode; and at 14 hr. 5·24 min., i.e. 9·93 min. later, at the Pic-Nic point. The first maximum elevation is seen at 14 hr. 1·05 min. at the binode, and at 14 hr. 10·57 min., i.e. 9·52 min. later, at Pic-Nic point. If the disturbance had travelled as a solitary wave of sufficient extent to be treated as "long," I calculate that it ought to have taken about 7 minutes to make the journey in question.

After this wave reached St. Fillans, the limnogram shows that it was reflected backwards and forwards between the ends of the lake, at first with a good deal of irregularity. But gradually it developed the characteristics of a regular seiche; and finally settled down into a moderately strong dicrote seiche, in which the uninodal component predominated. This seiche attained its full development about three and a half hours after the squall; and it is a very remarkable fact, that its range considerably exceeds the first maximum elevation recorded immediately after the passage of the squall.

The study of this interesting seiche disturbance raises a number of highly interesting points, which, however, I cannot discuss to-night.

[G. C.]

WEEKLY EVENING MEETING,

Friday, May 24, 1907.

The Right Hon. Lord Kelvin, O.M. G.C.V.O. P.C. D.C.L. LL.D. D.Sc. F.R.S., in the Chair.

Professor J. A. Fleming, M.A. D.Sc. F.R.S. *M.R.I.*

Recent Contributions to Electric Wave Telegraphy.

The achievements and possibilities of wireless telegraphy have not yet ceased to interest the public mind. In less than ten years from the practical inception of that form of it conducted by electric waves, it has developed into an implement of immense importance in naval warfare and manœuvres. It has provided a means of communication between ship and shore which has added greatly to the safety of life and property at sea. It has so far altered the conditions of ocean travel that great passenger liners, separated by vast distances on stormy seas, speak to each other through the æther with far-reaching voices, and are never out of touch with land during the whole of their voyage from port to port.

You are doubtless aware that it is now the usual thing for an Atlantic liner, equipped with long distance receivers, to be in communication with either the Marconi stations at Poldhu in England or Clifden in Ireland, and that at Cape Cod in the United States throughout the voyage, and at the same time to exchange messages not only with the other shore stations when passing, but with a score or so of sister vessels during the journey.*

On board many of the Cunard liners small daily newspapers are published containing the latest news of the day sent by wireless telegraphy from both coasts.

Every important navy in the world has now adopted it in some form as an indispensable means of communication. In our own navy, Admiral Sir Henry Jackson, to whom the country is so much indebted in this matter, informs me that every ship above the size of a torpedo boat is or will soon be fitted. Large battleships carry fairly high power transmitters for long distance work. The Admiralty

* The Cunard liner *Lucania*, which arrived March 18, 1907, at Liverpool from New York, reported that she was, when in mid-Atlantic, in communication by wireless telegraphy with Poldhu, in Cornwall, and Cape Cod, in the United States, at the same time. During the voyage she spoke with thirty-two North Atlantic steamers, and with twenty-four of these she had wireless communication.

are satisfied that this method of signalling is of the greatest utility, and there is no need to remind you of the evidence of this furnished in the recent Russo-Japanese war. No modern liner or large passenger vessel is now complete without a wireless telegraph equipment, and an elaborately organised system of communication has been created by the Marconi Company in connection with this marine telegraphy.

Concurrently with this practical development of the art, much scientific investigation has been conducted, having for its object the elucidation and measurement of the various physical operations involved, as well as further improvement. There comes a time in the history of every applied science when the ability to measure precisely the effects concerned is a condition of further progress. It is this alone which enables us to test our theories, or hold in leash hasty opinions as to the possibilities of the invention.

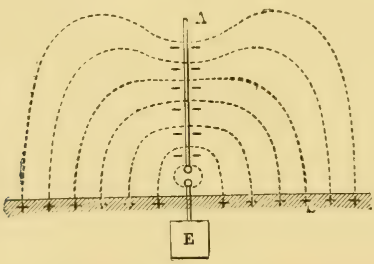


Fig. 1.

Lines of Electric Force round an Antenna before Discharge.

In considering, then, during the present hour some of the recent contributions to this new telegraphy, we may pay a moment's attention to the nature of the things or effects in it which can be measured. An essential element in all electric wave telegraphy is the elevated insulated wire or wires called the antenna, in which high frequency electric currents are set up, and from which the electric waves radiate. Consider a long vertical wire, insulated completely from the earth and charged with electricity (see Fig. 1). There must be somewhere on the surface of the earth nearby a charge of opposite sign. If the wire is negatively charged, then, on its surface, there is, according to modern views, an excess of negative ions or electrons, and on the ground surface round the wire there is a deficiency, that is, there is a positive charge. Furthermore, in the interspace around the rod there is a state of strain of some kind distributed along certain curved lines, commonly called lines of electric force. From one point of view these lines may be regarded simply as a convenient mode of delineating the direction of the strain, having not more material reality than lines of latitude and longitude. There are, however,

some reasons for considering that they *do* possess an actual physical existence, and that they are a necessary part of the mechanism of atoms and electrons.* They have a strong resemblance in many ways to the vortices or vortex lines, which can be created in a fluid. Moreover, just as vortex lines in a fluid can be self-closed or endless, or else terminate in little whirlpools on the free surface of the liquid, so lines of electric force can form either closed loops, or else have their ends terminating on opposite charges of electricity, that is, on an electron at one end and the positive charge of an atom, whatever that may be, at the other end. Suppose, then, that the rod is suddenly connected to the earth at the bottom end by allowing it to spark to the earth. Its electric charge rushes out, that is, the excess or deficit of electrons on its surface disappears, and this movement of electricity constitutes an electric current flowing into or out of the rod from the earth. The electrons, however, possess inertia or mass, hence when they rush out of the rod into the earth they not only discharge it, but overdo it, and leave the rod with a positive charge. They then rush back again, and the process repeats itself, and we thus obtain a rapid ebb and flow of electricity into and out of the wire, called a series of electric oscillations. Each rush, however, is feebler than the last, and therefore the oscillations decay away or, as it is termed, are damped. The energy represented by the initial charge is frittered away, partly owing to collisions of the electrons and atoms in the rod and spark during the movement, and partly because the electron radiates or communicates its kinetic energy to the medium when it is accelerated or retarded.

We have next to attend to the effects taking place outside the rod or antenna. As the negative charge disappears from the rod owing to the removal of the excess of free electrons from its surface the ends of the lines of electric force which abut on it and stretch between it and the earth glide downwards along the rod and end by forming a semi-loop of electric force or strain with its ends or feet resting on the earth (see Fig. 2). This arises from the facts that the lines of force exercise a lateral pressure on each other, whilst lengthways they are in a state of tension, and also that lines of electric strain cannot exist inside a conductor such as a spark. Hence when the spark happens, the lines which a moment ago stretched across the spark gap disappear. There is then an unbalanced pressure on the remaining lines which are thus squeezed in towards the gap and deformed, so

* *Cf.* Faraday. "Experimental Researches in Electricity," vol. iii. series xxix., 3273, 3297, and 3299. "On Physical Lines of Magnetic Force." Faraday used the expression physical line of force to denote their concrete reality as distinguished from a mere geometrical conception. Also in his paper, "Thoughts on Ray Vibrations," Phil. Mag., ser. 3, vol. xxviii., 1846, he considers that light may be a vibration propagated along lines of force. See also J. J. Thomson, "Electricity and Matter," p. 63, for an argument for the physical reality of lines of electric force drawn from the ionisation of gases by Röntgen rays.

that they finally extend, not from rod to earth, but from two adjacent places on the earth and form a semi-loop.

But as above explained the rod does not simply become discharged. Owing to the inertia of the electrons when they rush out, they more than discharge the rod, they overdo it and leave it positively charged. This then implies that a fresh system of lines of electric force grows up between the earth and the rod, and the first formed set of semi-loops is pushed outwards. Then the process is repeated as the oscillations of the electrons in and out of the rod die away, and in the space around we have a system of semi-loops of electric force being pushed outwards in every direction as shown in the diagram in Fig. 2.*

There is, however, another factor involved in the process. The movement of the electrons into and out of the rod constitutes an alternating electric current and this is accompanied by the production of an alternating magnetic field, the direction of which is represented by a system of concentric circles with their centres on the antenna (see Fig. 3). When the current in the rod is reversed in direction,

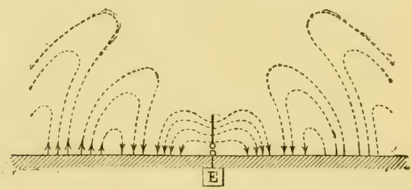


Fig. 2.—Diagrammatic Representation of the Detachment of Semi-Loops of Electric Strain from a Simple Marconi Antenna.

the field is not reversed at all parts of space instantaneously, but the reversal is propagated outwards with the speed of light. Accordingly, the electric oscillations in the antenna create periodic variations of magnetic force and electric force in the space outside. At points near the earth's surface some way from the rod the magnetic force is parallel to, and the electric force perpendicular to the surface of the earth or sea. Experience shows that electric wave telegraphy over any large distances cannot be conducted unless the antenna is so placed that the electric force is perpendicular to the surface of the earth or sea.

* In referring to lines or semi-loops of electric force as moving through space, we do not necessarily mean to imply that each line is ear-marked so that it preserves an individual identity. All that actually happens at any point in the field is a periodic oscillation or cyclical change in the electric and magnetic force at that point. This, however, is repeated successively from point to point, and we may hence speak of the line of force as moving forward just as we speak of a surface water wave moving forward, when in reality the only movement in the latter case is a small up and down motion of the water at each place, or at least a circular motion of no very great extent.

At any distance from the antenna, and at any one spot, the magnetic and electric forces are therefore periodically varying in magnitude, and owing to the finite rate of propagation of the forces through space we find that at certain equispaced intervals these forces are similarly reversed in direction at the same instant.

When we speak of the length of the electric waves we mean the shortest distance which separates two adjacent places at which either the electric or magnetic force reverses direction in the same way at the same instant. In wireless telegraphy the length of waves employed may vary from 200 or 300 feet to many thousands of feet or several miles. The determination of this wave length is a practically important matter, and accordingly instruments have been designed specially for its measurement by Dönitz, by Professor Slaby, and by me. I have ventured to name my own appliance for measuring long electric wave lengths, a *cymometer.** The importance

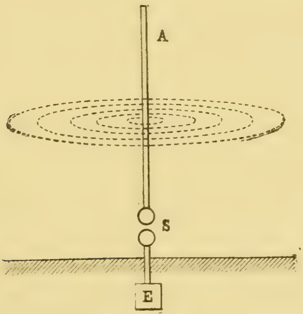


FIG. 3.

LINES OF MAGNETIC FORCE ROUND AN ANTENNA DURING DISCHARGE.

of the measurement is as follows: We know that the properties of short electric waves constituting light and radiant heat depends upon their wave length, and that some bodies are opaque to light waves but transparent to heat waves. So in the case of the much longer ether or electric waves used in telegraphy, the ease with which they pass through buildings, forests, and even mountains or cliffs, or round the earth's curved surface is determined by their wave length. Waves of one or two hundred feet in length are considerably obstructed by the closely packed houses in a town, but much longer waves go easily through them. The measurement of the wave length is made to depend upon the fact that there is a simple relation between the velocity of these waves (which is the same as that of light), the

* See Proc. Roy. Soc., vol. lxxiv., p. 490, 1905. On an instrument for the Measurement of the Length of Long Electric Waves. Also Phil. Mag., June, 1905, on the Applications of the Cymometer.

periodic time of the oscillations in the antenna, and the wave length as expressed by the formula *wave length = velocity × periodic time.* Since the velocity is nearly 1000 million feet per second the wave length in feet is easily found, when we know the time period of the oscillations in the antenna. This last quantity can be found by placing near to the antenna a circuit in which secondary electric oscillations can be sympathetically induced by those in the antenna. For this purpose we must have a circuit which possesses the two qualities of capacity and inductance. This is secured by joining in series some form of Leyden jar or condenser and some form of spiral wire or inductance. Moreover, we must have the means of varying this capacity and inductance, so as to bring the cymometer circuit into tune, as it is called, with the antenna. Every such circuit containing capacity and inductance has a natural period of electric oscillation, resembling in this respect the time of swing of a mechanical system composed of a heavy body suspended by an elastic spring.* In my cymometer the condenser part consists of one to four sliding tubes, each consisting of a pair of brass tubes, separated by an ebonite tube. The outer tubes can slide off the inner ones and so vary the capacity. The inductance consists of a long spiral of copper wire, and the circuit is completed by a thick copper bar. Matters are so arranged that when the outer tubes are drawn off the inner tubes so as to vary the electrical capacity, the effective amount of the spiral included in the circuit is simultaneously varied in exactly the same proportion. To determine when the time period of the cymometer circuit is in agreement with that of the antenna, I use a neon vacuum tube. Some three years ago I found that such a tube was extremely sensitive to a high frequency, electric field being caused to glow brilliantly when subjected to its action.

You are already familiar with the beautiful method discovered by Sir James Dewar for obtaining neon from atmospheric air by the use of charcoal at very low temperatures, and tubes filled with rarefied neon prepared by his process are able, as I have shown, to serve important purposes in connection with wireless telegraphy.

In the cymometer a neon tube is connected to the opposite coatings of the condenser. If then the cymometer bar is placed near to the lower part of a transmitting antenna, and we slide along the outer condenser tube, thus varying the capacity and inductance of the instrument, provided it has a suitable range, a position will be found in which the neon tube glows brightly. The cymometer is equipped with a scale which shows for every position of its handle the corresponding frequency or time period, and the related wave length. Hence the simplest operation, which a child can perform, serves to determine in one instant the frequency of the oscillations

* If the capacity C is reckoned as usual in microfarads, and the inductance L in centimetres, then the time period T of the oscillation is given by the ormula $T = \sqrt{CL}/5033000$.

in the antenna and the wave-length of the radiated waves. I have devised instruments of this type covering the whole range of wave-length measurement from 50 to 100 feet up to 20,000 feet or more. An instrument of the same kind, but with a more sensitive oscillation detector than a neon tube, can be used to measure the wave-length of waves being received on the antenna. The cymometer has other uses besides wave-length measurement. One of these is to draw a resonance curve and thence deduce the rate of decay of the oscillations in a train and their number. In a train of oscillations the time period occupied by each oscillation, whether of current or potential, is the same, but the amplitudes die away in geometric ratio. Hence the ratio of two successive amplitudes or oscillations is constant, and the natural logarithm of this ratio is called the *decrement.* We can determine this decrement when we know the frequency of the oscillations in the primary circuit and the current induced in any secondary oscillation circuit, placed near to the first, when the latter is in exact syntonism, and also slightly out of syntonism, with the primary. Employing a formula of Bjerknes, we can find the sum of the decrements D and d of the primary and secondary circuits by the formula

$$D + d = \pi\left(1 + \frac{n}{N}\right)\sqrt{\frac{a^2}{A^2 - a^2}}$$

where a is the current in the secondary circuit when it is tuned to a frequency n, and A is the maximum current when the secondary

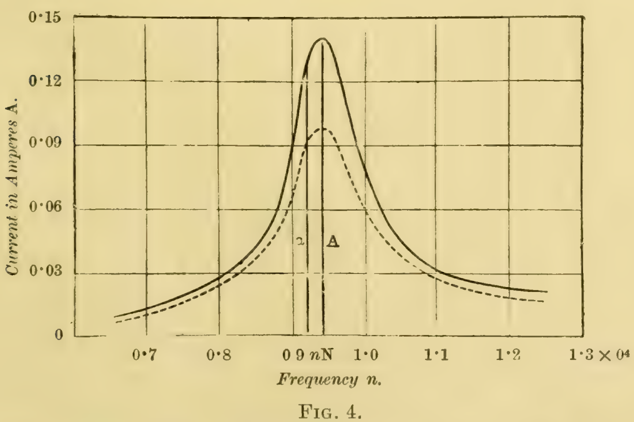


Fig. 4.

Resonance Curve of Loosely Coupled Oscillatory Circuit.

circuit is tuned to agree with the frequency N of the primary circuit. For this purpose I modified the cymometer by including in the bar

two fine resistance wires, against one of which a sensitive thermo-junction of iron and bismuth is attached. This enables me to measure the value of the current in the cymometer bar. The process of measurement is then as follows : We place the cymometer alongside the antenna and slide along the handle slowly, thus altering its time period or natural frequency. We observe the current and frequency, and plot a curve called a resonance curve showing the secondary or cymometer current in terms of the frequency (see Fig. 4). This curve rises to a maximum value, sometimes very sharply, the maximum corresponding to the condition of exact syntonism between the antenna and cymometer circuits.* From the curve we can determine the sum of the decrements of the cymometer and antenna. A second experiment made with a known additional resistance inserted in the cymometer bar enables us to eliminate the decrement (D) of the cymometer itself, and thus find that of the antenna alone. When this is done we know what percentage each oscillation in the antenna is of the previous one. Suppose we agree that when the oscillations have decayed away to 1· per cent. of their initial value, the train

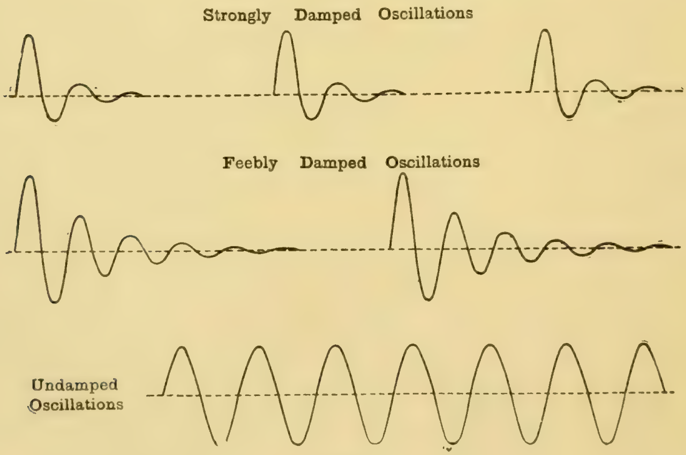


FIG. 5.

shall be considered to be finished, then another simple formula $M = (4 \cdot 605 + D) / 2D$ enables us to find the number of the complete oscillations M in a train when we know the decrement D.†

* If the damping of the secondary circuit is small, as it is in the case of the cymometer circuit, then the resonance curve is very sharply peaked or rises quickly to a maximum when the primary oscillations are feebly damped, provided always that the "coupling" or mutual inductance of the two connected circuits is small.

† See "The Principles of Electric-Wave Telegraphy," Fleming, p. 167.

Electric oscillations are classified into highly damped, feebly damped and undamped varieties corresponding to few, many and infinite oscillations in a train (see Fig. 5). In electric-wave telegraphy we have various kinds of transmitters or wave-makers which are intended to create these types of oscillation. In the first case, if we set up an antenna and connect the lower end to one of the spark balls of an induction coil, the other being to earth, we have an arrangement which produces highly damped oscillations and waves (see Fig. 6). This is due to the fact that since the capacity of the

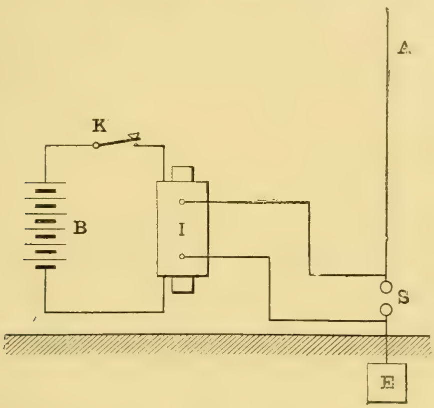


Fig. 6.
Antenna emitting Strongly Damped Telegraphic Electric Waves.

antenna itself is small, the energy which can be stored up in it and liberated at each spark discharge is also small, at most a fraction of a foot-pound or a few foot-pounds. Hence it is rapidly frittered away by resistance and in radiation, and the oscillations are few, say half a dozen or so, and highly damped. If, however, we form an oscillation circuit consisting of a large condenser, inductance and spark gap we can store up a larger amount of energy and liberate this suddenly across the spark gap at each discharge (see Fig. 7). If, then, these oscillations are made to induce others in a directly or inductively connected antenna, we can liberate the energy as radiation, and having a larger store to draw upon create longer trains, say of 20 to 100 more feebly damped oscillations.

Corresponding to these types of transmitter there are various suitable forms of receiver. With a highly damped radiator we must use some form of wave-detector, such as a coherer, which is chiefly affected by the first or maximum oscillation, and this must be inserted in a receiving circuit which is easily set in oscillation by a single or at most a few electromagnetic impulses. On the other hand this

renders the receiver more liable to disturbance by vagrant electric waves due to atmospheric electricity, or other transmitters if of sufficient strength.

If, however, we employ a feebly damped radiator emitting long trains of waves, say 20 to 50 waves, we can make use of a stiffer receiver circuit, that is one containing a good deal of inductance, and a detector such as Marconi's magnetic detector, which operates under the action of feeble but oft-repeated and properly timed impulses. We have then the advantage that the receiving circuit can be made far less sensible to non-syntonic or isolated impulses unless these are of extreme violence.

Again, there are certain forms of detector—such as the thermal and one of my own, to be described presently—which are affected by

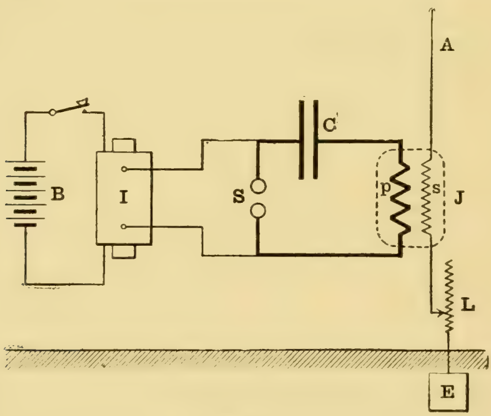


FIG. 7.

ANTENNA EMITTING FEEBLY DAMPED TELEGRAPHIC ELECTRIC WAVES.

the product of the mean-square value of the oscillations during a train and by the number of trains per second. Hence, in this case the effect on such a receiver at a given distance under the same conditions will be increased by increasing the number of trains of oscillations per second, as well as by diminishing damping in each train. It was therefore foreseen that we should gain some advantage by the use of undamped trains if some form of electric radiator could be found emitting waves continuously, like the steady note of an organ-pipe, rather than sounds like intermittent blasts on a trumpet or blows on a drum. There are at least three ways in which these undamped oscillations can be created. The first is a mechanical method, viz. by a high-frequency alternator. Assuming we possess an alternating current dynamo giving a current of a sufficiently high frequency, we can connect one terminal to earth and the other to a radiating

antenna, and then on setting the machine in operation high-frequency undamped currents would be created in the antenna, and corresponding waves radiated. To secure the best results, it is necessary, however, to syntonise the free-time period of the antenna circuit and the natural frequency of the alternator. The chief difficulty, however, is to construct a machine which shall give alternating currents of sufficiently high frequency and voltage with sufficient power and current capacity. Sixteen or seventeen years ago Professor Elihu Thomson and M. Tesla built dynamos giving an alternating current of 10 amperes at a frequency of 10,000 to 15,000, and an output of about 1000 watts. Mr. Duddell exhibited to the Physical Society, in April 1905, an alternator capable of a frequency of 120,000, but its power output was not more than 0·2 watt. I have on the table a small alternator made by Mr. S. G. Brown, giving an alternating current having a frequency of 12,000, an E.M.F. of 20 volts, and a power of about 50 watts. Professor Fessenden has recently given a description of an alternator made for him having a frequency of 60,000, with an output of 250 watts, running at a speed of 10,000 R.P.M., and giving an E.M.F. of 60 volts. Since steam turbines of the Leval type are now made to run at 500 revolutions a second, it is not difficult to construct an inductor alternator having a frequency of 50,000 to 100,000. Such a type of alternator has, however, always a large fall in terminal potential difference if called upon to give out current. For this reason, a type of machine without iron in the armature is to be preferred, but then it becomes more difficult to balance the moving parts for very high speeds. In spite of some attempts, the difficulties of making and driving a high-frequency and high-potential alternator of any considerable output, say 10 kilowatt size, have not yet been overcome. Even if we could secure a frequency of 50,000, this corresponds to a wave of 4 miles in length, and special antenna arrangements are necessary to radiate and receive such waves. Hence the alternator method of electric wave production will certainly not supersede the spark method, although in some cases it may be practicable and useful.

In the next place we have the electric arc method to which so much attention has lately been directed, employing a continuous current arc with a condenser and inductance placed in series across the terminals of the arc. As in many other cases the seeds of this invention were sown in the form of discoveries by several workers. In July 1892, Professor Elihu Thomson filed a United States Patent No. 500,630, in which he proposed a method for creating high-frequency alternating currents by connecting a condenser and inductance to a pair of spark balls and this spark gap was also connected through two other inductances with a source of continuous current supply such as a storage battery or dynamo (see Fig. 8). An air blast or magnetic field was employed to continually extinguish the continuous current arc formed. The operation of the arrangement was thought to be as follows. When

the arc is blown out, or before it is formed, the condenser is charged by the dynamo.* When the arc is re-established the condenser is discharged with oscillations. In the above specification nothing is said about the use of a continuous current arc between carbon poles, but Professor Thomson asserts that oscillations with frequency up to 50,000 could be obtained. In 1900 Mr. Duddell showed that if a suitable condenser and inductance was shunted across the poles of a continuous current arc formed with solid carbons, high-frequency alternating currents were set up in the condenser circuit and the arc emitted a musical sound (see Fig. 9).

Much discussion subsequently took place as to the causes of the effect and as to the highest frequency of oscillation it was possible to secure by this method. Duddell and others based their explanation

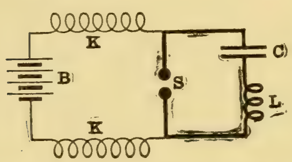


Fig. 8.—Elihu Thomson's Method of producing Oscillations.

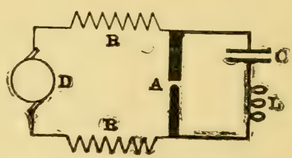


Fig. 9.—Duddell Musical Arc.

of the phenomenon upon the known fact that a small decrease in the current through the carbon arc is accompanied by an increase in the potential difference of the carbons. The continuous arc with solid carbons was said therefore to have a negative resistance.†

The explanation of the manner in which the continuous current arc maintains undamped oscillations in the condenser circuit is then as follows: If a condenser and inductance are shunted across the arc, the condenser begins to be charged, and this robs the arc of some current. This change, however, raises the potential difference of the carbon poles and the charging of the condenser therefore continues. When the condenser is full the arc current is again steady. The

* An interesting and not very dissimilar device has recently been described by Mr. S. G. Brown; he employs a revolving aluminium wheel against which a copper spring presses lightly. The spring and wheel are connected through an inductance and resistance with a source of direct current supply, and also by a circuit consisting of Leyden jar in series with a coil of wire. When the wheel revolves an arc is formed at the loose contact, and high-frequency oscillations are set up in the Leyden jar circuit, see *The Electrician*, November 23, 1906, vol. lviii., p. 201.

† The term negative resistance is a very inappropriate term. It is better to call the curve for an electric arc showing the relation of current through the arc to potential difference of the electrodes or poles the *characteristic curve* of that arc, following a usual nomenclature in connection with dynamos. This characteristic is a curve sloping downwards when the current is taken as abscissa and the P.D. as ordinate.

condenser then begins to discharge back through it, and this increases the current through the arc and therefore decreases the potential difference of the carbons. The condenser therefore continues to discharge. The action resembles that by which the vibrations of the column of air in an organ pipe controls the behaviour of the jet of air from the mouth which impinges against its lip, forcing the jet of air alternately into and outside the organ pipe, and so maintaining stationary oscillations in it. The jet of air from the mouth of the pipe corresponds to the continuous current arc, the closed or open pipe associated with it is a resonant circuit and corresponds with the condenser and inductance.

Consider the state when the oscillations have been set up in the condenser circuit. We must assume that there is a stream of electrons from the negative terminal of the arc making their way across the interspace to the positive terminal. If then we consider the state at the instant when the condenser has reversed its charge, so that the coating connected to the negative arc terminal is positively charged, we see that there is a tendency for the stream of electrons to enter the condenser and supply the deficiency represented by the positive charge on that plate. They are, so to speak, sucked into the condenser. Accordingly this action either annuls or reduces the current in the arc. When the condenser is charged to the potential difference then existing between the terminals of the arc, no more electrons enter it, and they then all travel across the arc. This increase in the arc current is accompanied by a fall in the electronic density difference, or the potential difference of the arc terminals, and the condenser then begins to discharge across the arc, and still more reduces this potential difference. Owing to the inductance in series with the condenser, or in other words in consequence of the kinetic energy of the moving electrons, the condenser is not only discharged but charged up again in the opposite direction.* It parts with the excess of electrons forming the negative charge on its plate in connection with the negative arc terminal, and that plate is left with a deficiency of electrons, that is with a positive charge. Then the process repeats itself over again. Two conditions seem necessary for the automatic continuance of this process. First, the arc must be formed between terminals of such nature and in such surroundings that rapid variations of current through it must cause correspondingly rapid and large changes in the potential difference (P.D.) of the terminals in an inverse sense, that is, as H. T. Simon has shown, there must be a steep falling characteristic curve for the arc (see Fig. 10).† Secondly, the arc

* The amplitude of the potential difference of the condenser terminals may and does become very much greater than the mere steady potential difference of the electrodes between which the arc is formed. Thus with a P.D. of 220 or 300 volts across the arc the R.M.S. of the condenser plates may reach 1000 or 1500 volts.

† A careful study of the phenomena of the electric arc between metal and metal and carbon terminals in air and hydrogen has recently been made in

must have the power of re-starting itself if entirely extinguished for a short time, but this should not take place until the P.D. between the terminals exceeds a certain value, that is, it must not take place too easily or at too low a voltage. If the arc is formed between solid

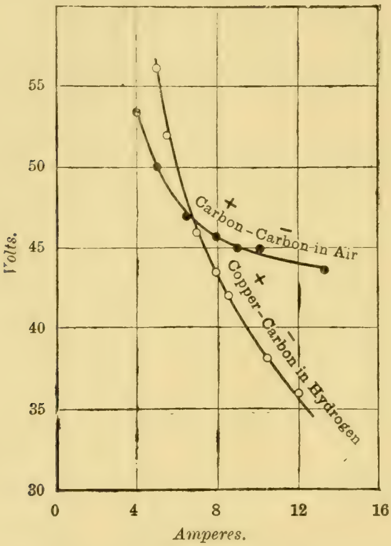


FIG. 10.—CHARACTERISTIC CURVES FOR CONTINUOUS-CURRENT ARC IN AIR AND HYDROGEN (UPSON). Arc length = 1·25 mm.

carbon terminals then it appears that these conditions are only fulfilled up to a certain frequency, that is when employing a rather large capacity in the condenser circuit. We then obtain Mr. Duddell's musical or singing arc, which emits a sound because the rapid variations of current through the arc, by varying the energy expended in it, expand and contract the column of incandescent vapour forming the true arc, and therefore the layers of air next to the arc, and hence send out air waves which are heard as sound. Frequencies up to 10,000 or so are possible, although many physicists, such as Banti, Corbino, and also Maisel, contend that much higher frequencies can

my laboratory, under my direction, by Mr. W. L. Upson. It has been found that for an arc between a cold metal and a carbon terminal in hydrogen for the same length of arc, the rate of decrease of terminal voltage with increase of current is always greater than for an arc in air between two carbon terminals; in other words, the volt-ampere characteristic is steeper. Also it has been found that in the case of a carbon arc in air the current can be interrupted for a much longer time without permanently extinguishing the arc than is the case for the metal-carbon arc in air or hydrogen.

be obtained. In 1903 Mr. Poulsen introduced a further improvement. He found that by enclosing the arc in a vessel containing hydrogen or coal gas, and forming the arc between a cold metal terminal, which is the positive, and a large carbon terminal, which is the negative, the arc being also traversed by a strong magnetic field, much higher oscillation frequencies could be obtained than with the double carbon arc in air (see Fig. 11).

He also found it is an advantage to rotate the carbon terminal. When this arc is shunted by an appropriate small condenser in series with an inductance, we can obtain in this last circuit electric oscillations having a frequency of a million or more depending on the capacity and inductance used. If a suitably tuned antenna is connected to one terminal of this condenser, and one arc terminal to

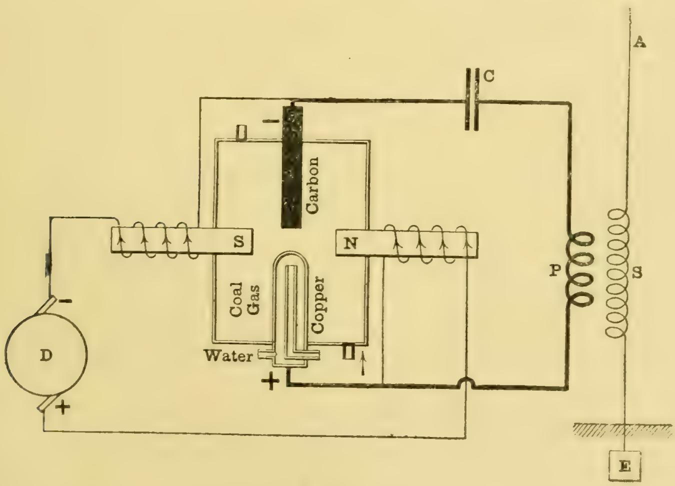


FIG. 11.
POULSEN'S APPARATUS FOR CREATING UNDAMPED OSCILLATIONS.

the earth, as shown in the diagram, we are able to radiate from the antenna undamped trains of electric waves.

I have before me an apparatus of this kind with which much work has been done in my laboratory during the last few months. It consists of a water-jacketed brass cylinder with marble ends, through which project at one end a thick carbon rod, kept in rotation by a motor, and at the other a water-cooled brass tube with copper beak at the end. An electric arc is formed with 400–500 volts between these terminals taking 6–10 amperes.

The terminals are connected by a sliding inductance and by a condenser. Then, in addition, a long helix of wire is connected

to one terminal of the condenser. This helix is tuned to the condenser circuit and may be taken to represent the antenna when the apparatus is used in wireless telegraphy. If we start the arc, then high-frequency oscillations are produced in the helix, and by the action of resonance the potential at the free ends becomes large enough to create an electric brush discharge. There is, of course, a strong oscillatory electric field outside the helix, and vacuum tubes held there, particularly neon tubes, glow brilliantly. It has been contended that these oscillations are undamped and continuous, but I can show you a simple experiment with a neon tube which proves that they are not always uninterrupted. If I hold a neon tube near the helix, and move it rapidly to and fro, you see a broad band of light, due to persistence of vision, but this is cut up by dark lines and spaces. In the same manner if a neon tube is rotated near the helix it does not produce a uniform disc of light, but the disc presents the appearance of radial dark bands and bright spaces. The same effect is seen with a vacuum tube filled with any other gas, provided the tube is sufficiently narrow in the bore. It appears to me that this proves incontestably that the oscillations are not uninterrupted, but are cut up irregularly into groups of various lengths.*

To obtain these high-frequency oscillations the various contributory factors—strength of magnetic field, length of arc, supply of coal gas—have to be carefully adjusted with reference to the capacity and inductance used and the voltage on the arc. No one who has practically worked with the apparatus can say that it is a simple and easy one to use. A very little want of exact adjustment causes the arc to be extinguished or else it fluctuates greatly in current, and compared with the extremely simple appliances required for spark telegraphy, the advantage in ease of working is largely on the side of the spark. But we have to consider whether there are not counter-balancing advantages as a generator of telegraphic electric waves which make up for the increased difficulty of working and greater complexity of apparatus. The claim made for it is that if the transmitter produces undamped continuous oscillations these can be reduced to such small amptitude that they will not affect other neighbouring wireless non-syntonic receivers even if only a little out of tune, but can by the cumulative effects of resonance actuate their own corre-

* Previous experimentalists seem to have been satisfied with examining in a revolving mirror the flaming arc or brush produced at the secondary terminals of a transformer, the primary of which forms the inductance in the condenser circuit, and finding the image drawn out into a band of light concluded that the oscillations were continuous. The neon tube is a more delicate test, and reveals the discontinuity mentioned above. This discontinuity of the train of oscillations seems to depend to some degree upon a want of perfect regularity in the rotation of the carbon terminal. It may also be brought about by the energy transferred to the condenser circuit being radiated or dissipated faster than it is supplied.

sponding or exactly syntonised receiver at the same or a greater distance. This claim is based on the known fact that for certain types of receiving circuit, the current created in them can be largely increased by increasing the number of oscillations in the incident train of waves, so that if oscillations or waves are undamped they can make up for feebleness by their persistency. This, however, depends essentially upon the nature of the receiving circuit, and is only true within certain limits.

When electric waves radiated from one antenna fall on another syntonised or tuned secondary circuit they set up oscillations in the latter of the same period. It might be thought that if these impinging waves are undamped, we should have an infinitely large current produced in the secondary circuit. As a matter of fact we do not. The electro-motive impulses from the sender only increase the secondary current up to a certain point. The secondary circuit necessarily possesses resistance and other sources of energy dissipation which rapidly increase with the current induced in it. Moreover, when the secondary circuit has an antenna attached, this itself radiates part of the energy it absorbs. Hence it follows that beyond a certain point the energy thrown on to the secondary circuit is no longer utilised to increase the current in it, but only just suffices to maintain it. The case is exactly analogous to that of a body being warmed by radiant heat. A thermometer exposed to full sunshine only rises to a certain height.

A comparison between the damped and undamped radiation, to be valid, must be made as follows: Assume that we have two wireless transmitting stations side by side, one sending out intermittent trains of feebly damped oscillations, the other continuous trains of undamped oscillations, and let them be so adjusted that the transmitters take the same mean power to work them. Let the frequency of these damped and undamped waves so radiated be the same. At a distance let there be a suitable movable receiving station, say a ship, with receiver tuned to the same frequency. Then the principal question at issue is, whether the undamped waves can affect this receiver at a greater distance than the damped waves of the same integral energy. Otherwise, at the same distance can the undamped wave station affect the receiver when using less power than the damped wave station. Since, however, by assumption the undamped waves from one station have the same integral energy as the damped waves from the other, the latter will have a higher initial value in each train to compensate for their decreased value and intermittent cessation. Hence we may ask another question, viz., What will be their relative effect on receiving stations in their neighbourhood not quite in tune with the emitted waves? Can we bring the undamped waves nearer into tune with these outlander stations without disturbing the latter, than we can in the case of the damped waves, and if so within what ratio of wave length? Claims have been made for a great superiority in this

respect in the case of undamped waves, but we are still awaiting quantitative confirmation. Amongst other assertions it has been stated that the undamped waves are less easily "tapped" to use the newspaper expression. This is a fallacy. With the proper experimental appliances a receiving circuit can be gradually adjusted to any electrical frequency, and when it comes to the right frequency it must be affected just as much as true receiving stations for which the waves are intended. It is all a matter of apparatus and skill. To illustrate the first point, viz., the effect of the nature of the receiving circuit we may take an instance from optics. When we look through a telescope at the stars we can see a certain number down to some limiting magnitude. No amount of prolonged gazing when using the eye as a wave receiver increases the effect produced by a just invisible star. If, however, we use a photographic plate the effect on it is cumulative, and we can by a sufficiently long exposure obtain impressions of invisible stars in countless numbers. The photographic film is a wave detector of quite a different kind to the retina. In the case of the film it can make up by time what is wanting in intensity in the wave motion. In the case of wireless telegraphy it is clear therefore that the nature of the receiver has a great deal to do with the possible advantages of undamped waves, and it is not merely a question of the tuning or the transmitter.* Again the ordinary 10-inch induction coil and spark transmitter as used on ships takes up $\frac{1}{5}$ of a horse-power when in full work, and can send wireless messages 200 miles or more when an appropriate receiver is used. I find it very difficult if not impossible to obtain sufficiently high frequency oscillations by the arc method unless at least 1 or $1\frac{1}{3}$ horse-power is being expended in the arc. Hence for short distance work on the point of economical working as well as simplicity of apparatus and ease of working the spark method has advantages denied to the arc. We were told not long ago by an eminent electrician that the arc method of creating undamped waves sounded the death knell of spark telegraphy. It is always advisable to exercise some caution in issuing obituary notices of well tried inventions prior to their actual decease, and in this case although the power to create

* In order that he may take the utmost advantage of the principle of resonance, Mr. Poulsen uses in the receiver a device he calls a "ticker." This serves to keep the condenser-inductance circuit of the receiver closed, until resonance has exalted the oscillations to the utmost. The ticker then opens it at intervals and inserts the particular oscillation detector, whether electrolytic or other, which makes the audible or visible signal. In his syntonic receiver Mr. Marconi has always adopted a similar plan for he keeps the coherer terminals joined by a condenser which closes the secondary circuit of the receiving jigger. A point of interest not yet considered is whether we *do* need absolutely undamped waves to gain all the possible practical advantages derivable from them. It may be that very slightly damped trains containing say 50 oscillations per train and following each other several hundred times per second will with an appropriate receiver give us all that we can obtain from the use of forced undamped waves.

continuous trains of electric waves will doubtless greatly assist space telegraphy, it does not follow that their generation by the arc method is the best or final method.

In the production of continuous oscillations we are not limited to the arc method. Mr. Marconi has for some time past been engaged in developing an ingenious method of creating undamped electric waves for telegraphic purposes which involves neither an arc nor alternator, but is a new mechanical method of great simplicity.

This method is capable of producing astonishingly large alternating currents of very high frequency, in other words, so called undamped or persistent oscillations. I have recently witnessed some of his experiments, and was surprised at the results obtained. Long distances have been telegraphically covered with every prospect of great efficiency. Unfortunately, the incomplete state of certain

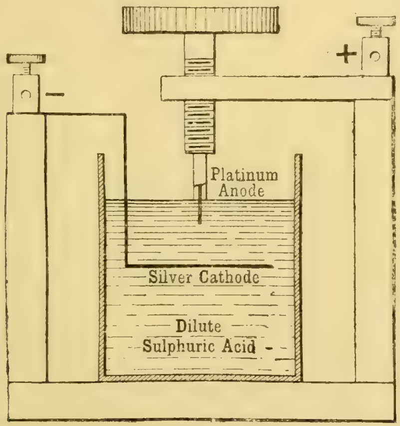


FIG. 12.—ELECTROLYTIC DETECTOR.

foreign patents prevents me from entering into details of this method now, but I hope he himself will be able to do so soon. Turning then from transmitters to receivers, we may notice one or two recent types. By far the larger portion of electric wave telegraphy was until a few years ago conducted by means of some form of coherer, either requiring tapping or else self-restoring. The coherer in certain forms has the advantage that a current of about 0·1 to 1·0 milliampere can be passed through it, and hence through a relay, so that messages can be printed down by it when using a Morse inker in dot and dash signals. After that came Mr. Marconi's magnetic detector, making use of a telephone to create an audible signal. This is now the instrument employed by him on all long distance work. In Germany and the United States a type of telegraphic wave detector

has come into use, commonly called the electrolytic receiver. In one form it was invented in the United States by Fessenden, and called by him a liquid barretter. It was independently discovered, and described shortly afterwards in Germany by W. Schloemilch, and is generally there called the electrolytic detector (see Fig. 12). It consists of an electrolytic cell or vessel containing some electrolyte, usually nitric acid. In it are placed two electrodes, one a metal or carbon plate of large surface, and the other an extremely fine platinum wire prepared by the Wollaston process, a very short length of which is immersed in the liquid. A convenient plan is to prepare a Wollaston wire of silver, having a core of platinum which is drawn down until the latter is only 1/1000th of a millimetre in diameter. If the electrolyte is strong nitric acid, then when the above wire is immersed to the depth of a millimetre the acid dissolves off the

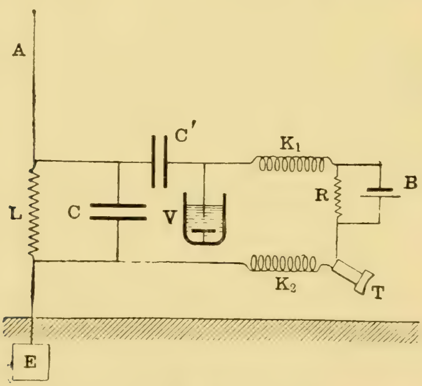


FIG. 12A.

ELECTROLYTIC DETECTOR WITH SHUNTED CELL AND TELEPHONE.

silver and leaves the fine platinum wire exposed as an electrode. This cell has its two electrodes connected respectively to a receiving antenna, and an earth plate, and also to a circuit containing a shunted voltaic cell and a telephone (see Fig. 12A). The voltaic cell sends a current through the electrolyte in such a direction as to make the fine wire the positive electrode or anode. Some dispute has taken place whether the cell will work when the fine wire is the negative electrode. Fessenden, who adopts a thermal theory of the cell, claims with Rothmund and Lessing that it is equally sensitive, whether the small electrode is positive or negative.

According to one theory, the action of the cell as a wave detector depends on the power of the oscillations to remove the so-called polarisation of the electrodes or adhering films of ions. According to another theory it is due to the heating action of the oscillations on

the small electrode and liquid in its neighbourhood. In any case, the action is just as if the resistance of the electrolytic cell were suddenly changed, either increased or decreased. It has also been found by Rothmund and Lessing that the cell may be made to supply its own electromotive force. If we form a simple polarisable voltaic cell with fine zinc and platinum wires immersed in dilute acid and connect a telephone or high resistance galvanometer to these elements; then, when electric oscillations pass through the cell, the current sent by it through the telephone or galvanometer is momentarily increased. That the action is not altogether due to the removal of polarisation films is shown by the fact that the fine platinum wire in the Schloemilch form of detector wears away or is dissolved in the nitric

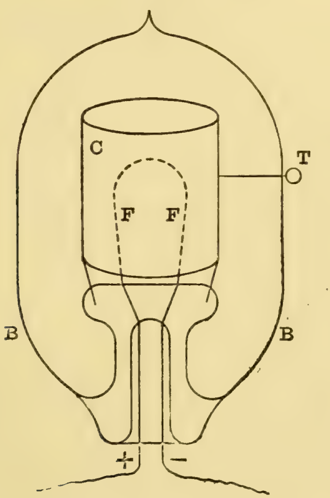


FIG. 13.—OSCILLATION VALVE (FLEMING).

acid when oscillations are passed for some time through the cell, and there is some evidence that gold and platinum can be made to dissolve even in dilute acids by the action of electric oscillations.

In 1904 I was so fortunate as to discover another and quite different principle on which a sensitive electric wave detector can be based. If a carbon filament glow lamp has a metal plate carried on a third terminal sealed into the bulb, it is well known that a current of negative electricity flows from the plate to the positive terminal of the lamp, when the filament is rendered incandescent by a continuous current. This is the so-called Edison effect. It is also now known that incandescent bodies discharge negative corpuscles or electrons from their surface, and incandescent carbon, when in a vacuum,

exhibits this power in a marked degree. Negative electricity escapes freely from it, but not positive. In 1904 I was endeavouring to find some way of rectifying electrical oscillations, that is, of separating out the two sets of alternate currents and making them separately detectable by an ordinary galvanometer. It occurred to me to make use of a carbon filament lamp, having a metal cylinder insulated in the bulb surrounding the filament, the cylinder being connected to a platinum wire sealed through the bulb (see Fig. 13). This lamp was then used as follows. A circuit was connected between the terminal of the metal plate and the negative terminal of the filament, the latter being made brightly incandescent by a small battery. In this circuit a galvanometer and one circuit of a small transformer or induction coil

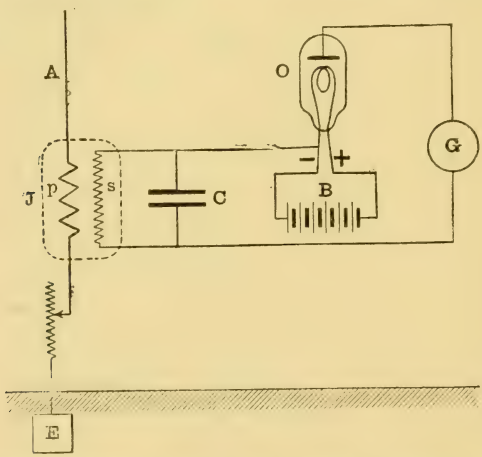


Fig. 14.—Oscillation Valve or Glow Lamp Detector, used as a Receiver in Electric Wave Telegraphy.

was inserted. On connecting the other circuit of the transformer between an antenna and the earth, I found that oscillations set up in the antenna caused a deflection in the ordinary mirror-galvanometer (see Fig. 14). The action is as follows. The antenna oscillations induce others in the circuit of the transformer, which is in connection with the lamp. A movement of electricity in this circuit, which consists in the flow of negative electricity from the filament to the plate through the vacuum, can take place, since this negative electricity is, so to speak, carried across the vacuous space by the electrons emitted from the hot carbon. On the other hand, negative electricity cannot flow in the opposite direction. Hence the glow-lamp separates out the two oppositely directed movements of electricity and allows only one to pass. I therefore called the appliance an oscillation valve. This

instrument was shown by me to the Royal Society early in February 1905, and was employed by Mr. Marconi soon after as a long-distance wireless-telegraph receiver, in conjunction with other improvements. M. Tissot, of the Naval College, Brest, in France, has made use of this glow-lamp detector, and with a sensitive galvanometer has received signals at a distance of 50 kilometres.* Employing a special form of transformer, and a telephone in place of a galvanometer, Mr. Marconi has used it for some time past over distances of 200 miles or more, and finds it a very sensitive form of receiver. Since this particular form of electric wave detector was brought to notice by me, Dr. Wehnelt has found that a metallic wire, coated with oxides of calcium, barium, or other earthy metals, may be substituted for the carbon filament in the vacuous bulb.

The oscillation valve is capable of giving very remarkable effects when used as a receiver with a transmitter producing undamped waves. The reason for this is obvious. The valve passes all the unidirectional currents in the attached secondary circuit. If, then, these are intermittent damped trains, say having a frequency of 100,000, and 50 trains of 20 oscillations per second, the total time during which electric current is passing is only one-thousandth of the whole time. Accordingly, if we, so to speak, fill up the gaps between the trains of oscillations with other oscillations, and generate a continuous train, we greatly increase the quantity of electricity passing and re-passing any point in the secondary circuit, and the indications on a galvanometer in circuit with the valve are enormously increased. A true comparison between the two cases of damped and undamped waves involves many factors, and is not fair unless we compare together transmitters taking the same mean power. Generally speaking, however, we may say that not only this glow-lamp detector, but all forms of thermal detector, give greatly increased effects when employing undamped oscillations. I find, for instance, that if undamped oscillations are created in a closed wire circuit which forms part of a circuit containing capacity and inductance shunted across a Poulsen arc, I can induce powerful secondary oscillations in a similar closed and syntonic secondary circuit at a considerable distance, and detect these by the use of my oscillation valve and a galvanometer placed. In fact the use of undamped oscillations in a closed primary circuit, and this oscillation valve used with a telephone in a closed secondary circuit, brings to the front again the possibility of making use of so-called wireless telegraphy by electro-magnetic induction over very large distances. The old form of electro-magnetic induction telegraphy as practised by Trowbridge, Preece, Lodge and others made use of low-frequency alternating currents (50 to 100) in a closed primary circuit, and

* See *The Electrician*, vol. lviii., p. 730, Feb. 22, 1907. M. C. Tissot, "On Ionised Gas Electric Wave Detectors."

employed a telephone in a distant closed secondary circuit to detect the magnetic field so produced, signals being made by interrupting the primary current. I have, however, found a means of greatly improving this form of wireless telegraphy. In a closed primary circuit I establish continuous undamped oscillations of, say a quarter of a million frequency by the arc method. At a distance I place a syntonic secondary circuit containing my oscillation valve as a detector, a telephone being used with it connected between the middle plate and negative filament terminal. Both the primary circuit and secondary circuit are connected to earth at some point. The signals are made by breaking and making the earth connection of the transmitter in accordance with Morse code. When the earth connection is made at both ends a sound is heard in the telephone, but not when it is broken. This seems to depend upon the fact that the oscillations produced by the arc method are not absolutely continuous, but cut up into groups, as already proved by the experiment with the rapidly-moving neon tube and helix.

I have found that it is not necessary to employ a high voltage carbon filament, a small lamp with 4-volt filament, taking about one ampere, works quite as well as a wireless telegraph receiver as a 12 or 100 volt lamp. The filament has, however, to be at a certain critical temperature to obtain the best result; the vacuum also has to be extremely good. There are, no doubt, many possible variations of the above-mentioned type of oscillation valve wave detector. Every glass vessel containing rarefied gases or mercury vapour having electrodes of different sizes or shapes or temperatures, has some degree of unilateral conductivity, and can be used in the above manner to separate out the two constituent currents of an electrical oscillation, and make them detectable by an ordinary galvanometer or telephone. I have also tried with some success a flame in which two platinum wires are immersed, one of which carries a bead of potassium sulphate as a means of rectifying oscillations of high frequency. It is well known that negative ions are then liberated in the flame, and negative electricity can pass over more freely from the electrode which carries the bead of salt to the other than in the opposite direction. I have not, however, found anything as simple and useful as the above-described low-voltage carbon filament glow lamp. Moreover, other inventors have endorsed its utility by granting it the compliment of imitation. In October 1906, Dr. de Forest described to the American Institute of Electrical Engineers an appliance he called an "audion," which is merely a replica of my oscillation valve, described to the Royal Society eighteen months previously and to the Physical Society of London six months before, particularly with reference to its use as a wireless telegraph receiver. Apart from the name the only difference introduced by him was to substitute a telephone and battery in series connected between the middle plate and positive terminal of the filament, for the

galvanometer used by me connected between the middle plate and the negative terminal. As Mr. Marconi had before that time used my oscillation valve with a telephone with it for long distance work, and M. Tissot has found a galvanometer, used as I described it, effective up to 50 kilometres, the modification made by Dr. de Forest does not make any fundamental difference in the operation of the device as a wave detector.*

Very closely connected with the question of the production of continuous or undamped electric waves is that of the electrical transmission of speech through space without wires ; in other words, wireless telephony. Some considerable progress has already been made in this direction. Any complete treatment would require a lecture in itself. If, however, we pass by the investigations of Bell with the photophone, Simon, Ruhmer, and others with apparatus employing the resistance variation of selenium by projected beams of powerful light, and also those of Preece, Gavey, and others with electro-magnetic induction, we may say that at the present time the chief interest attaches to methods of wireless telephony which involve the use of undamped electric waves. The problem may then be stated to be as follows : Articulate speech made against a diaphragm at a transmitting station has to affect similarly the diaphragm of a telephone at a receiving station not connected with it by wire.

Time only permits me to give you a brief sketch of some interesting experiments which have been carried out lately by the German Wireless Telegraph Company between Berlin and their large station at Nauen, 20 miles distant. At the transmitting station they employ 12 electric arcs in series, each of which is composed of a carbon negative, and a water-cooled copper positive electrode. These arcs take 4 amperes at 440 volts (see Fig. 14). In parallel with this series of arcs is joined a condenser and inductance, to which is inductively but loosely coupled an antenna from which undamped electric waves, 800 metres in wave length, are radiated, having a frequency therefore of 400,000. The oscillations set up in this antenna can be more or less enfeebled by shunting them to earth through a microphone transmitter, the resistance of which is varied by the act of speaking against it. Hence, although the wave length of the emitted electric waves is not altered, their intensity is modulated in accordance with the wave form of the sounds impressed on the transmitter diaphragm. At the receiving station there is a receiving antenna tuned to the wave length used, having a quantitative electrolytic detector in connection with a telephone coupled inductively to the antenna circuit. Hence the vibrations of the transmitter diaphragm vary the intensity of the radiated electric waves but not their wave length.

* In a private letter M. C. Tissot has already acknowledged gracefully my priority of invention in this matter, although he himself was independently working in the same direction.

These waves travel through space, fall on the receiving antenna and affect the resistance of the electrolytic detector in proportion to their intensity. Hence the receiving telephone repeats the sounds or articulations made against the transmitting microphone and reproduces speech. The German experimentalists say that a satisfactory wireless transmission of speech can be made in this manner, 20 kilometres or 12 miles over water with antennæ 25 metres or about 80 feet high.

Ruhmer has recently described in the *Elektrotechnische Zeitschrift* some similar experiments made with a 220 volt Poulsen arc. In this case the necessary modulation was impressed upon the radiated electric waves by inserting the primary circuit of an induction coil in the continuous current arc circuit, and closing its secondary

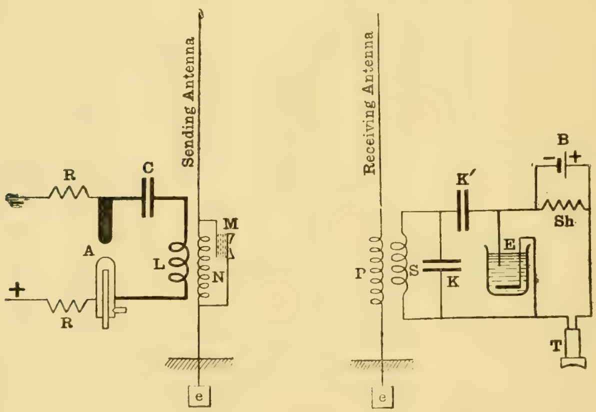


Fig. 15.—Wireless Telephony by Electric Waves.

through a microphone transmitter and working battery. The receiving arrangement involved an electrolytic receiver as just described. Professor Fessenden has recently described very similar arrangements for electric wave wireless telephony.* We can, however, say that something more than a beginning has been made in the art of the wireless transmission of human speech to a distance. The energy expenditure is at present considerable, and much will have to be done before telephony without wires can be looked upon as coming within the range of commercial work. Nevertheless, having regard to the enormous improvements in wireless telegraphy in the last seven years it is quite within the bounds of possibility we may soon be able to speak across the English Channel without a wire, and not scientifically impossible for the sounds of the human

* See *The Electrician*, vol. lviii. p. 710, 1907.

voice to be some day transmitted from the shores of England or the United States to an Atlantic liner in mid ocean.

We may consider in the next place another problem of great practical importance, towards the solution of which some considerable progress has been made, viz. that of locating the direction of the sending station, and giving direction to the emitted radiation sent out from it. The early attempts to do this depended upon the use of parabolic mirrors, or some arrangement of vertical rods equivalent to it. But although comparatively short electric waves of a few feet in wave length can be directed in this manner in the form of a beam, it is out of the question for electric waves hundreds of feet in length, because reflection can only take place when the dimension of the mirror are at least comparable with that of the wave length.

The ordinary vertical antenna, of course, radiates equally in all directions, and when it is so far off as to be below the horizon a corresponding receiving antenna may respond to it, but cannot locate the position of the sending station.

It seems to have been noticed by several persons that if the

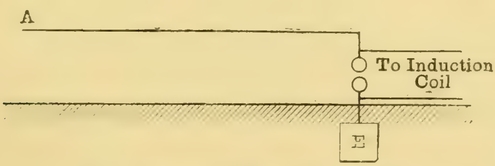


Fig. 16.—Marconi Bent Antenna.

antenna is not vertical, it radiates rather more in one direction than another, and the same for a non-vertical receiving antenna. It is more receptive to waves coming from one direction than another. Various observations on the operation of non-vertical, looped, or duplex antennæ have from time to time been made by Zenneck, Sigsfeld, Strecker, Slaby, and de Forest, whilst methods for locating the sending station or directing the transmitted waves were described in patent specifications by de Forest, Garcia, and Stone. Although claims were made for arrangements said to be effective, these various researches were not pressed to such logical issue as to disclose any definite general scientific principle, whilst in some cases the results said to have been obtained are clearly in contradiction to well ascertained facts.

Time will not permit further reference to these early and inconclusive observations.

In March last year Mr. Marconi communicated to the Royal Society a paper on the radiation from an antenna having a short part of its length vertical and the greater part horizontal, and on the receptive powers of a similar antenna in various azimuths (see Fig. 16).

He found that such a bent antenna emits a less intense radiation at any given distance in the direction in which the free end points, than in the opposite direction. Also, since the law of exchanges holds good for electric radiators, a similar form of antenna receives or absorbs best electric waves which reach it from a direction opposite to that to which the free end points.* Hence two similar bent antennæ, when set up back to back, that is, with their free ends pointing away from each other, form a system of radiator and receiver which has greater range in that position than in any other for the same distance, and hence has directive qualities not possessed by the ordinary vertical antennæ.

Although I have given the mathematical explanation of the reasons for this in another place,† it is not difficult to translate the common

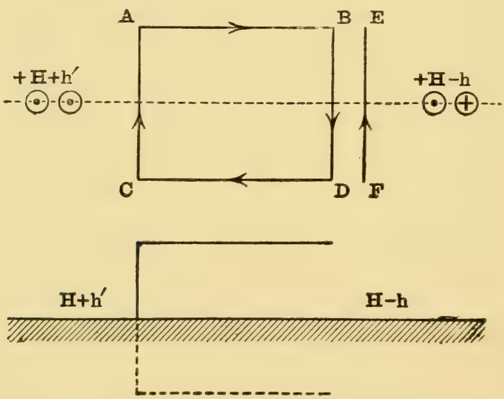


Fig. 17.—Theory of Marconi Bent Antenna.

sense of it into non-symbolic language. Imagine a square circuit of wire half buried vertically in the earth (see Fig. 17). Let a current be supposed to flow round it, in clockwise direction. Then it creates a magnetic field, the direction of which along the surface of the earth in a direction at right angles to the plane of the circuit, and at equal distances from the centre, is towards the spectator on both sides. Suppose, then, that a wire equal in length to one side of the square is placed in contiguity to one vertical side, and that it carries a current

* This is an extension to electric radiation of the principle known as Prevost's Theory of Exchanges, as amplified by Balfour Stewart and Kirchhoff, which forms the basis of spectrum analysis laid down by Stokes, Kirchhoff, Bunsen, and others.

† See "A Note on the Theory of Directive Antennæ," Proy. Roy. Soc. Lond., vol. lxxviiiA., 1906, p. 1.

opposite in direction to that in the side of the square (say, the right-hand side) to which it is in proximity. Then the magnetic field of this straight current is from the spectator at the right-hand side, and to the spectator on the left-hand side. Accordingly, the total field on the right-hand side, due to the currents in the closed and open circuits together, is less than that on the left, because the individual fields are added on one side and subtracted on the other. Since the two oppositely directed currents in the adjacent wires may be imagined to come so close as to annul each other, and since the parts of the remainder below ground may be considered to be removed without affecting the field above ground, we arrive at the conclusion that an antenna partly vertical and partly

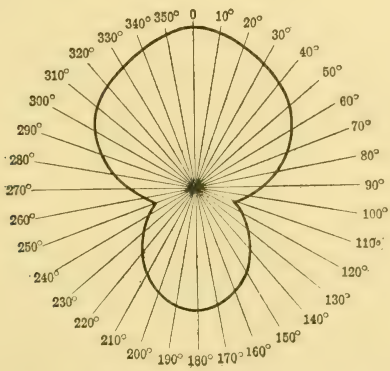


FIG. 18.
RADIATION IN VARIOUS AZIMUTHS FROM MARCONI BENT ANTENNA.

horizontal radiates most strongly in the direction opposite to that in which the free end points.

Mr. Marconi discovered this fact experimentally, and made measurements of the currents induced in receiving antennæ placed at equal distances round this bent transmitter, and plotted the results in the form of a polar curve (see Fig. 18). As a quantitative receiving detector he made use of a Duddell's thermal ammeter. In repeating and confirming these experiments on a smaller scale last summer in the grass quadrangle of University College, I employed a form of thermal ammeter of my own design, made as follows: A vacuum vessel made like those which Sir James Dewar devised for storing liquid gases has four platinum wires sealed through the bottom of the inner test tube. One pair of these is connected in the vacuous space by an extremely fine constantin wire and the other pair by a fine tellurium-bismuth thermo-junction, with the junction resting on the

fine wire (see Fig. 19). When a galvanometer of suitable resistance is connected to the terminals of the thermo-junction and the constantin wire inserted in the circuit of the receiving antenna we have an arrangement which enables us to measure as well as detect the intensity of the electric waves incident on the antenna. This detector, skilfully made by my assistant, Mr. Dyke, proved very useful. I was thus able to confirm Mr. Marconi's observations and my own theory of them, and furthermore noticed that when the non-vertical part of the transmitting antenna was bent so that it was not horizontal but pointed downwards, a very remarkable non-symmetry of radiation occurred, quite, however, accounted for by theory (see Fig. 20). Mr. Marconi has made very effective practical use of the bent receiving antenna to locate the position of a ship or station

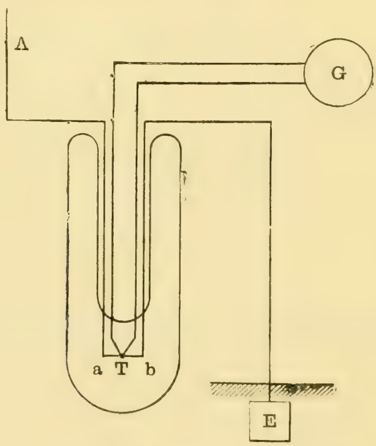


FIG. 19.—THERMAL DETECTOR.

sending out electric-wave messages when so far off as to be below the horizon.

In this case he arranges the receiving antenna so that a very short part is vertical and the greater part horizontal, and furthermore permits the horizontal part to be swivelled round the vertical part as a centre. In the vertical portion he places his magnetic or some other detector. If then there be a distant station in correspondence with this receiver, the direction in which the transmitter lies can be determined within a few degrees by swivelling round the receiving antenna and noting the position in which it picks up signals or picks them up best from this transmitter. The transmitter then lies in the direction opposite to that in which the free end of the receiver wire points. If it is not convenient to swivel round the horizontal portion,

then Marconi arranges a number of horizontal receiving antennæ like the spokes of a wheel, all having a common shorter part vertical as

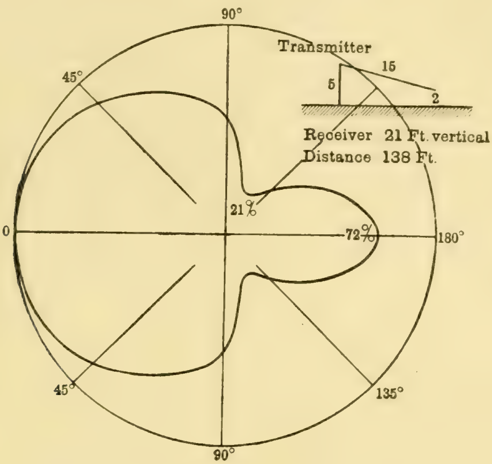


FIG. 20.

their centre (see Fig. 21). In the vertical part a magnetic detector is inserted, and by means of a switch any one of the horizontal radial

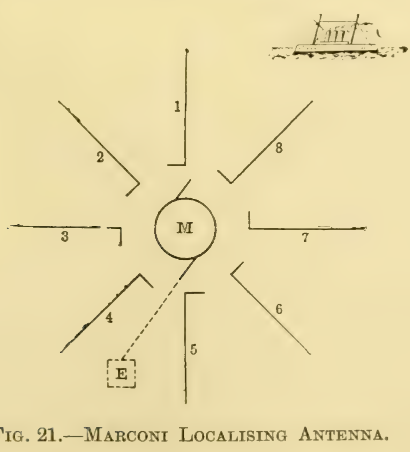


FIG. 21.—MARCONI LOCALISING ANTENNA.

antennæ can be put in connection with it. By finding which radial gives the strongest signals, the direction of the sending station is

easily located. It will be seen, therefore, that two well defined principles had been arrived at by Marconi. First, that the non-symmetry of the radiation and reception depends upon the employment of antennæ having their horizontal portions large compared with the vertical, and secondly, that the maximum radiation is in the direction opposite to that in which the free end of the horizontal part points. These observed effects rest on a sound scientific basis, and, as I have shown, are immediately derivable from first principles.

Previously to Marconi's experiments no definite guiding principles as to directive telegraphy had been published, but a number of unconnected observations made, not always correctly interpreted or even described, and in any case with limited application.

Meanwhile, however, Professor F. Braun, of Strassburg, had been engaged on a different plan for directing the radiation from antennæ. Briefly stated, his method is as follows : He erects three vertical antennæ at the corners of an equilateral triangle, or four at the corners of a square, the sides of which are about equal to the height of the antennæ, and he creates in them electrical oscillations which have a defined and constant difference of phase by methods contrived by him, Drs. Papalini and Mandelstam, not yet fully described. It is found that the waves sent off from these three antennæ interfere with each other in an optical sense, exalting each other in some directions and nullifying each other in other directions, in accordance with their relative amplitude and phase difference. The resultant effect can be so arranged that the radiation is extremely unsymmetrical, being much more towards one side than the other. The intensity in various azimuths may be represented by the *radii vectores* of a sort of oval or heart-shaped curve, the triple transmitter occupying a position on the cusp or apex of the curve (see Fig. 22). It will be seen, therefore, that popular notions on the subject of directive telegraphy are wide of the mark. Whilst we cannot yet project a narrow beam of long-wave electric radiation in any required direction, or focus it entirely on a given receiving station at a great distance, much can be done to prevent radiation being sent out from transmitters in directions in which it is of no use or not desired.

At Coast Stations communicating with ships at sea something has already been done to achieve this result. Mr. Marconi has for some time past employed such directive antennæ at his large power stations at Poldhu and elsewhere.

These, then, are a few of the contributions which have recently been made by practicians and theorists to this fascinating and progressive subject. But, whilst we may congratulate ourselves that progress continues to be made, there are still large districts of it in which our knowledge is most incomplete. One matter having a very practical bearing is the necessity for systematic study of the causes which vary the transparency of space to long electric waves. You will continually see references in the daily papers to isolated feats of

communication between ship and ship, or ship and shore, over unusually large distances. Ships equipped with what is called short-distance apparatus, that is intended to send and receive over 200 miles or so, are able occasionally to communicate with others 600, 800, or even 1000 miles away. This is not altogether a matter of personal skill or of apparatus. Our terrestrial atmosphere varies from day to day and hour to hour in its transparency to long telegraphic electric waves, just as it does to the short light waves. One reason, and probably a valid one, which has been advanced for this is the ionisation of the atmosphere by sunlight, radio-active matter, or matter electrically charged reaching our earth from the sun or cosmical space. These ions or electrically charged particles suspended in the air are set in motion by the electric force of long electric waves passing through

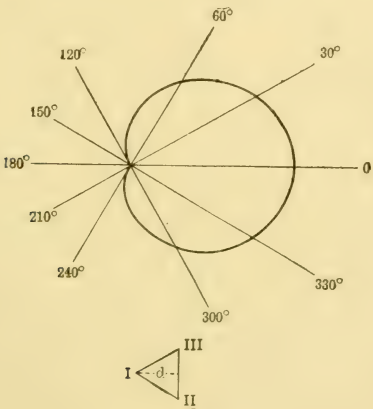


FIG. 22.
POLAR DIAGRAM FOR BRAUN'S TRIPLE DIRECTIVE ANTENNA.

the region. This, however, involves energy which must be taken from the wave, and hence the wave passes on so much the weaker. This effect is altogether different from the disturbing effects of atmospheric electricity on the receiving antenna. As first noticed by Mr. Marconi on one of his Atlantic voyages, the atmospheric transparency for long electric waves is decreased by daylight and this reducing effect of light on the wave energy takes place chiefly near the transmitting antenna where the electric force is largest. It fluctuates from hour to hour and month to month according to laws as yet undetermined, and has no doubt secular and irregular fluctuations superposed on its regular variations. The subject of long distance wireless telegraphy is yet too young to provide observations for any safe

generalisations on this matter, but doubtless these will be accumulated in course of time.

Wireless telegraphy has now reached a position of such importance, especially in connection with super-marine communication, that scientific research for its advancement should have the utmost possible encouragement, subject, of course, to the consideration that there is only one æther for us all. Whilst we derive satisfaction from the thought that so much valuable discovery and invention has already rewarded the labours of workers in many lands, we have but to glance around us to see in all directions, in connection with it, unsolved problems, untrodden paths, wide fields of knowledge ripe for harvest in which the sickle of the reaper has never yet been moved.

[J.A.F.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Commander G. R. Bethell, late R.N.
Dr. Hans Goldschmidt,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Oswald Lewis for his Donation of Five Guineas to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Chairman reported the decease of Sir Benjamin Baker, K.C.B. K.C.M.G. F.R.S., a Vice-President and Manager, on the 19th of May, and the following Resolution of condolence passed by the Managers at their Meeting held this day was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss Engineering Science and the Institution has sustained in the decease of Sir Benjamin Baker, K.C.B. K.C.M.G. LL.D. D.Sc. F.R.S., past President of the Institution of Civil Engineers.

Sir Benjamin Baker was elected a Member of the Royal Institution in 1882. He took an active interest in the welfare of the Institution, and on former occasions both as Vice-President and Manager rendered invaluable service to the Institution by contributing to the Research Fund.

He delivered Friday Evening Discourses on the two great engineering works completed under his direction, namely: "Bridging the Firth of Forth," on May 20, 1887; and "The Nile Dams and Reservoirs," on June 6, 1902, when H.R.H. The Prince of Wales, K.G., the Vice-Patron, presided.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of their most sincere sympathy and heartfelt condolence with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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Nova Scotian Institute of Science—Proceedings, Vol. XI. Part 2. 8vo. 1906.
Odontological Society—Transactions, Vol. XXXIX. No. 6. 8vo. 1907.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for April, 1907. 4to.
Pharmaceutical Society of Great Britain—Journal for May, 1907. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LVIII. Part III. 8vo. 1907.
Photographic Society, Royal—Journal, Vol. XLVII. No. 5. 8vo. 1907.
Pratt, Edwin A., Esq. (the Author)—The Licensed Trade. 8vo. 1907.
Robinson, V. J., Esq., C.I.E. M.R.I. (the Author)—Ancient Furniture. 4to. 1902.
Rome, Ministry of Public Works—Giornale for Feb.-March, 1907. 8vo.
Royal Engineers' Institute—Journal, Vol. V. No. 6. 8vo. 1907.
Royal Society of Edinburgh—Proceedings, Vol. XXVII. No. 1. 8vo. 1907.
Royal Society of London—Philosophical Transactions, A, Vol. CCVII. Nos. 417–418; B, Vol. CXCIX. No. 253. 4to. 1907.
Proceedings, Vol. LXXIX. Series A, No. 529; Series B, No. 531. 8vo. 1907.
S. Paulo, Commissao Geographica—Exploraçao do Rio Tieté. 4to. 1906.
St. Pétersbourg, Imperial Academy of Sciences—Bulletin, 1907, Nos. 8–9. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXVIII. No. 5. 8vo. 1907.
Selborne Society—Nature Notes for May, 1907. 8vo.
Smithsonian Institution—Contributions to Knowledge, No. 1694. 4to. 1907.
Societa degli Spettroscopisti Italiani—Memorie, Vol. XXXVI. Disp. 4. 4to. 1907.
Society of Arts—Journal for May, 1907. 8vo.
South Australian School of Mines—Report for 1906. 8vo. 1907.
Transvaal Department of Agriculture—Journal for April, 1907. 8vo.
Annual Report, 1905–6. 8vo. 1907.
Turner, Professor H. H., M.A. D.Sc. F.R.S. M.R.I.—Oxford Astrographic Cataloguè, Vol. II. 4to. 1906.
United Service Institution, Royal—Journal for May, 1907. 8vo.
Index to Journal, Vols. XXXI.-L. 3vo. 1907.

United States Department of Agriculture—Experiment Station Record, Vol. XVIII. No. 8. 8vo. 1907.

Monthly Weather Review for Jan. 1907. 4to.

United States Department of the Interior—Geological Survey: Bulletin, Nos. 279, 286, 297, 303, 305–307, 310. 8vo. 1907.

Water Supply Papers, Nos. 182, 183, 187, 188, 189. 8vo. 1907.

Mineral Resources, 1905. 8vo. 1906.

Monograph L. 4to. 1906.

United States, Library of Congress—Naval Records of the American Revolution, 1775–1788. 8vo. 1906.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1907, Heft 5. 4to.

Western Australia, Agent-General—Supplement to Government Gazette for March, 1907. 4to.

Monthly Statistical Abstract for March, 1907. 4to.

GENERAL MONTHLY MEETING,

Monday, July 1, 1907.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

George Jamieson, Esq., C.M.G.
Samuel Shore Nightingale, Esq., M.R.C.S.
William Stone, Esq., M.A.
Herbert Skyring Stoneham, Esq.
Eric Turk, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Agricultural Journal of India, Vol. II. Part 2. 8vo. 1907.
Geological Survey: Records, Vol. XXXV. Part 1–2. 8vo. 1907.
Supplement to Report on Public Instruction in Bengal, 1905–6. 4to. 1907.
Palæontologia Indica: Series XV. Vol. V. No. 2. 4to. 1907.
Report of the Board of Scientific Advice, 1905–6. 8vo. 1907.
Memoirs of the Department of Agriculture in India, Botanical Series, Vol. II. No. 1. 8vo. 1907.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Mathematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVI. 1° Semestre, Fasc 10–11. 8vo. 1907. Classe di Scienze Morali, Serie Quinta, Vol. XVI. Fasc. 1–3. 8vo. 1907.
American Geographical Society—Bulletin, Vol. XXXIX. No. 6. 8vo. 1907.
Astronomer Royal—Report of the Board of Visitors, 1907. 4to.
Astronomical Society, Royal—Monthly Notices, Vol. LXVII. No. 7. 8vo. 1907.
Automobile Club, Royal—Journal for June, 1907.
Belgium, Royal Academy of Sciences—Bulletin, 1907, Nos. 2–4. 8vo.
Bevan, Rev. J. O., M.A. M.R.I. (the Author)—An Archæological Survey of Herefordshire, Mediæval Period. 4to. 1897.
Boston Public Library—Monthly Bulletin for June, 1907. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIV. Nos. 15–16. 4to. 1907.
British Astronomical Association—Journal, Vol. XVII. No. 8. 8vo. 1907.
Cambridge Philosophical Society—Proceedings, Vol. XIV. Part 2. 8vo. 1907.
Chemical Industry, Society of—Journal, Vol. XXVI. Nos. 10–11. 8vo. 1907.
Chemical Society—Proceedings, Vol. XXIII. Nos. 327–328. 8vo. 1907.
Journal for June, 1907. 8vo.
Chicago University—Decennial Publications—The Interpretation of Italy. By C. von Klenze. 8vo. 1907.
Cornwall, Royal Institution of—Journal, Vol. XVII. Part 1. 8vo. 1907.
Cracovie, Academy of Sciences—Bulletin, 1907. Classe des Sciences, Mathématiques, Nos. 1–3. Classe de Philologie, Nos. 1–2. 8vo. 1907.

Dance, H. A., Esq.—Dante's Inferno, Purgatorio and Paradiso, in Modern Greek. 8vo. 1890.
Dax, Société de Borda—Bulletin, 1907, No. 1. 8vo. 1907.
Editors—American Journal of Science for June, 1907. 8vo.
Analyst for June, 1907. 8vo.
Athenæum for June, 1907. 4to.
British Homœopathic Review for July, 1907. 8vo.
Chemical News for June, 1907. 4to.
Chemist and Druggist for June, 1907. 8vo.
Dioptric Review for June, 1907. 8vo.
Dyer and Calico Printer for June, 1907. 4to.
Electrical Contractor for June, 1907. 8vo.
Electrical Engineer for June, 1907. 4to.
Electrical Engineering for June, 1907. 4to.
Electrical Review for June, 1907. 4to.
Electrical Times for June, 1907. 4to.
Electricity for June, 1907. 8vo.
Engineer for June, 1907. fol.
Engineer-in-Charge for June–July, 1907. 8vo.
Engineering for June, 1907. fol.
Horological Journal for June, 1907. 8vo.
Journal of the British Dental Association for June, 1907. 8vo.
Journal of State Medicine for June, 1907. 8vo.
Law Journal for June, 1907. 4to
London University Gazette for June, 1907. 4to.
Model Engineer for June, 1907. 8vo.
Motor Car Journal for June, 1907. 8vo.
Musical Times for June, 1907. 8vo.
Nature for June, 1907. 4to.
New Church Magazine for July, 1907. 8vo.
Nuovo Cimento for May, 1907. 8vo.
Page's Weekly for June, 1907. 8vo.
Photographic News for June, 1907. 8vo.
Revue d'Electrochimie for June, 1907. 8vo.
Science Abstracts for June, 1907. 8vo.
Zoophilist for June, 1907. 8vo.
Electrical Engineers, Institution of—Journal, Vol. XXXVIII. No. 183. 8vo. 1907.
Field Museum of Natural History—Publications: Botanical, Vol. II. Nos. 4–5; Geological, Vol. III. No. 5; Zoological, Vol. VIII. 8vo. 1907.
Florence Biblioteca Nazionale—Bulletin for June, 1907. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. IV. Disp. 1. 8vo. 1907.
French Government—Lettres du Cardinal Mazarin. Tome IX. 4to. 1906.
Geographical Society, Royal—Journal, Vol. XXIX. No. 6, 1907. 8vo.
Geological Society—Abstracts of Proceedings, No. 847. 8vo. 1907.
Quarterly Journal, Vol. LXIII. Part 2. 8vo. 1907.
Geological Literature, 1906. 8vo. 1907.
Göttingen, Royal Academy of Sciences—Nachrichten, 1907: Mat. Phys. Klasse. Heft 1. 8vo.
Johns Hopkins University—American Journal of Philology, Vol. XXVIII. No. 2. 8vo. 1907.
Leicester Municipal Libraries—Thirty-sixth Annual Report, 1906–7. 8vo. 1907.
Linnean Society—Journal: Zoology, Vol. XXX. No. 195. 8vo. 1907.
Liverpool Literary and Philosophical Society—Proceedings, Vol. LIX. 8vo. 1906.
London County Council—Gazette for June, 1907. 4to.
Manchester Municipal School of Technology—Third Annual Report of the Godlee Observatory, 1906. 8vo. 1907.

Mexico Secretaria de Comunicaciones—Anales, Num. 15. 8vo. 1907.
Microscopical Society, Royal—Journal, 1907, Part 3. 8vo.
Midi, Société Archéologique—Bulletin, Nouvelle Série, No. 36. 8vo. 1906.
Munich, Royal Academy of Sciences—Sitzungsberichte, 1907. Heft 1. 8vo.
National Church League—Gazette for June-July, 1907. 8vo.
National Physical Laboratory—Collected Researches, Vol. II. 4to. 1907.
Report for the year 1906. 8vo. 1907.
Navy League—Journal for June, 1907. 8vo.
New York, Society for Experimental Biology—Proceedings, Vol. IV. Nos. 4–5. 8vo. 1907.
North of England Institute of Mining Engineers—Transactions, Vol. LVI. Part 4; Vol. LVII. Part 2–3. 8vo. 1907.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May, 1907. 4to.
Pharmaceutical Society of Great Britain—Journal for June, 1907. 8vo.
Photographic Society, Royal—Journal, Vol. XLVII. No. 6. 8vo. 1907.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LIX. Part 1. 8vo. 1907.
Reed, Thomas E., M.D. (the Author)—The Sex Cycle and the Germ Plasm. 8vo. 1907.
Royal Society of Edinburgh—Proceedings, Vol. XXVII. No. 2. 8vo. 1907.
Royal Society of London—Proceedings, Vol. LXXIX. A, No. 530; B, No. 532. 8vo. 1907.
Saxon Academy of Sciences, Royal—Abhandlungen: Phil. Hist. Klasse, Band XXIII. No. 3; Band XXV. Nos. 2, 4, 5. 4to. 1906–7.
Berichte: Mat. Phys. Klasse, 1906, Nos. 6–8; 1907, No. 1; Phil. Hist. Klasse, 1906, Nos. 3–5. 8vo. 1906–7.
S. Paulo Commissao Geographica—Boletin, No. 21. 8vo. 1907.
St. Petersburg Imperial Academy of Sciences—Bulletin, 1907, Nos. 10–11. 8vo
Sanitary Institute, Royal—Journal, Vol. XXVIII. No. 6. 8vo. 1907.
Scottish Society of Arts, Royal—Journal, Vol XVII. No. 10. 8vo. 1907.
Selborne Society—Nature Notes for June, 1907. 8vo.
Smith, B. Leigh, Esq., M.R.I.—The Scottish Geographical Magazine, Vol. XXIII. No. 6. 8vo. 1907.
Societa degli Spettroscopisti Italiani—Memorie, Vol. XXXVI. Disp. 5. 4to. 1907.
Society of Arts—Journal for June, 1907. 8vo.
Stockholm, Royal Swedish Academy of Sciences—Handlingar, Band XLI. No. 4; Band XLII. Nos. 2–4. 4to. 1906–7.
Arkiv för Matematik, Band III. Häfte 2. 8vo. 1907.
Les Prix Nobel in 1904. 8vo. 1907.
Toronto University—Studies: Chemical Series, Nos. 54–58, 60, 61, 63; Geological Series, No. 4; Pathological Series, No. 1; Physiological Series, No. 6. 8vo. 1906–7.
United Service Institution, Royal—Journal for June, 1907. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XVIII. No. 7. 8vo. 1906.
Bulletins: Use of Fruit as Food; Nutritive Value of Legumes. 8vo. 1907.
Monthly Weather Review for February, 1907. 4to.
United States Department of Commerce and Labour—Bulletin of the Bureau of Standards, Vol. III. No. 1. 8vo. 1907.
United States Patent Office—Gazette, Vol. CXXVII. Nos. 5–9; Vol. CXXVIII. Nos. 1–7. 4to. 1907.
Vienna, Imperial Geological Institute—Verhandlungen, 1907, Nos. 4–6. 8vo.
Washington, Library of Congress—Calendar of the Correspondence of George Washington. 4to. 1906.
Wellcome Chemical Research Laboratories—London Botanic Gardens. By P.E.F. Perrédès. 8vo. 1907.

Western Australia, Agent-General—Statistical Abstract for April, 1907. 4to.
Supplement to Government Gazette for April, 1907. 4to.
Western Society of Engineers—Journal, Vol. XII. No. 2. 8vo. 1907.
Yorkshire Philosophical Society—Annual Report for 1906. 8vo. 1907.
Zoological Society of London—Proceedings, 1907, pp. 1–236. 8vo. 1907.
Zurich, Naturforschenden Gesellschaft—Vierteljahrsschrift, Band XIX. Heft 1. 8vo. 1907.

MEDAL.

American Philosophical Society—Medal struck by order of the Congress of the United States to commemorate the Two Hundredth Anniversary of the Birth of Benjamin Franklin. 1906.

GENERAL MONTHLY MEETING,

Monday, November 4, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Robert John Collie, M.D.
Sir Murland Evans, Bart.
Harry Richardson, Esq. M.I.E.E.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to "A Member" for a Donation of £50 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary reported the decease of Sir William Perkin, LL.D. Ph.D. D.Sc. F.R.S. F.C.S., a Manager, on July 14, 1907, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, The Managers of the Royal Institution of Great Britain desire to record at this, their first Meeting subsequent to his death, their sense of the loss sustained by the Institution and by Chemical Science in the decease of Sir William Perkin.

Sir William Perkin became a Member of the Royal Institution in 1900, and he was elected a Manager in May 1907. He delivered a Friday Evening Discourse on the "Newest Colouring Matters" so long ago as May 14, 1869.

On the occasion of the International Celebration of the Coal Tar Colour Jubilee, which took place in the Lecture Room of the Royal Institution in July 1906, Sir William Perkin was presented with numerous addresses and medals by eminent chemists and representatives from Foreign Societies and Academies.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of their most sincere sympathy with Lady Perkin and the family in their bereavement.

The following Address to the Geological Society on the occasion of the Centenary celebration was presented by Professor Henry E. Armstrong, F.R.S., on behalf of the Members of the Royal Institution on September 26 last :—

THE ROYAL INSTITUTION OF GREAT BRITAIN desires to offer to THE GEOLOGICAL SOCIETY OF LONDON cordial congratulations on the occasion of the celebration of its Centenary.

The Members of the Royal Institution appreciate and honour the great work accomplished by the Geological Society, and revere the names of the many distinguished Fellows, who, in the past, by their investigations and writings, have rendered such signal services to the cause of Geological Science.

It is the earnest wish of the Royal Institution that the Geological Society may continue to prosper in the Century to come, and that Science be still further enriched by its valuable contributions to our knowledge of the material structure of our Globe.

(Signed) NORTHUMBERLAND,
President.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Kodaikanal Observatory Bulletin, Nos. 9-11. 4to. 1907.

First Report on Fruit Experiments at Pusa. 8vo. 1907.

Memoirs of Department of Agriculture, Chemical Series, Vol. I. Nos. 3-4; Entomological Series, Vol. I. Nos. 2-5; Botanical Series, Vol. I. No. 1, Part 2, No. 6. 8vo. 1907.

Report on Madras Government Museum and Connemara Library for 1906-7. 4to.

Geological Survey: Records, Vol. XXXV. Part 3. 8vo. 1907.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVI. 1° Semestre, Fasc. 12; 2° Semestre, Fasc. 1-7. 8vo. 1907.

Classe di Scienze Morali, Serie Quinta, Vol. XVI. Fasc. 4-5. 8vo. 1907.

Rendiconti, 1907, Vol. II. 4to.

Allegheny Observatory—Miscellaneous Scientific Papers, Nos. 18-20. 8vo. 1906-7.

American Academy of Arts and Sciences—Proceedings, Vol. XLII. Nos 27-29; Vol. XLIII. Nos. 1-3. 8vo 1907.

Memoirs, Vol. XIII. No. 5. 4to. 1907.

American Geographical Society—Bulletin, Vol. XXXIX. Nos. 7-9. 8vo. 1907.

American Philosophical Society—Proceedings, Vol. XLVI. Nos. 185-6. 8vo. 1907.

Amsterdam, Royal Academy of Sciences—Verhandelingen, 1e Sectie, Dl. IX. No. 4; 2e Sectie, Dl. XIV. Nos. 1-3. Zittengsverslagen, Vol. XV. Proceedings, Vol. IX. Jaarboek, 1906. 8vo. 1906-7.

Antiquaries, Society of—Archæologia, Vol. LX. Part 1. 4to. 1906.

Proceedings, Vol. XXI. No. 1. 8vo. 1906.

Aristotelian Society—Proceedings, N.S. Vol. VII. 1906-7. 8vo. 1907.

Asiatic Society, Royal—Journal for July-Oct. 1907. 8vo.

Asiatic Society of Bengal—Journal, Vol. II. No. 10; Vol. III. Nos. 1-4. 8vo. 1906-7.

Astronomer-Royal—Greenwich Observations, 1905. 4to. 1907.

Photoheliographic Results, 1905. 4to. 1907.

Cape Observatory Annals, Vol. XII. Part IV. 4to. 1907.

Astronomical Society, Royal—Monthly Notices, Vol. LXVII. No. 8. 8vo. 1907.

Memoirs, Appendix to Vol. LVII. 4to. 1906.

Automobile Club—Journal for July-Oct. 1907. 8vo.

Balkan States Exhibition, Executive of—Bulgaria of To-day. 8vo. 1907.

Official Catalogue of the Bulgarian Section. 8vo. 1907.

Bankers Institute—Journal, Vol. XXVIII. Parts 7-8. 8vo. 1907.

Basel, Naturforschenden Gesellschaft—Verhandlungen, Band XIX. Heft 2. 8vo. 1907.

Bauer, L. A., Esq. (the Author)—Reports on Terrestrial Magnetism. 8vo. 1906-7.

Belgium, Royal Academy of Sciences—Bulletin, 1907, Nos. 5-8. 8vo.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1907, Nos. 23-38. 8vo.

Birmingham Natural History Society—Linnæus, 1707-1778. By W. Hillhouse. 8vo. 1907.

Blakesley, Thomas H., Esq. (the Author)—Logarithmic Lazytongs and Lattice Works. (2 copies.) 1907.

Board of Trade—Report on Weights and Measures Act, 1905-6. 4to. 1907.

Borredon, Capitano G. (the Author)—Tempo e Spazio. 8vo. 1907.

Boston Public Library—Monthly Bulletin for July-Oct. 1907. 8vo.

Botanic Society, Royal—Record, Jan-June, 1907. 8vo.

Brazilian Legation—The United States of Brazil. 4to. 1907.
British Architects, Royal Institute of—Journal, Third Series, Vol. XIV. Nos. 17–20. 4to. 1907.
Kalendar, 1907–8. 8vo.
British Astronomical Association—Journal, Vol. XVII. Nos. 9–10. 8vo. 1907.
List of Members. 8vo. 1907.
Buenos Aires—Monthly Bulletin of Statistics, March–June, 1907. 4to.
Bulletin Démographique Argentino, No. 14. 4to. 1907.
Cambridge Observatory—Annual Report, 1906–7. 4to.
Cambridge Philosophical Society—Transactions, Vol. XX. Nos. 13–14. 4to. 1907.
Proceedings, Vol. XIV. Part 3. 8vo. 1907.
Cambridge University Library—Report of the Library Syndicate, 1906. 4to. 1907.
Canada, Royal Society of—Transactions, Second Series, Vol. XII. 8vo. 1906
Carey, A. E., Esq. (*the Author*)—The Protection of Sea Shores from Erosion. 8vo. 1907.
Carpmael, A., Esq., M.R.I.—Patents and Designs Act, 1907, with Index. 8vo. 1907.
Carnegie Institution—Contributions from Solar Observatory Nos. 16–19. 8vo. 1907.
Chemical Industry, Society of—Journal, Vol. XXVI. Nos. 12–20. 8vo. 1907.
Chemical Society—Journal for July–Oct. 1907. 8vo.
Proceedings, Vol. XXIII. No. 329. 8vo. 1907.
Civil Engineers, Institution of—List of Members, 1907. 8vo.
Proceedings, Vol. CLXVIII. 8vo. 1907.
Clerke, A. St. John, Esq., M.R.I.—Agnes M. Clerke and Ellen M. Clerke: An Appreciation. By Lady Huggins. 8vo. 1907.
Colonial Institute, Royal—Proceedings, Vol. XXXVIII. 8vo. 1907.
Cornwall, Royal Polytechnic Society—Seventy-fourth Annual Report. 8vo. 1907.
de Rustafjaell, R., Esq., F.R.G.S. (*the Author*)—Palæolithic Vessels of Egypt. 8vo. 1907.
East India Association—Journal, Vol. XL. N.S. Nos. 45–46. 8vo. 1907.
Editors—Aeronautical Journal for July–Oct. 1907. 8vo.
American Journal of Science for July–Oct. 1907. 8vo.
Analyst for July–Oct. 1907. 8vo.
Astrophysical Journal for June–Sept. 1907. 8vo.
Athenæum for July–Oct. 1907. 4to.
Author for July 1907. 8vo.
British Homœopathic Review for August–Oct. 1907. 8vo.
Chemical News for July–Oct. 1907. 4to.
Chemist and Druggist for July–Oct. 1907. 8vo.
Concrete for July–Oct. 1907. 8vo.
Dioptric Review for July–Oct. 1907. 8vo.
Dyer and Calico Printer for July–Oct. 1907. 4to.
Electrical Contractor for July–Oct. 1907. 8vo.
Electrical Engineer for July–Oct. 1907. 4to.
Electrical Engineering for July–Oct. 1907. 4to.
Electrical Industries for July–Oct. 1907. 4to.
Electrical Review for July–Oct. 1907. 4to.
Electrical Times for July–Oct. 1907. 4to.
Electricity for July–Oct. 1907. 8vo.
Engineer for July–Oct. 1907. fol.
Engineer-in-Charge for July–Nov. 1907. 8vo.
Engineering for July–Oct. 1907. fol.
Horological Journal for July–Oct. 1907. 8vo.
Journal of the British Dental Association for July–Oct. 1907. 8vo.
Journal of Physical Chemistry for May–Oct. 1907. 8vo.
Journal of State Medicine for July–Oct. 1907. 8vo.

Editors—continued.
Law Journal for July–Oct. 1907. 8vo.
London University Gazette for July–Oct. 1907. 4to.
Machinery Market for July–Oct. 1907. 8vo.
Model Engineer for July–Oct. 1907. 8vo.
Motor Car Journal for July–Oct. 1907. 8vo.
Musical Times for July-Oct. 1907. 8vo.
Nature for July–Oct. 1907. 4to.
New Church Magazine for August–Nov. 1907. 8vo.
Nuovo Cimento for June–Sept. 1907. 8vo.
Page's Weekly for July–Oct. 1907. 8vo.
Photographic News for July–Oct. 1907. 8vo.
Physical Review for June–Oct. 1907. 8vo.
Review of Internationalism for April and June, 1907. 8vo.
Revue d'Electrochimie for July–Sept. 1907. 8vo.
Science Abstracts for August–Oct. 1907. 8vo.
Science of Man for May, 1907. 8vo.
Terrestrial Magnetism for March–June, 1907. 8vo.
Zoophilist for August-Oct. 1907. 8vo.

Electrical Engineers, Institution of—Journal, Vol. XXXIX. Nos. 184–186. 8vo. 1907.
List of Fellows, 1907. 8vo.

Faraday Society—Transactions, Vol. III. Part 1. 8vo. 1907.

Florence, Biblioteca Nazionale—Bulletin for July–Oct. 1907. 8vo.

Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. IV. Disp. 2. 8vo. 1907.

Franklin Institute—Journal, Vol. CLXIV. Nos. 2–4. 8vo. 1907.

Geographical Society, Royal—Journal, Vol. XXX. Nos. 1–4. 8vo. 1907.

Geological Society—Abstracts of Proceedings, No. 848. 8vo. 1907.
Quarterly Journal, Vol. LXIII. Part 3. 8vo. 1907.

Göttingen Academy of Sciences—Nachrichten, 1907, Mat.-Phys. Klasse, Heft 2–3; Geschäftliche Mittheilungen, Heft 1. 8vo.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Série II. Tome XII. Liv. 3–4. 8vo. 1907.

Historical Manuscripts Commission—Report on American Manuscripts in the Royal Institution, Vol. III. 8vo. (2 copies.) Also 2 copies, 4to, unbound. 1907.

Humanitarian League—Facts about Flogging. By J. Collinson. 12mo. 1905.

Imperial Institute—Bulletin, Vol. V. No. 2. 8vo. 1907.

Iron and Steel Institute—Journal, Vol. LXXIII. No. 1; Vol. LXXIV. No. 2. 8vo. 1907.
List of Members, 1907. 8vo.

Johns Hopkins University, Baltimore—Studies: Series XXIV. Nos. 11–12; Series XXV. Nos. 1–5. 8vo. 1906–7.
Circulars, 1906, No. 10; 1907, Nos. 1–6. 8vo. 1906–7.

Kansas University—Science Bulletin, Vol. IV. Nos. 1–6. 8vo. 1907.

Kyōto Imperial University, College of Science—Memoirs, Vol. I. No. 3. 8vo. 1907.

Lehmann, Dr. O. (the Author)—Begriffe und Gesetze der Physik. 16mo. 1907.
Flüssige Krystalle und Mechanische Technologie. 4to. 1907.

Life-Boat Institution, Royal National—Journal for August–Nov. 1907. 8vo.

Linnean Society—Journal, Zoology, Vol. XXX. No. 196; Botany, Vol. XXXVIII. Nos. 263–4. 8vo. 1907.
Transactions, Second Series, Botany, Vol. VII. Parts 4–5; Zoology, Vol. IX. Part 11; Vol. X. Parts 6–7. 4to. 1906–7.

Literature, Royal Society of—Transactions, Vol. XXVII. Part 4. 8vo. 1907.

Liverpool University, Institute of Commercial Research in the Tropics—Quarterly Journal, Vol. II. No. 5. 8vo. 1907.
Commercial Possibilities of West Africa. 8vo. 1907.

London County Council—Gazette for July–Oct. 1907. 4to.
McClean, Mrs.—Frank McClean, 1837–1904. 8vo. 1907.
Madrid, Royal Academy of Sciences—Revista, Tom. V. Nos. 7–8. 8vo. 1907.
Manchester Literary and Philosophical Society—Proceedings, Vol. LI. Part 3. 8vo. 1907.
Massachusetts Institute of Technology—Technology Quarterly, Vol. XX. No. 2. 8vo. 1907.
Mechanical Engineers, Institution of—Proceedings, 1906, Parts 3–4. 8vo. 1907.
Merck, E., Esq.—Reports on Pharmaceutical Chemistry, Vol. XX. 8vo. 1907.
Mersey Conservancy—Report on the River Mersey, 1906. 8vo. 1907.
Meteorological Office—Second Annual Report of the Meteorological Committee. 8vo. 1907.
Meteorological Society, Royal—Quarterly Journal, Vol. XXXIII. No. 143. 8vo. 1907.
Record, Vol. XXVI. No. 103. 8vo. 1907.
Metropolitan Water Board—Fourth Annual Report. 8vo. 1907.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias, Tome XXII. Nos. 9–12; Tome XXIV. Nos. 1–9. 8vo. 1906–7.
Microscopical Society, Royal—Journal, 1907, Parts 4–5. 8vo.
Monaco, L'Institut Océanographique—Bulletin, Nos. 102–4. 8vo. 1907.
Munich, Academy of Sciences—Sitzungsberichte, 1907, Heft 2. 8vo.
Natal, Commissioner of Mines—Report on the Mining Industry for 1906. fol. 1907.
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Proceedings, 1907, pp. 237–746. 8vo.
Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, 1907, Heft 1–2. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 2, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Miss Joyce Burton Buckley,
Louis Gottschalk, Esq., A.M. Ph.D.
Mrs. Robert Home,
David George Johnston, M.D.
Sir Edwin Ray Lankester, K.C.B. M.A. D.Sc. LL.D. F.R.S.
Sidney Hugh Godolphin Osborne, Esq.
Miss Edith A. Stoney, M.A.

were elected Members of the Royal Institution.

Professor Louis Joseph Troost, Hon. F.C.S. (Paris).
Professor Albin Haller, D.Sc. (Paris).
Professor Walther Victor Spring, Ph.D. Hon. F.C.S. (Liège).

were elected Honorary Members of the Royal Institution.

The Special Thanks of the Members were returned to the Right Hon. Lord Sanderson, G.C.B. K.C.M.G., for his Donation of Five Guineas to the Fund for the Promotion of Experimental Research at Low Temperatures.

The following Lecture Arrangements were announced :—

SIR DAVID GILL, K.C.B. LL.D. D.Sc. F.R.S. *M.R.I.* Six Lectures (adapted to a Juvenile Auditory) on ASTRONOMY, OLD AND NEW. On Dec. 28 (*Saturday*), Dec. 31, 1907; Jan. 2, 4, 7, 9, 1908.

ALBERT A. GRAY, M.D. F.R.S.E. Two Lectures on THE INTERNAL EAR OF DIFFERENT ANIMALS. On *Tuesdays*, Jan. 14, 21.

PROFESSOR F. J. HAVERFIELD, M.A. LL.D. Two Lectures on ROMAN BRITAIN: (*a*) ITS FRONTIERS AND GARRISON, (*b*) ITS INTERIOR CIVILISATION. On *Tuesdays*, Jan. 28, Feb. 4.

PROFESSOR WILLIAM STIRLING, M.D. LL.D. D.Sc., Fullerian Professor of Physiology, *R.I.* Six Lectures on MEMBRANES, THEIR STRUCTURE, USES AND PRODUCTS. On *Tuesdays*, Feb. 11, 18, 25, March 3, 10, 17.

E. A. WALLIS BUDGE, Esq., M.A. Litt.D. Three Lectures on THE EGYPTIAN SUDAN; ITS HISTORY, MONUMENTS AND PEOPLES, PAST AND PRESENT. On *Tuesdays*, March 24, 31, April 7.

PROFESSOR W. W. WATTS, M.A. M.Sc. F.R.S. Two Lectures on 1. THE BUILDING OF BRITAIN; 2. RECENT LIGHT ON ANCIENT PHYSIOGRAPHIES. On *Thursdays*, Jan. 16, 23.

MAJOR MARTIN HUME. Three Lectures on THE STORY OF THE SPANISH ARMADA. On *Thursdays*, Jan. 30, Feb. 6, 13.

PROFESSOR WILLIAM SOMERVILLE, M.A. D.Sc. Two Lectures on WOOD, ITS BOTANICAL AND TECHNICAL ASPECTS. On *Thursdays*, Feb. 20, 27.

PROFESSOR SIR JOHN RHYS, M.A. D.Litt. Two Lectures on EARLY BRITISH HISTORY AND EPIGRAPHY. On *Thursdays*, March 5, 12.

RICHARD T. GLAZEBROOK, Esq., M.A. D.Sc. F.R.S. *M.R.I.* Two Lectures on STANDARDISATION IN VARIOUS ASPECTS: 1. MECHANICAL ENGINEERING; 2. ELECTRICAL ENGINEERING. On *Thursdays*, March 19, 26.

R. LYDEKKER, Esq., F.R.S. Two Lectures on 1. THE ANIMALS OF AFRICA; 2. THE ANIMALS OF SOUTH AMERICA. On *Thursdays*, April 2, 9.

PROFESSOR GISBERT KAPP, Dr. Eng. M. Inst. C.E. Two Lectures on THE ELECTRIFICATION OF RAILWAYS. On *Saturdays*, Jan. 18, 25.

LIONEL CUST. Esq., M.V.O. M.A. Two Lectures on ANTHONY VAN DYCK. On *Saturdays*, Feb. 1, 8.

SELWYN BRINTON, Esq., M.A. Three Lectures on THE ART OF FLORENCE. On *Saturdays*, Feb. 15, 22, 29.

PROFESSOR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.* Professor of Natural Philosophy, *R.I.* Six Lectures on ELECTRIC DISCHARGES THROUGH GASES. On *Saturdays*, March 7, 14, 21, 28, April 4, 11.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Fossil Plants, Part 2. 8vo. 1882.
Report on Canadian Rocks. 8vo. 1903.
Organic Remains, Decades 1, 3, 4. 8vo. 1858–9.
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Maps, 652–4. fol. 1900.
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Chemical Industry, Society of—Journal, Vol. XXVI. No. 21. 8vo. 1907.

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 Journal for November, 1907. 8vo.
 List of Fellows, 1907. 8vo.
Devonshire Association—Transactions, Vol. XXXIX. 8vo. 1907.
Editors—American Journal of Science for November, 1907. 8vo.
 Analyst for November, 1907. 8vo.
 Astrophysical Journal for October, 1907. 8vo.
 Athenæum for November, 1907. 4to.
 Author for Oct.–Nov. 1907. 8vo.
 British Homœopathic Review for November, 1907. 8vo.
 Chemical News for November, 1907. 4to.
 Chemist and Druggist for November, 1907. 8vo.
 Dioptric Review for November, 1907. 8vo.
 Dyer and Calico Printer for November, 1907. 4to.
 Electrical Contractor for November, 1907. 8vo.
 Electrical Engineer for November, 1907. 4to.
 Electrical Engineering for November, 1907. 4to.
 Electrical Review for November, 1907. 4to.
 Electrical Times for November, 1907. 4to.
 Electricity for November, 1907. 8vo.
 Engineer for November, 1907. fol.
 Engineering for November, 1907. fol.
 Horological Journal for November, 1907. 8vo.
 Journal of the British Dental Association for November, 1907. 8vo.
 Journal of State Medicine for November, 1907. 8vo.
 Law Journal for November, 1907. 4to.
 London University Gazette for November, 1907. 4to.
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 Motor Car Journal for November, 1907. 8vo.
 Musical Times for November, 1907. 8vo.
 Nature for November, 1907. 4to.
 New Church Magazine for December, 1907. 8vo.
 Nuovo Cimento for October, 1907. 8vo.
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 Photographic News for November, 1907. 8vo.
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Rennes, Université de—Travaux Scientifiques, Tome V. and V^2. 8vo. 1906.
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Royal Society of Edinburgh—Proceedings, Vol. XXVII. No. 5. 8vo. 1907.
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Saxon Academy of Sciences, Royal—Abhandlungen: Mat. Phys. Klasse, Band XXX. Nos. 1–3; Phil. Hist. Klasse, Band XXIII. No. 4, Band XXV. No. 3, Band XXVI. No. 1. 4to. 1907.
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The Times—The History of the Book War. 12mo. 1907.
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Monthly Weather Review for June–August, 1907. 4to.
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Geological Survey:
Bulletin, Nos. 304, 311, 313, 317, 318, 320, 323, 324. 8vo. 1907.
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Western Australia, Agent-General—Geological Survey: Bulletin, No. 26. 8vo. 1907.
Wisconsin Academy—Transactions, Vol. XV. Part 1. 8vo. 1905.
Ziegler, William C., The Estate of—The Ziegler Polar Expedition, 1903–5: Scientific Results. 4to. 1907.

WEEKLY EVENING MEETING,

Friday, May 31, 1907.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

A. HENRY SAVAGE LANDOR, ESQ., *M.R.I.*

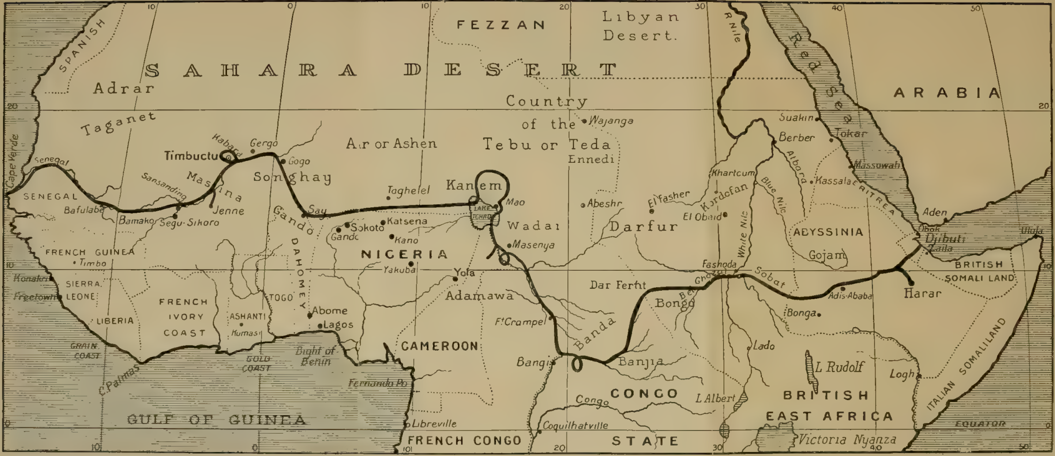
Across Widest Africa.

MR. A. HENRY SAVAGE LANDOR said:

I crossed Africa from east to west in its widest part. The journey was accomplished in 364 days, including halts. Immense detours and zig-zags were described during the journey. The distance travelled over by my expedition was not less than 8500 English statute miles.

Starting from Djibuti in French Somaliland, Abyssinia was crossed in a south-westerly direction as far as the Baro river. The tribes north and south of the river Sobat, a continuation of the Baro, were visited on the way. I did not travel by water, but rode mules and horses until I arrived at the Nile at a place some seventy miles south of Fashoda. From there my journey was continued across the Bahr-el-Ghazal, then through the forest down to the Mbomu and the Ubanghi rivers in the French Congo. The Congo Free State was also visited.

At a point where the river Ubanghi turns sharply to the south, I proceeded with a number of carriers in a north-westerly direction towards lake Tchad. I crossed lake Tchad, but instead of continuing towards the west, I made a great detour coming back towards the east, as I wished to inspect the formation of some of the depressions and basins in the desert of the Kanem. Roaming about the desert, I had an opportunity of visiting some of the tribes north of lake Tchad. Having crossed what is called the Chitati country, I returned southward until the north-eastern part of lake Tchad was reached. Approximately some thirteen hundred kilometres (about 850 miles) on camels across the desert brought me to the Niger, via the market of Zinder. Another thousand kilometres (about 680 miles) by canoe, took me to the sacred city of Timbuctu; from there the journey up the Niger was continued. Then I crossed over to the Senegal river until Cape Verde, the most westerly point of Africa, was reached. With the exception of a flying machine, I availed myself of every possible means of transportation in order to get on. Horses, mules, donkeys, oxen, camels, human carriers, canoes, steel boats, rafts, were used, and naturally, on a long journey of that kind, I lost many animals and some men. At no time of my journey did I possess more



Equatorial Africa, showing the Author's Route.

than thirty pack animals and never more than some forty men. I had hardly gone one-third of the way when I found myself abandoned by everybody, and, in the most difficult part of the journey, in the heart of Africa, everybody had disappeared except one faithful Somali. The Somali and I managed to take the entire caravan across the forest during the season of the heavy rains, a labour requiring some patience.

Going from lake Tchad across the country of the warlike Tuareg, a new caravan I had formed was reduced to about half-a-dozen camels and three men. Two out of these three men became insane. As you know, in tropical climates a continuous march of ten miles a day is considered fair marching. On this particular journey an average of twenty-three miles were daily covered. With a great many changes of animals the caravan was constantly kept moving. The longest halts between Adis-Ababa and Cape Verde, were twelve days on the Nile and ten days in Timbuctu.

We were the greater part of the time in unhealthy regions, where malarial fever is rampant. In the Senegal, yellow fever was bad at the time of my visit. On nearing the coast, having taken advantage of the short but most excellent line of railway in the French Possession, I was delayed by being placed in quarantine, a French officer having been attacked by yellow fever in my own carriage. No medicines to speak of were carried on the journey, no filters for the water. We did not worry about mosquito bites, nor did we adopt any of the precautions suggested by the medical profession. The result was that I returned to Europe in excellent health. No sun-helmet, no patent clothes for explorers, no patented boots were worn, but just the ordinary attire of London was used, the head-gear being a mere straw hat. During the entire journey I carried no fire-arms upon me, nor weapons of any kind, not even a pen-knife. My men, of course, were armed with small calibre repeating rifles, but I seldom gave them any cartridges. There were, of course, no other white men with me, and I bore the entire cost of the expedition.

Now that you have roughly seen where I went, we will begin again from the beginning, as I will endeavour to point out to you one or two out of the myriads of things which seemed interesting to me.

The journey across Abyssinia presented no difficulty, barring the likelihood of accident on the shaky little railway from Djibuti to Dire Dawa, a distance of about one-hundred and ninety miles. The actual marching began from Dire Dawa, but I first made an excursion to the city of Harrar, where I had the pleasure of a long audience with Ras-Makonnen, one of the finest men Abyssinia then possessed. On my return to Dire Dawa I made up a fresh caravan, and started almost immediately for the Abyssinian capital by the semi-desert route of Assabot.

Several tribes of Danakils were visited on this journey. The

Danakils are nice people, but they are occasionally given to killing strangers and mutilating their bodies. They had killed two Arabs and an Abyssinian only a few days before I passed through their country. The Abyssinian soldiers of my escort were terrified when we got near these fellows. Twelve days quick marching took me to Adis-Ababa, where I was hospitably received by Sir John Harrington, our Minister there, and by the few foreigners residing in the place. The Emperor Menelik was extremely kind to me, and received me on several occasions. Adis-Ababa was more like a huge camp than a city. Menelik's palace (or rather series of palaces) had the appearance of factory buildings ; it certainly did not look like an Imperial home. Enclosed within the Imperial walls were the mint, and sheds for traction engines. All sorts of workshops were constructed round the palace, and in these workshops rather than upon his throne Menelik spent most of his time—as he disliked being bothered with politics.

I intended saying a few words about Sir John Harrington, the British Minister in Adis-Ababa, and his work. Perhaps you are aware that the remarkable personal influence of this man has been able to save our prestige in Abyssinia at a moment when we had practically lost every atom of power in that country, and we were ready to let that region slip out of our hands. To-day, thanks to the immense respect which our Minister commands in Abyssinia, we have little to fear in political competition with other nations. In fact, anyone who has travelled in Abyssinia will tell you that there are two men in the country who command absolute reverence and fear : one is Emperor Menelik, the other Sir John Harrington. His good, honest advice to the Abyssinians is much appreciated by Menelik, and I think that many of the beneficial reforms that have been made in that country have been due to the good advice that our Minister has given the Emperor.

I have no time to enter into the intricate political side of the Abyssinian question. There is little doubt that Abyssinia owes its present independence mainly to the jealousy of the envious European Powers surrounding her. Nevertheless, Abyssinia has now reached a stage when serious development of her capabilities is expected of her, or else division of her land must follow among her grasping European friends. With the French and the English on the Somali coast, the Italians in the Danakil country, and the Anglo-Egyptians in the Soudan pressing her from every side, it is not possible for Abyssinia to remain in her present semi-barbarous condition. Menelik's power is so great that it carries everything before it. His word is law, and is everywhere obeyed in a manner quite amazing to Europeans. Menelik is a kind of god to the Abyssinians themselves, and if not exactly worshipped by subjected non-Abyssinian chiefs in the country, like the Galla and others, they have nevertheless a wholesome fear of him. The Abyssinians owe, I think, their constant

victories in colonial wars rather to their fame than to their present fighting qualities, or their skill and courage. The conquered and neighbouring tribes are in positive terror of the Abyssinians. But with the death of Menelik matters will change, and perhaps it will not be so easy to hold the country together. I am not a prophet, but there is no foretelling what all these European *ententes cordiales* may do for many an African potentate. One cannot help admiring Emperor Menelik personally. He possesses an abnormal amount of sound sense ; he is as just and fair to his countrymen as is possible to an Emperor ; he is generous enough with what he possesses, and tries hard to do all that is right and proper. Perhaps were Menelik a younger man, and were he persuaded to take a journey to Europe, a great many sensible reforms—and possibly some not quite so sensible—would certainly follow. All Abyssinia needs is to be established on a sound basis for natives and foreigners alike, and above all the establishment of a proper government and administration, with some stability of laws.

The journey from Adis-Ababa to the Baro and then the Sobat river presented no difficulty. One or two rivers had a good deal of water, but we had no trouble in crossing them.

In Western Abyssinia I found the Galla people extremely interesting. They were to my mind the only important race of people in Menelik's empire who were worth anything. First of all they were not Christians but Mussulmans. They detested the Abyssinians. At Menelik's death I am sure they would only be too happy to be under British rule. They were great shepherds and cultivators of the land, and had a fair idea of trading. They were peaceful enough, and, for a nation of tropical Africa, they seemed to possess sound sense.

The western portion of the Abyssinian plateau seemed by far the richest of Menelik's possessions. Owing to the elevation of the plateau the climate was semi-temperate. The local agricultural resources could be improved to no mean extent. Wild coffee of delicious quality was plentiful, and also rubber. The mineral wealth of the country offered fair prospects, but perhaps it is problematic whether the difficulty and expense of transporting machinery, and other greater risks, make it worth while under present conditions to exploit it. Gold was washed in small quantities in the Baro river.

As you came down very abruptly from the Abyssinian plateau, you found yourself in a lower zone owned by the Abyssinians, but partly under the management of Anglo-Egyptian officials. The result of this was that nobody seemed to have any authority in that portion of the country, and the natives took advantage of it.

At the foot of the escarpment there lived the Yambo—a race of people of a stature a good deal above the normal. Some, like the chief and his brother, were regular giants ; but they were all tall.

Gambela, the first Anglo-Egyptian post on Abyssinian territory under an Egyptian officer, was a deadly spot for men and beasts.

There were only a few Greek traders at the time of my visit, all more or less in a pitiable condition owing to malarial fever of a violent kind. A cross, made of two pieces of a kerosene box nailed upon a stick, marked the grave of General Gatacre, who died of fever in a tragic manner on the river Sobat a short time before I passed through. Thorns thrown over the grave prevented hyenas from eating the body.

During the rainy season small steam-boats can come up the Nile and the Sobat as far as Gambela. I was there during the dry season, and, as I wished to see some of the tribes north and south of the Sobat, I took my entire caravan overland in great zig-zags, and not by river, as far as the Nile. My men and animals suffered considerably from the intense heat along the dreary flat mud country.

A great tribe of long-legged people is to be found south of the Sobat: the Nuers, a strange, suspicious, unreliable lot, possessing thousands of cattle which they will on no account sell or barter. Their oxen, hardly less civil than their masters, took special delight in charging my caravan whenever they had a chance. The Nuer men paint their bodies white, whereas Nuer ladies retain the original colour of the simplest costume supplied by nature. These people are notable for the abnormal length of their legs—quite characteristic of most Nilotic tribes. Nuer men often plaster the hair into a long point; it is done with a preparation of mud and other ingredients, which have the property of dyeing the hair red.

Anthropological and ethnological studies were made on the interesting tribes on both sides of the Sobat river. A long description of the customs and manners, as well as numerous photographs of types, will be found in the two volumes describing this journey, viz. "Across Widest Africa." * The anatomical structure of these people would certainly lead one to believe that they were specially built by nature to live in marshy regions. They were tall, long-legged, like most members of other Nilotic tribes. Not unlike the Shiluk whom we shall meet further on the journey, they possessed the habits of water birds. One frequently perceived them standing on one leg, not unlike flamingoes along the river banks.

The habits of the Nuers were extraordinary. Anatomically all the races of the Nile valley, and along tributaries of the Nile, were of great interest.

I crossed and re-crossed the Sobat several times with my entire caravan in order to visit tribes which interested me. The river was broad and swift, and we employed much energy, time and patience to convey baggage and mules across. The mules were compelled to swim across the river. Shots were fired during the time the animals were in the water, in order to scare away crocodiles. Many were the incidents of our long and rapid march across that wretched land

* Across Widest Africa, by A. Henry Savage Landor (Hurst and Blackett).

until we reached the Nile at Taufikia, one of the dreariest spots on earth. A garrison of Sudanese soldiers was stationed there.

On getting near the Nile one finds the Shiluk, a tribe formerly much more numerous and powerful than now. The incursions of the Dervishes, the Egyptians, and slave-merchants, have played havoc among them. Their influence must at one time have been considerable, at least if we are to judge by their language, which, with certain variations, is understood and spoken by many distant tribes towards the east as far as the foot of the Abyssinian plateau, and, I am told, also as far south as the Victoria Nyanza. They divide themselves into two great families: the Quagnaret and the O-chiolla. The O-chiolla recognise their fraternity with the Nuer but principally with the Denca.

The nucleus of the Shiluk population is upon the north bank of the Nile, between lake No and the mouth of the river Sobat. Also small settlements, some few miles up the Sobat.

The land of the Shiluk is a vast plain, smothered in grass, and cut up into myriads of channels and depressions which get filled with water during the rising of the Nile. Wood for construction there is practically none, if the *Dum* palm—very scarce—is excepted, of which one sees one or two here and there near Shiluk villages. The *Higlig* or *Balanites ægyptiaca* and the *Deleb* and *Dum* are about the only fructiferous palms in the Shiluk country. The flowers on land are few and ugly, but not so upon the water and along the river banks, where lotuses and white lilies are to be seen. During the rainy season the climate is unhealthy.

The Quagnaret and the O-chiolla occasionally intermarry. It is seldom that a Shiluk can allow himself more than one wife, as women in the Shiluk country are an expensive luxury. The near relatives of the girl expect in exchange of her value no less than three or four oxen or cows and at least forty sheep or goats, besides sundry articles. Then, when a Shiluk goes to bargain for a wife with her relatives, he has to bring an extra half-dozen goats and sheep to present, one to the father of the girl, the others to the assembled members of the family. By means of pieces of straw laid upon the floor to facilitate counting, the number of oxen, cows and goats to be paid is agreed upon, while the ladies of the house produce vessel after vessel of *merissa* (an intoxicating beverage) in order to cheer all present. The business part of the transaction being over, if the girl consents she is presented with a bracelet of brass or ivory, which is passed over her wrist. Fellows have been known to take a wife on credit, but these rash individuals heavily mortgage their happiness, and even run the risk of losing their better half should a wealthier purchaser present himself on the scene prepared to pay hard cash. Among the O-chiolla we find the habit so common in Central Africa of removing four front teeth of the lower jaw, but the Quagnaret do not indulge in this practice.

It is not easy to imagine a more dreary, uninteresting, unpleasant country than the Bahr-el-Ghazal. I happened to be marching from Meshra-el-rek towards Wau during the hottest and dryest month of the year, just before the rainy season. My animals were on several occasions unable to obtain water from the wells along the trail.

The Denca, a powerful race next to the Shiluk, to whom they are closely allied, derive their name, I think, from the Shiluk legend of the two brothers: Guacango and Dengo, who, according to the Quagnaret, were the first of their race to appear in the land. Dengo having quarrelled with his brother, crossed the Nile with his cattle on to the right bank, where he settled. Evidently, the Quagnaret originated from Guacango, and the Dinka from Dengo, the word Dinka having been suosequently modified into Denca.

Having marched across the Bahr-el-Ghazal as far as Dem Zeibir, I proceeded to cross the tropical forest in an almost southerly direction, but described some considerable detours as no trail existed. The Kresh, and many other interesting tribes, were encountered near the western boundary of the Bahr-el-Ghazal.

You are all well acquainted with the dwarfs of the forest generally called the Niam-niam. As you know, Niam-niam is not what they really call themselves, but merely a disparaging name applied to them by others. They call themselves A-sandeh, *sandeh* meaning "under."

A number of these people came under my observation. Malformed anatomically, ill-proportioned, with big paunches, elongated and slanting skulls, they were absolutely devoid of any intelligence, and timid and treacherous to a degree.

Curious and indisputably well-defined peculiarities in their language lead one to believe that these people have degenerated from a higher standard of mental ability. However, these peculiarities might also be accidental. Whether accidental or not, we find in the A-sandeh tongue examples of deep philosophy which are not to be found in more complete languages, such as Italian, French, German, Spanish, Portuguese, or English. For instance, in order to explain that some inanimate object belongs to him, such as a hut (*Kuorau*), a spear (*Basso*) . . . the A-sandeh would use the pronoun corresponding to "my" in English. "*My* hut, *my* spear," etc., but in describing a part of himself, or talking of people of his own blood, he will never say "*My* father, *my* mother, *my* eye, *my* leg, *my* hand," but will say: "*I* father, *I* mother, *I* leg, *I* hand," etc., to denote that people of his blood and flesh, as well as any part of his own anatomy, are more than mere possessions. They form part of himself. This is generally done by the suffixes "*sse*" or "*re*" or "*mi*" after the noun. My father, *ba-mi*; my friend, *badia-re*; my eye, *bengli-sse*.

We do not find the same accurate philosophy in many other A-sandeh expressions, although some descriptiveness is generally

noticeable in many of their words, borrowed from meteorological phenomena or from the botanical world. Beard, for instance, *mainguengoua*, is nothing less when translated literally than "rain from the chin." The hand, *ppé'be*, is the "leaf of the arm" (*ppé*, leaf; *be*, arm). A finger nail, *sissi ouil insaga* (*sissi*, bark; *ouil insaga*, finger), means literally the "bark of one's finger." The foot, *ppé'ndoue'*, is the "leaf of the leg." Perhaps the most remarkable of all is the word, *de'goude*; meaning girl, but which translated literally means: *de*, woman; *goude*, boy—or, a "woman boy." They are almost as immoderate as we are in speaking of their sensations, nothing short of death being sufficient to describe love or drunkenness. *Kpi na gnamou*, "to die of love"; *kpi na boda*, "to die of beer." Astronomy is perhaps not the strongest point of the A-sandeh. The stars in their language, *care'courou*, are the "enemies of the sun" (*care'*, enemy). Numerals are counted, as usual with almost all African tribes, with the aid of their fingers up to five: *ssa* 1, *ioué* 2, *bia'ta* 3, *biama* 4, *bissoué* 5. Six, *bati ssa*, being "give one from the other hand"; seven, *bati ioue*, "give two from the other hand," and so on. The fingers of the hands being exhausted, the feet come to the assistance. Therefore eleven is *ba'ti sande'yo ssa* (or "give one from the ground," meaning the foot.) Sixteen is, *cobain ssa*, or "one from the other side" (the other foot). Beyond twenty the fingers and toes of neighbours are required: forty-one being two men and one finger; sixty-two, three men and two fingers, etc.

My object in crossing the forest was to reach the Mbomu river in the land of one of the great Sultans of Central Africa, the Sultanate of Zemio. This part of the journey was fatiguing, as all my men except the faithful Somali had abandoned me, and we two had a good deal of trouble to convey the caravan through the forest. The rainy season had by now come in full force, and added to the discomfort. My animals were dying fast, and those few that remained had to carry the heavy loads of the animals lost. The heat was stifling, the exertion great. We had to pack the loads upon the animals dozens of times a day, as in forcing our way through the forest the loads were constantly getting undone and tumbling off. We were marching all the time on swampy ground, with torrents of rain drenching everything, and vines and thorns and entangled roots of trees twisting and catching and wounding our feet. The poor Somali and I contracted fever. After one month of strain, we arrived at Zemio upon the Mbomu river, having succeeded in bringing over all the baggage. I cannot speak too highly of the faithfulness of the poor Somali who accompanied me. When we arrived, my original caravan of some thirty animals was reduced to three or four donkeys. Having rested two days I again became in good health, and was able to proceed westwards.

There are a great number of tribes, mostly of cannibals, along the Mbomu and Ubanghi rivers. I paid visits to the big Sultans

of Rafay and Bongasso. These big sultanates in the very heart of Africa were exploited by a French company, at the head of which, was locally Mr. Charles Pierre, an intelligent and business-like man to whom the Society owes to-day its enormous development and flourishing condition.

The river Mbomu and the river Welle join at a place called Yacoma, and form the river Ubanghi. The Mbomu and the Ubanghi define the boundary between the French Congo and the Congo Free State. There are innumerable tribes of cannibals on both sides of the river, much too numerous to be even mentioned in this paper.

I will not enter into the controversy regarding the administration of the Congo Free State. Of the portion of the Belgian Congo which I visited all I can say is that the country is kept in excellent order; that the natives seem quite happy and well cared for; and the country, far from being damaged, is greatly improved by the construction of roads, by enormous plantations of rubber, as well as immense plantations of rice, millet, Indian corn, etc., which the natives prefer to receive in payment for the rubber instead of money which is useless to them. Many of the statements popular in this country are grossly exaggerated, if not altogether unfounded.

The Italian officers employed by the Congo Free State, have done remarkable work in that country. I ever found them loved for their kindly treatment towards the natives. I admired the sensible and moderate way in which they administered justice and ameliorated the agricultural resources of the districts under their jurisdiction.

I had occasion in several places to cross or to go along the route followed by Colonel Marchand on his expedition to Fashoda. Much abuse has been showered on that officer, both in this country and in France; but it must not be forgotten that, besides the adventurous side of the expedition—a feat of remarkable pluck and endurance —Colonel Marchand and his magnificent officers have left a work of great scientific value in the shape of a large map of marvellous precision of the entire country traversed by them. I had occasion to check some of the observations taken by them at different points of the journey, and I have always found them accurate.

With about forty carriers I left the Ubanghi on the fifth parallel of latitude north, and marched towards lake Tchad. It was an interesting journey, done at a time of the year during the heavy rains, when marching was not always a pleasure and crossing the swollen streams not easy. We had dealings with many different tribes. Their manners and customs were peculiar.

In the basin of the Shari river I came across many interesting tribes. Several of the tribes, like the Mandjia and the Sanga, have adopted peculiar ways of decorating, or rather ornamenting, their lips. The women insert a crystal, wooden or bone cone into the lower lip, or else they elongate the lips to an extraordinary extent by inserting

a large disc, as big as a saucer, into the upper lip and occasionally even in both lips. The northern tribes on the Shari go in for this fashion of elongating the lips in a more exaggerated manner than the Mandjia. Quite a number of photographs of these interesting types will be found in "Across Widest Africa."

I made an incursion into the German Cameroon, where I was hospitably received by German officers. The natives were kept in excellent order, and the country was gradually and sensibly being improved.

There are a great many interesting questions to investigate regarding lake Tchad. They say that lake Tchad is drying up fast, and is likely to disappear altogether. This is not exactly the case. Naturally, like all lakes which depend on their supply of water from streams with no outlet into the sea; in climates where the evaporation is rapid and where the absorption of the soil is considerable; in a region where the rainy seasons are not alike two years running, it follows as a matter of course that when the rivers flowing into and forming the lake do not carry the same volume of water, the level of the lake cannot always be the same. For several years lake Tchad had actually dried up in the northern portion, so much so that a French officer, Lieutenant Freydenberg, was able to walk on foot from the northern dune right into the centre of the lake, further exploring still on foot the north-eastern part of lake Tchad as far as Kulua.

The two principal rivers bringing water into the Tchad are the Shari river, coming from the south-east, and the Komadugu, coming from the west, the latter flowing into the northern part of the Tchad. The Shari brings the largest volume of water.

As you are well aware, the growth of reeds and grass and all sorts of water plants in the swampy parts of the Tchad is enormous. This thick vegetation decays and settles at the bottom of the lake. The winds of the desert bring over a great quantity of sand, which settles down at the bottom of the lake, with the decayed vegetable matter, and on the top of it. Therefore the bed of the lake is constantly, and fairly rapidly, raised. In the central portion of the lake we find a regular barrage of grass and mud, forming a number of small islets hardly above water. This barrage extends from east to west of the lake, but south of it we find two large pockets of clear water, which are always kept fairly well filled by the normal supply of water brought by the Shari river. Again, in the north-westerly part of the lake, north of the barrage, we have another pocket of clear water from three to six feet deep, the water of this pocket being supplied by the Komadugu. In the northern part of this pocket we find banks of grass, some islets in course of formation and others already formed and definitely emerged. This region of islets is contained almost entirely north of a second barrage of grass and mud of a similar formation, and almost parallel to the larger one we have found stretching across the lake. In the south-west corner of the lake we

find a great swamp of reeds and mud, so much so that the Germans, who own a portion of the lake's coast, are quite unable to get to the water. There are a great many islands in the most easterly part of the lake.

Possibly the name Tchad has come from the word Tchuku, the name given to the lake by the Buduma. The name Shari is a corruption, I think, of the Bornu word, Djari, meaning "great river." The Buduma call it Ndjeri, which is merely the Bornu word badly pronounced.

In the year 1906, the Shari brought so much water that the entire lake has been filled up again. This happens every few years; according to the natives, every few years a fairly ample flood occurs, and after longer periods a great flood. It is nevertheless undoubted that the lake must have been centuries ago of a greater size than now. Lieutenant Freydenberg dug up a well at Kulua, now some distance north-east of lake Tchad, and found an excellent proof that what I have said above is correct. In digging he came to a deep layer of sand, under which he found a comparatively small layer of decayed vegetation; under that he again found another layer of sand, then another layer of decayed vegetation; then again another layer of sand, above another layer of decayed vegetation—which proves how the bed of the lake is being gradually raised.

In this paper I cannot go into the problem of supplementary lakes formed by infiltration from lake Tchad, nor can I go into the controversy of whether the Bahr-el-Ghazal is a river flowing into lake Tchad, or whether lake Tchad flows into the Bahr-el-Ghazal. Personally, I have no doubt whatever on what actually happens, and in the full account of my journey, "Across Widest Africa," I have endeavoured to explain what takes place.

I have not sufficient space to describe my journey east and north of lake Tchad; nor the long journey across the desert from the Tchad to the Niger and Timbuctu at a time after the rebellion, when there was unrest among the local tribes. I had by then only a modest caravan. Although anxiety was entertained by French officers for my safety, I got through the desert quickly and well.

I went up the Niger in a steel canoe, taking some twenty-eight days to reach Timbuctu. My canoe was indeed but seldom on the Niger itself. In fact, to be strictly accurate, we navigated over the submerged banks of the river instead of the actual river channel itself. Inundations covered a great part of the country at the time of my visit, and in order to avoid the strong current and find sufficiently shallow water to allow my men to punt, we travelled over the inundated country most of the time among thick grass, reeds, *borgu*, and across innumerable paddy fields. Heavenly places for mosquitoes at night. Every now and then we came to rocky hill masses, where the water was forced through a narrow passage from one huge reservoir to the lower one, and then we were compelled to come back to he

river channel. In these places we generally found rapids to negotiate, sometimes quite troublesome owing to the number of scattered rocks and the violent current. Punting and paddling were impossible, and I had to land my men—about a dozen—with a long tow-rope, in order to pull the canoe up the steps from the lowest to the highest point of the rapid.

The rapids at Dirawami gave us a good deal of work, and we had an accident. There was a high step to get over, with a violent rush of water flowing over it. My men had as usual landed, and were pulling their hardest, while I, alone in the boat, did the steering with a long paddle. We had got the canoe nearly one-third over, and then she stuck at an angle of about thirty degrees, but with every prospect of describing a still wider arc of a circle in mid-air with the prow of the boat. The canoe was full of heavy baggage, which unfortunately slid in confusion from aft to stern, giving a bad list. The canoe swung violently, and was caught sideways by the chute of water. It was washed away with great force, dragging into the stream most of the men who were pulling the tow-rope. Some of them narrowly escaped getting drowned. The canoe flowed down sideways at a good speed for some distance, when it collided against two rocks and became filled with water. As she was about to sink we just managed to pull her on the shore, and the baggage was saved.

Numerous stone implements, silex arrow-points and knife-blades, axes and hammers, are to be found in the northern part of the Niger, and, if one could spare the time, important archæological discoveries could be expected in that region. Regular camps of these former stone-workers are to be found, and curious legends are related by the natives regarding the stone implements.

Let us come to Timbuctu. Timbuctu the mysterious, let me tell you at once, has no mystery left at all. The town is built on two sides of a dune running east to west, and on the side of a second dune parallel to the first and north of this. The population of Timbuctu consists of about five thousand inhabitants with a fixed residence, and a floating population of about four thousand people, mostly traders, from Tripolitania, Morocco, from Ghadannon, Tenduf, Tadjakant, Tuat, etc. The two principal elements in the population are the Songõy and the Arma. Another class—but not a separate race—are the Alfa (or learned men). They form an influential class in Timbuctu, the most learned centre of Mussulman science in Western Africa.

Timbuctu being mainly a city of transit, many tongues are spoken there, such as Songõy, Tamatchek (or the Tuareg language), Malinke, Bambara, Pular (the language of the Fulbé), and Arabic. Songõy is, however, the language of the country, and is understood as far as Agades east of Timbuctu; as far south as Djenne and Say; or in other words, over the entire extent of the ancient Empire of Askia.

The Tuareg of the desert are now well kept in hand by the French. The word Tuareg comes from the Arabic *Tarik* (plural *Tuareg*), meaning "the God-forsaken people." The Tuareg call themselves *Tarogi*, the feminine of which is *Tarogia*; but more commonly they go by the name of each large division of tribe, such as Imohag, Imotchak, Imagarin. They mostly speak Tamatchek. The Tuareg can be divided into four great divisions, two of these divisions inhabiting the basin of the Niger, the other two the mountainous regions of the central Saharian plateau. Then in the desert were the Sinussis, a troublesome lot of fanatical Mohammedans, living east of the Tuareg country.

There were two principal religious confraternities, or two great families of Marabu, who held under their religious dependency nearly the entire population of the Sahara. The Tedjadjina, the oldest of these confraternities, was based on the True Light of Islam, and was principally created to unite all the people of the Sahara. The second division, the Sinussi, was organised after the French conquest of Algeria, in order to fight against the ever increasing European influence over Mussulman states, and to preserve the people of the Sahara and Central Africa from European contamination. Fanaticism of the most exalted kind was preached by the latter. These people were kept in check near their frontier by a camel corps under the command of Captain Mangin, a man most remarkable for his pluck.

We find in the Upper Niger Valley that the important river courses have their birth upon soil of primary formation, and flow between "walls of sandstone," often in cascades, forcing a passage between rocks and along tortuous channels. When at the last stage of this natural stairway, they form immense valleys. The mountainous country constitutes a forest zone rich enough in rubber (the *Landolphia Heudelotii*), *karité*, fairly valuable woods, and a variety of spices. In mineral wealth, we have iron, gold, and lime. The mineral and forestry resources of the mountainous regions are not to be compared with those which might be brought about by the agricultural development, and the breeding of cattle and sheep, between the forestry zone and the semi-desert zone in the northern part of the French Colony. From Kangala to Sansanding the Niger flows in a well traced bed, as much as 1000 yards wide in certain places. At high water the river floods the surrounding country for a distance, seldom more than half a mile. The level of low and high water in this portion of the Niger varies from fifty to ninety centimetres. The rainy season begins towards the end of May and ends in October. During the year 1906, however, the last rains were registered on the 2nd of November. A N.N.E. dry wind, the "harnattan," blows during November, causing a distinct depression in the temperature. The soil in that region is formed mostly of sand and clay in suitable proportions, but has no great

depth, and for agricultural purposes is not calcareous enough—a common fault of many a tropical soil. In some districts we find a clay mud fairly fertile but not sufficiently porous, in fact quite waterproof and difficult to work. These are the regions more often inundated during part of the rainy season, and generally used by the natives for the cultivation of rice. As far as Nyamina the river valley is narrow; further it widens considerably, and what the French so well define as "*affleurements grèseux*" altogether disappear. From this point the Niger flows across a country fairly well populated and fertile.

Between Sansanding and Diafarabé the Niger divides itself into many arms, and receives on its left the tributary Bani. These arms converge towards a great basin, the Debo lake, at the entrance of which are found four islets of sandstone. They are the spurs of the mountain mass of Bandiagara. The aspect of this region changes with the seasons. During low water one finds a succession of plains, on which can be seen grazing numerous flocks of sheep, goats, and herds of jebu oxen. The French are now beginning the exportation of wool, with some success.

The principal arms of the Niger and the Bani traversing the above mentioned plain are navigable all the year round for small boats and barges. During flood time the plain is transformed into a huge green lake—green because of the immense quantity of "borgu" (the *panicum*), a wild water-plant of great utility in those regions. The borgu is a forage plant containing much nourishment, being rich in sugar. Then we have in this flooded region immense paddy-fields between a regular network of channels. Millet is grown upon dry land.

Lake Debo has two outlets, with ramifications which join later. Here, too, we are in a country of yearly inundations. Further north the high water reaches small mountain masses without a well-defined connection, and in flood time fills a series of great natural reservoirs at different elevations, from which the water is gradually drained by the actual river bed. Between Bamba and Gao is the very narrow *défilé* of Tossaye.

In the region of Timbuctu the water rarely rises more than thirty centimetres during the rainy season. When dry, the natives utilise for agricultural purposes the bed of the lake and the various marshes alluvially enriched by the inundation. Rice, big millet and corn are raised. Rice is cultivated during the ascending period of the flood, whereas corn and millet are grown during the decreasing period.

In what the French call "la boucle du Niger," the curve or elbow of the Niger, north of the fifteenth degree of latitude, the agricultural resources cannot attain any serious development until the irrigation of the country has been managed in such a way as to obtain a more methodical irrigation from the vast reservoirs. That portion of the country is good, I think, for breeding purposes, the grazing

being of excellent quality for oxen and sheep. Perhaps the agricultural capacity of production of the high Senegal and Niger is somewhat handicapped by the scantiness of the population.

It was pleasing to find at Kulikoro an agricultural station, started by the French Government in 1902, under the able direction of Monsieur Jean Vuillet, a practical, enthusiastic and hardworking gentleman, whose careful study of the agricultural resources of the country should certainly bring about valuable results in the future development of that interesting French colony. There is in the station a farming school, some model villages, and a botanical garden where innumerable experiments are made with indigenous and imported plants. The efforts of the agricultural station have been directed towards the improvement of the local cotton-growing industry—in which I personally believe the country may have a future —and to the study of parasitic diseases, the creation of hybrids, and experimenting on the effects produced by climatic conditions upon local and foreign plants and their hybrids. Experiments on locally grown American cotton, for instance, from Upland and Louisana, have shown that it is possible to produce on the Niger cottons answering the exacting requirements of French weaving looms. But the American cottons have drawbacks in those countries, as they are very sensitive to parasitic and climatic influences, and they need soil of unusual fertility. The fibre obtained is somewhat short, and lacking in regularity. Experiments made in crossing American with local cotton have not been successful from an economical point of view, but a careful selection of indigenous cotton with improved methods of cultivation have given most satisfactory and hopeful results. Monsieur Vuillet told me that some two thousand tons of cotton were grown upon the Niger in 1905, out of which one hundred tons only were of American cotton.

Arachide (pea-nuts), sesame, rice and tobacco, will also some day be crops of importance in those regions. Then we have the karité, or butter plant, and the rubber plant, locally known as "gohine" (technically *Landolphia Heudelotii*). The commercial possibilities of the karité are not yet fully known, but I think will be considerable, as the karité tree is exceedingly common in the high Senegal and Niger. karité butter is in many ways superior to margarine. French chemists are, I understand, busy experimenting on the most suitable methods of extracting butter from the karité. Butter extracted from the karité nut has been on the market for some time in France, and has a good sale. The Landolphia rubber-vine is chiefly abundant up to the eleventh degree of latitude north, fairly common as far as the twelfth degree, and scarce further north. Beyond 13° 4′ or 5′ lat. N., the vine is not found at all. The principal markets for the rubber are Sikasso, Bamako, Bouguni, and Bobo-Dioulasso. The exportation of the rubber, which practically only began in 1899, is now one of the most important of local trades. Over 750,000 kilos, or about

1,500,000 pounds in weight, were exported in 1906. The French government has wisely established several schools where practical demonstrations are given to the natives, in order to teach them how to extract the *latex* without injury to the plants, and fresh plantations are being constantly made with considerable success in appropriate districts. The indigenous *Landolphia Heudelotii* is of course the most suited quality for the locality, but experiments with such excellent latex-giving plants as the *Ficus elastica* have been successful. The *Agave rigida* var. *sisalana* (*sisal*), and the *Furcroya*, also imported, produce in those climates textile fibres much sought after in commerce, and seem exceptionally remarkable for their vigorous growth. The *Bagana* (Bambara name) a variety of *Acacia arabica*, more correctly the *Acacia adamsonii*, may also prove a profitable plant for tanning purposes.

Experiments are constantly made at the Kulikoro station with all kinds of fruit trees and seeds, which are sent over from the Jardin de Nogent in France. Undoubtedly we shall hear in the future that the country has much benefited by the devoted work and intelligence of the practical and self-sacrificing scientists whom France has sent to study the agricultural possibilities of the Niger and the high Senegal.

My trans-continental journey ended on the last rock of Cape Verde, the most westerly point of the African continent.

I cannot leave the French Central African Colonies without saying a word about French officers and officials. Wherever I met them their hospitability had no bounds, and it would be difficult to imagine more noble-minded and good-hearted fellows than these men of the French colonial infantry, who go and spend the best years of their life in solitude in Central Africa. It is seldom that you meet more than one officer with a few black Senegalese soldiers at any of their military posts; and the posts are generally several hundred miles apart.

The training of the French officers was marvellously up-to-date. They could turn their hand almost to anything, from surveying to building houses and fortresses, making irrigation works, administering justice, doctoring the sick, teaching the natives all sorts of useful things, and drilling their Senegalese soldiers in a practical manner. It is amazing what the French officers have been able to accomplish with these magnificent fellows—perhaps the best black soldiers of all the central zone of tropical Africa.

In the way of colonial wars, we all know what the French have done in Africa, and what they are still doing with few officers and not many men. I happen to know their work on the Wadaian frontier, in the Bornu and in the Tuareg country, and it is only when one takes the trouble to find what these men have done—*and with what little resources they have done it*—that one remains filled with admiration for the great work they have accomplished.

In the civil administration, too, the French are sending out able men, such as Governor Ponty of the high Senegal, Governor Guy at St. Louis, Monsieur Roume at Dakar, not to speak of Monsieur Gentil (the Governor of the Congo), the man who was able to defeat "Rabah the Terrible," and who destroyed for ever the fanatical bands of that once powerful chief.

From east to west of Africa, whenever I came across foreigners, few and far apart—whether they were officials or traders—whether they were French, German, English, or Belgian—I only met with the most unbounded politeness from everybody. I owe special thanks to Lord Cromer and the Sirdar, the French and Belgian Ministers in Cairo, as well as to Captain Owen, the Sirdar's agent, for much civility shown me. All these men, I can assure you, have my deepest gratitude for rendering my journey across Africa one of the most delightful I have ever taken.

[A. H. S. L.]

WEEKLY EVENING MEETING,

Friday, June 7, 1907.

THE RIGHT HON. LORD KELVIN, O.M. G.C.V.O. P.C. D.C.L. LL.D. D.Sc. F.R.S., in the Chair.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, Fullerian Professor of Chemistry, R.I.

Studies in High Vacua and Helium at Low Temperatures.

IN a former lecture the production of very high vacua by means of charcoal absorption at the temperatures of liquid air and liquid hydrogen was discussed. With this new means of research, we shall now follow its application in various directions. We live in an age characterised by volume and rapidity of publication, in which scientific literature takes its place, so that science is now burdened with much that in former days would have been confided to the waste-paper basket. There is a type of the modern scientist who needs to be continually before the public, and the result is the appearance of immature communications often loaded with needlessly endless details. The foundations of scientific research in our time would seem to be not a little undermined; the tendency being to regard quantity and not quality of output. In these pioneer studies we shall be content to describe the general lines of this investigation, omitting for the present any reference to refinement of details.

High vacua and helium might not appear to have much to do with each other, but, as we shall see, they are intimately connected. Immediately after the liquefaction of hydrogen by regenerative expansion in 1896, I attacked the problem of the liquefaction of helium, following by strict thermo-dynamic analogy the process that had succeeded with hydrogen.* The hydrogen process was as follows: hydrogen at a pressure of 180 atmospheres cooled down to $-205°$ C., was made to issue, at the rate of about 15 cubic feet per minute, from a nozzle terminating a long spiral coil of copper pipe placed in a silvered glass vacuum vessel having a spiral tube connected with the interior. After five minutes' circulation liquid hydrogen began to drop from the end of the spiral tube, and when the liquid was evaporated under exhaustion, it froze into a white frothy mass of solid hydrogen, presenting an appearance quite different from the common belief that in this condition it would in all probability

* For full description see Presidential Address, British Association, 1902.

possess metallic lustre. The density of liquid hydrogen at its boiling point ($-252^{\circ}\cdot5$ C. or $20^{\circ}\cdot5$ Ab.), was found to be about $0\cdot07$, so that liquid hydrogen was about six times lighter than the lightest liquid hitherto known, namely, marsh gas, whose density is $0\cdot4$. It is sixteen times lighter than an equal volume of liquid oxygen, or in other words the two bodies show a greater difference in density than water and mercury. The boiling points and densities are as follows :—

Gas.	Boiling Point.	Liquid Density at the Boiling Point.
Oxygen . . .	$-182^{\circ}\cdot5$ C.	1·13
Nitrogen . . .	$-195^{\circ}\cdot6$ C.	0·80
Hydrogen . . .	$-252^{\circ}\cdot3$ C.	0·07

In my experiments with helium in 1901, it was expanded adiabatically, using the Cailletet method, from a pressure of 100 atmospheres at the temperature of solid hydrogen down to one atmosphere, without showing any temporary mist during expansion, from which it was inferred that the gas had been cooled to at least 9° and still no liquefaction had occurred. Similar experiments were made by Olszewski in 1905, and he has expressed the view that helium may be practically a permanent gas which we should hardly ever succeed in liquefying. Without offering any opinion other than what I have already expressed as to the probability of helium being liquefiable, let us study the application of the method to helium which was found successful with hydrogen. This method, first used technically by Linde, consisted in the use of a regenerative circuit along with expansion through a fine nozzle (pin-hole).

A pipe fixed in the King's Well at Bath enabled the escaping gas to be collected and sent to London. This gas consists almost entirely of nitrogen, but contains about one two-thousandth part of its volume of helium—in other words, 2000 cubic feet of the gas would contain 1 cubic foot of helium. The helium was concentrated from the crude gas by partially liquefying out the nitrogen, marsh gas and other impurities until it contained only about 30 per cent. of nitrogen along with helium and neon. In this condition it was put through the regenerative circuit under a pressure of about 100 atmospheres, yielding a quantity of liquid nitrogen, which was removed. The nitrogen remaining in the helium mixture had now fallen to 5 per cent., and on continuing to circulate the regenerator tubes got plugged with solid. To the helium gas, which was now too small a quantity to circulate well, was added 25 per cent. of hydrogen, and this mixture, on circulating, froze also in the regenerator tubes. A further addition of hydrogen, up to 50 per cent., was made to the gas, which after passing through the regenerating circuit, gave a

considerable amount of solid on the external coil, and finally the internal tubes plugged. During the evaporation of the solid deposited on the coil in the fourth circulation, the composition of the gases given off in three successive portions were as follows :—

—	First	Second	Third
Hydrogen . . .	41·0	37·0	34·8
Nitrogen . . .	5·2	16·4	48·0
Helium and Neon .	53·8	46·6	17·2
	100·0	100·0	100·0

The gas analysis suggests that the solid was composed of a mixture of solid hydrogen and nitrogen along with dissolved helium and neon.

The gaseous portion remaining after the last circulation, on being freed from hydrogen, consisted of 94 per cent. of helium and 6 per cent. of neon. To this helium and neon 75 per cent. of hydrogen was added, which, on passing round the regenerative circuit, yielded liquid hydrogen containing 13 per cent. of helium in solution, along with some solid presumably neon in the bottom of the vacuum vessel. The final helium left uncondensed in the circuit after the removal of the hydrogen contained 4 per cent. of neon.

Now how could the hydrogen be frozen during the circulation of the helium and hydrogen mixture? An experiment made on a former Friday evening with nitrogen will explain. On that occasion hydrogen was allowed to bubble up through a quantity of liquid nitrogen. The bubbles, in accordance with Dalton's law, being free of nitrogen induce a rapid evaporation of the liquid nitrogen into them as they pass through it, thus causing cooling, until the liquid nitrogen becomes first viscous and finally freezes into a jelly containing spiral tubes through which the hydrogen escapes. In the same way the passage of the uncondensed helium through or over liquefied hydrogen formed by spray on the regenerator coils lowers its temperature and finally causes it to freeze.

Such regenerative operations were carried on under considerable difficulties as it was impossible to see properly what was taking place in the apparatus, and at any moment the vacuum vessels might collapse. On one occasion, the whole of the helium that had been accumulated during 2 years was lost owing to the collapse of the glass vacuum vessel containing the regenerator coil, and the experiment of accumulating helium had to begin *de novo*.

The use of charcoal provides us with a ready means of studying the properties of gas mixtures containing helium. A sparking-tube, having a branch charcoal bulb attached, was charged with Bath-gas.

On immersing the charcoal bulb in liquid air, the nitrogen was rapidly absorbed and the discharge finally was that due to helium and neon. Now the helium spectrum contains two very prominent lines, one in the yellow and one in the green, and at very low pressures the green predominates. In another tube, the initial pressure was considerably reduced, and the green coloration was very strikingly displayed.

The intense red colour given out by neon vacuum tubes is now well known. On immersing one end of such a neon tube in liquid hydrogen, the gas was immediately differentiated, the more condensable and heavier neon sinking to the lower end of the tube, where it revealed itself by its orange-ruddy glow, while the upper end of the tube retained the yellow colour of the helium discharge, thus demonstrating that the gas in the tube was a mixture of helium and neon.

Velocity of Absorption of Air by Charcoal at -185° C. under Small Pressures.

For this purpose a long horizontal glass tube, A, Plate I., over an inch in diameter having either platinum electrodes sealed in at the ends or external tin foil electrodes, had a charcoal condenser immersed in liquid air attached, B, together with a means, after the vacuum was so high that no discharge would pass, of allowing a definite small volume of dry air to enter between two stop-cocks C. On opening the stop-cock D to the charcoal condenser, rapid exhaustion took place, which was measured at definite periods, after shutting off the charcoal, by the McLeod gauge E, which had no india-rubber joints or tubes used in its construction.

In the course of observations in high vacua it was found that metallic electrodes are unreliable because they occlude gas. For this reason, therefore, it was necessary to work with external tin-foil electrodes.

The rate of exhaustion may be gathered from the fact that 20 grammes of charcoal cooled in liquid air was able to reduce the pres-

Time of Exhaustion.	Pressure in mm.	Time of Exhaustion.	Pressure in mm.
0 sec.	2·190	60 sec.	0·347
10 ,,	1·271	2 min.	0·153
20 ,,	0·869	5 ,,	0·0274
30 ,,	0·632	10 ,,	0·00205
40 ,,	0·543	19 ,,	0·00025
50 ,,	0·435		

sure of air in the tube of 2000 c.c. capacity from one three-hundred-and-fiftieth of an atmosphere to one three-millionth of an atmosphere

PLATE I.

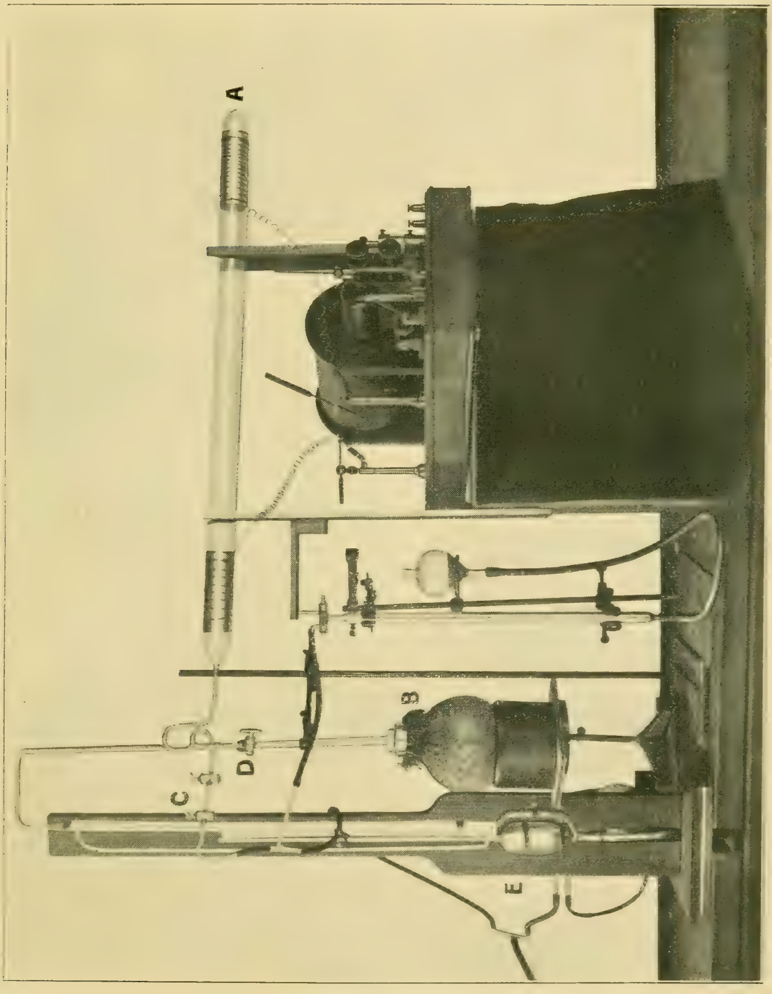
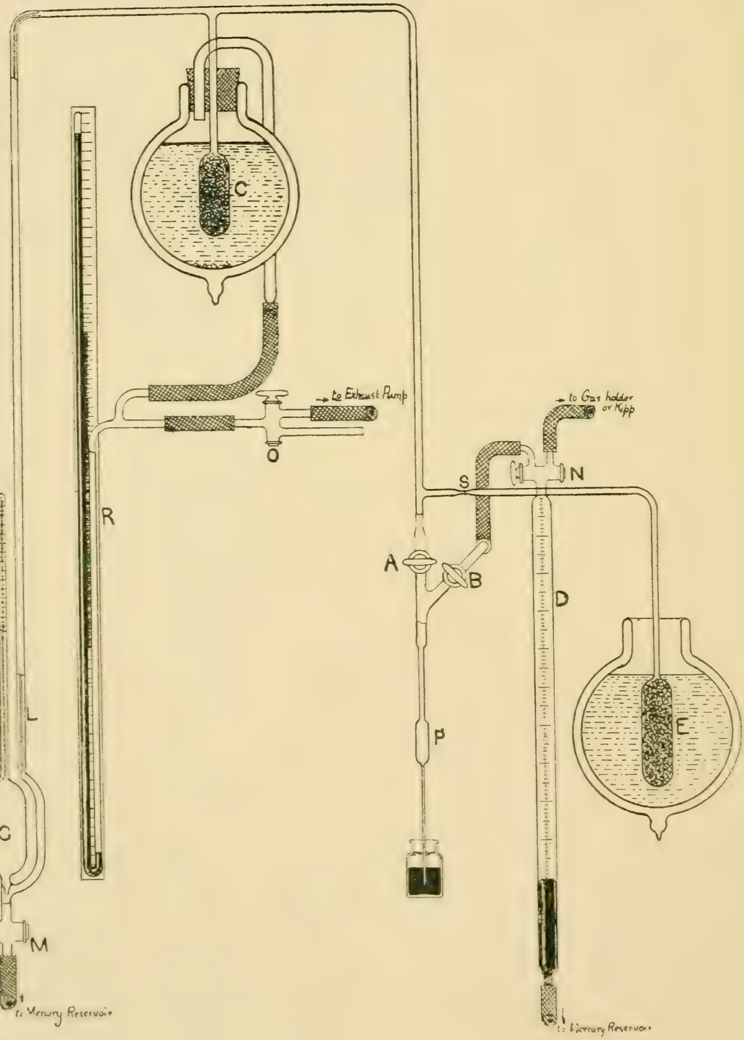


PLATE II.



C
to Exhaust Pump
O
R
to Gas holder or Kipp
N
S
A
B
D
K
L
P
E
C
M
to Mercury Reservoir
to Mercury Reservoir

in 19 minutes. The annexed table gives the observations. The law connecting pressure and time in this particular apparatus seems to be given by a formula of the type

$$\log\left(\frac{a}{t}\right)\log\left(\frac{b}{p}\right) = c$$

where t and p are time and pressure, and a b c are constants.

Charcoal Occlusion Pressures of Hydrogen and Nitrogen.

The apparatus sketched in Plate II., illustrates how the gas concentration, pressure and temperature are measured. The mass of charcoal E, immersed in liquid air, is used for the preliminary exhaustion of the McLeod gauge G, and the charcoal in C to be used in the experiments, and is then sealed off at S. Afterwards the bulb C is placed in a large spherical vacuum flask containing liquid oxygen which can be made to boil at any definite temperature under diminished pressure measured on the manometer R. The volume of gas admitted into the charcoal is measured by the burette D and pipette P and the corresponding occlusion pressure at any concentration and temperature below that of 90° abs., by the gauge G.

For small concentrations the relation between the pressure and the concentration of different gases in presence of charcoal, shows very great variation, all being at the same temperature. The following table gives the comparison between hydrogen and nitrogen at the temperature of liquid air, 25 grammes of charcoal being used.

Volume of Gas Absorbed.	Occlusion Hydrogen Pressure.	Occlusion Nitrogen Pressure.
c.c.	mm.	mm.
0	0·00003	0·00005
5	0·0228	..
10	0·0455	..
15	0·0645	..
20	0·0861	..
25	0·1105	..
30	0·1339	0·00031
35	0·1623	..
40	0·1870	..
130	..	0·00110
500	..	0·00314
1000	..	0·01756
1500	..	0·02920
2500	..	0·06172

Hence we see that 15 c.c. of hydrogen produced nearly the same pressure (0·0645 mm.) as 2500 c.c. of nitrogen (its pressure being

0·06172 mm.). This shows how different the occlusion volatility of hydrogen is at the temperature of liquid air as compared with that of nitrogen for equal concentration. In a corresponding manner the concentrations, for the same pressure, vary greatly with the temperature. The following table exemplifies this, even although the pressures are not quite constant.

Gas.	Concentration in c.c. per grm. of Charcoal.	Pressure in mm.	Temperature Absolute.
Helium . . .	97	2·2	20°
Hydrogen . . .	397	2·2	20°
Hydrogen . . .	15	2·1	90°
Nitrogen . . .	250	1·6	90°
Oxygen . . .	300	1·0	90°
Carbon dioxide . .	90	3·6	195°

The temperatures employed were the boiling-points of hydrogen, oxygen, and carbon dioxide.

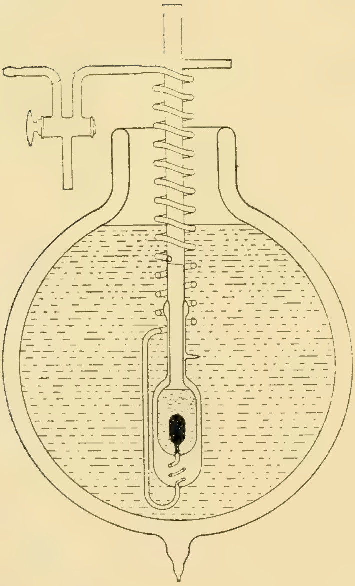
The Heat of Occlusion for Different Gases in Charcoal.

Accompanying the condensation of all gases to the liquid state there is evolution of heat, and we know that during the absorption of a gas in charcoal, or any other occluding body; as for instance, the occlusion of hydrogen in palladium; the amount generated exceeds that of direct liquefaction. From the relation between occlusion pressure and temperature at the same concentration, the reaction being reversible, we are able to calculate this heat-evolution. Thus, if the concentration in the charcoal for each of the following gases has the values given in the table, then the following mean molecular latent heats of occlusion result from my experiments.

Gas.	Concentration c.c. per grm.	Molecular Latent Heat.	Mean abs. Temperature.
	c.c.		°
Helium . . .	97	483·0	18
Hydrogen . . .	390	524·4	18
Hydrogen . . .	20	2005·6	78
Nitrogen . . .	250	3059·0	82
Oxygen . . .	300	3146·4	82
Carbon dioxide . .	90	6099·6	180

The concentrations of the occluded gases were so regulated as to start with an initial pressure not exceeding 3 mm. at the respective boiling-points of hydrogen, nitrogen, oxygen, or carbonic acid.

PLATE III.



A liquid air calorimeter such as I have described in former lectures* was modified so as to make a direct determination possible of the heat evolved during charcoal absorption at low temperatures. The apparatus will easily be understood from the drawing (Plate III.). The value of hydrogen absorption per molecule found by this method was 1940 calories, which is of the same order of magnitude as that which resulted from the occlusion pressure observations taken a little below the boiling point of liquid air.

Radiometer Studies.

No instrument is more convenient for the demonstration of the high vacuum produced by cooled charcoal than the radiometer of Sir William Crookes. A convenient arrangement of the attached charcoal tube is shown in Plate IV., Fig. 3. In order to wash out the radiometers, it was found that a bulb containing perchlorate of potash was the most reliable source of pure oxygen, and when the gases from minerals have to be examined a side tube must be added. The general arrangement is shown in Plate IV., Fig. 2, where A is the perchlorate bulb and E the side tube.

Helium Radiometer.

A Crookes radiometer, filled with helium, having a glass tube ending in a bulb containing charcoal, remained inactive to the concentrated beam of the electric arc after the charcoal was cooled in liquid air, but on replacing the liquid air by liquid hydrogen the radiometer vanes began to spin with great rapidity.

On further reducing the temperature by exhausting the hydrogen till it froze, the rotation seemed to be but little altered. This final drop in temperature to 14° absolute without much change in the motion indicated that there was still a considerable gas pressure left, from which we infer, by analogy with other gases, that the freezing point of hydrogen is still very much higher than the boiling-point of helium.

Hydrogen Radiometer.

A Crookes radiometer was filled with hydrogen instead of helium. When the charcoal bulb attached to this radiometer was immersed in liquid air, and the beam from the electric arc was focused on the vanes, rotation took place. This corresponded with what happened when the charcoal bulb of the helium radiometer was immersed in liquid or solid hydrogen ; in both cases the cooling had rarefied the gases sufficiently to permit motion. But, on allowing the vanes of the hydrogen radiometer to come to rest, and immersing its charcoal

* Roy. Inst. Proc., 1894, vol. xiv. p. 398, and 1904, vol. xvii. p. 581.

bulb in liquid hydrogen for only half a minute, the rarefaction became so great that, when the arc light was thrown on, the vanes remained perfectly still. From a comparison of these two experiments we may again derive an indication of the boiling-point of helium. For, in the case of the hydrogen radiometer, a fall of 75 per cent. in the temperature of the charcoal bulb, from the boiling-point of air to the boiling-point of hydrogen, reduced the vanes to rest; similarly, we may infer that a fall of like amount from the boiling-point of hydrogen would reduce the vanes of the helium radiometer to rest, a result which would make the boiling-point of helium about 5° or 6° absolute, as before.

A variation of the hydrogen radiometer experiment may be made as follows. Into the bulb of the radiometer a branch tube was sealed containing a little metallic sodium. On immersing the charcoal bulb in liquid hydrogen, and throwing on the electric beam, the vanes remained at rest as before; but on gently heating the sodium, a minute quantity of hydrogen was liberated which was sufficient to re-start the radiometer for a short period of time. In a few minutes, however, the absorption of hydrogen by the charcoal became so efficient that the radiometer stopped.

Ordinary Air Radiometer.

A radiometer, with attached charcoal bulb, was repeatedly washed out with the oxygen and nitrogen vapour coming from old liquid air, and sealed off at a pressure of a fraction of about a millimetre of mercury. On immersion of the charcoal tube in liquid air, the motion of the vanes did not cease, but, on immersion in liquid hydrogen, a minute or two sufficed to bring the motion completely to a stop. The explanation of the experiment is that old liquid air is not a mixture of oxygen with a small proportion of nitrogen and argon, but it always contains traces of neon and helium.

Oxygen and Nitrogen Radiometer.

The effect of the residual gases contained in air can be shown as follows: The bulb of a Crookes radiometer was thoroughly washed out with a mixture of pure chemical oxygen, care being taken that no hydrogen, neon or helium was present. This bulb was not provided with a side charcoal tube, but had a long quill tube attached, a few inches of which could be cooled in liquid hydrogen. The radiometer, thus charged with pure oxygen, had a portion of the quill tube cooled in liquid hydrogen. The motion entirely ceased through the condensation and solidification of the gas at 20° Abs. When air was used instead of the chemically prepared oxygen, the same radiometer was not stopped owing to the presence of helium and neon in the atmosphere.

PLATE IV.

Fig. 1.

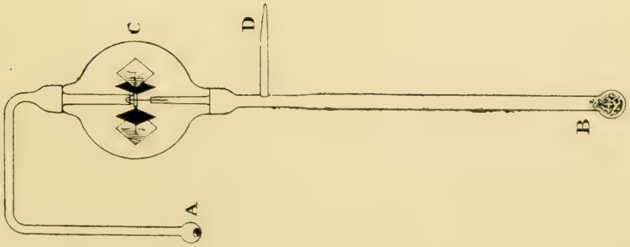


Fig. 2.

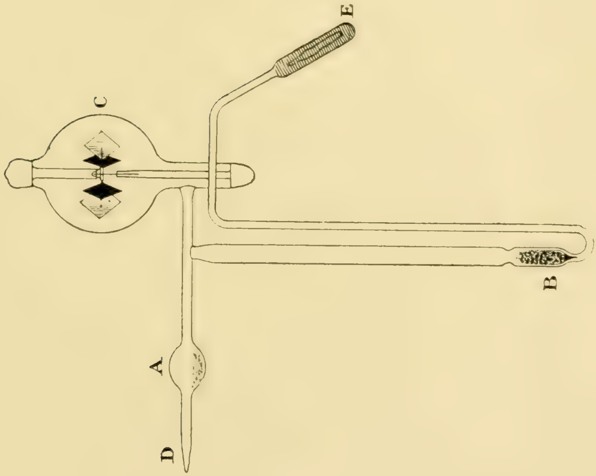
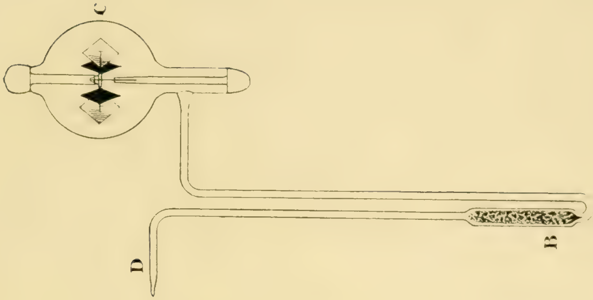


Fig. 3.



Thorianite.

The rare substance, thorianite, when heated gives off helium. Advantage of this was taken in the following experiment: A Crookes radiometer, with the usual tube of charcoal attached, was provided with an additional tube containing a small crystal of thorianite. On immersing the charcoal bulb in liquid hydrogen and throwing on the electric beam, no motion took place. The thorianite was, in these circumstances, heated by the flame of a Bunsen burner, and immediately supplied enough helium to set the vanes in motion, and the instrument remained active in spite of prolonged cooling of the charcoal with liquid hydrogen.

Electric Discharge Radiometer.

All the previous radiometers used were of the ordinary Crookes pattern in which the mica vanes were blackened on one side. In the experiment about to be described, a radiometer, with the usual charcoal tube attached, was employed; but it differed from the ordinary radiometer in that the one side of each vane was covered with a thin sheet of aluminium. The metallic frame bearing the vanes was connected to one of the terminals of an induction coil, and the other to a pole sealed through the glass of the radiometer. The bulb, therefore, was a kind of discharge-tube containing a light mill. On turning on the current, the vanes being made the negative pole, the bulb lit up with a fine luminescence, and began to rotate rapidly. But on immersing the charcoal bulb in liquid air, the vacuum was greatly intensified, the glow became much diminished, and the rotation of the vanes ceased altogether; even the additional stimulus of the beam from the electric arc was insufficient to produce any motion.

Experiment with Mercury Vapour to Measure Pressure in a Radiometer.

During the experiments on high vacua it became abundantly evident that the pressures reached were difficult to determine by means of the McLeod gauge. Minute quantities of helium were to be found everywhere—in the atmosphere, in the fine films of gases condensed on the surfaces of glass vessels, on vanes and elsewhere. It became, therefore, of importance to determine very small pressures by other means, if possible. The radiometer experiments suggested such a means, namely, by determining pressures below which the radiometer would not spin. The pressures of mercury vapour have been very accurately determined throughout a wide range of temperature. The following experiment shows how such measures can be used to ascertain the limit of pressure referred to above. A Crookes

radiometer (Plate IV., Fig. 1), with its attached charcoal bulb B, had sealed on to it a tube ending in a small bulb A containing a globule of mercury. The radiometer and charcoal bulb had previously been heated, exhausted, and repeatedly washed out with pure oxygen gas, and the mercury allowed to distil for some time into the charcoal cooled in liquid air. On exposing the radiometer to the electric beam the vanes began to spin. On cooling the mercury bulb in liquid air, the radiometer soon became inactive; but on replacing the liquid air by ordinary water, as the temperature rose, the mercury began to evaporate and the radiometer resumed its activity. It was found that the particular radiometer used became active when the temperature of the mercury had risen to $-23°$ C., which corresponded to a pressure of about a fifty-millionth of an atmosphere. As an example of the limits to which charcoal exhaustion extends, the following table gives the pressures obtained by the use of 5 grammes of charcoal attached to a bulb of 300 c.c. capacity containing air at an initial pressure of about 1·7 mm. and at the temperature of 15° C.

Time of Exhaustion.	Pressures in mm.
0 minutes in liquid air	1·6845
5 ,, ,,	0·0545
10 ,, ,,	0·01032
30 ,, ,,	0·000139
60 ,, ,,	0·000047
10 ,, ,, hydrogen	0·0000154
10 ,, solid ,,	0·0000058

Thus from an initial pressure of $\frac{1}{450}$ of an atmosphere, the pressure could be reduced to one 130-millionth of an atmosphere.

[J.D.]

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